Refutable Anthropology and Falsified Science

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Is anthropology a science? To put the question today amounts to a reply in the negative. The representatives of the 'true' sciences are not alone in suggesting a conjunctural or crippling lacuna which would preclude membership by right of the prestigious world, which, however, the name 'humanistic sciences' seems to demand. We should remember that some years ago Claude Lévi-Strauss caused a shudder to run through his discipline by describing it as a 'flattering imposture'.[1] Since then denigration has spread constantly, and it no longer consists of deploring a slowness, an equivocation or a handicap which we would have the hope of remedying: it is definitely a fatality which we are from now on invited to confirm.

But do we actually have to put the question in this way? Should we begin by listing the causes of doubt and the reasons for suspicion? Should we approach the problem by comparing the hypothetical 'scientificity' of the field in question (anthropology) with a known reality ('science') so as first to list the dissatisfactions, deficiencies and ways in which it fails to conform to the model? In this perspective science in fact appears as an ideal situation to return to, in other words, more like a state than a programme. As a result, it is the degree of proximity to a supposed perceived position or to an assured archetype that is debated.

If the assertion that anthropology is not a science signifies that it will never resemble any facet of physics, the debate will be rapidly brought to a close and, moreover, it will be of no interest. The enigma only persists because disavowal has a much wider significance and aims to convince society that anthropology has no right to assume the *responsibility* for a programme of a scientific nature. But one need do no more than present this distinction to understand straightaway that it is not by measuring the distance which separates anthropology from a study directed towards a radically different object that we will learn anything about the legitimacy or illegitimacy of this claim. In other words, by observing that anthropology does not have the appearance of a science, it would be mistaken to conclude too quickly that the shortcoming lies with anthropology and not with the appearance that is expected.

Here, none the less, the debate proves itself to be trapped by the cleverly maintained sophism of a fraudulent alternative: the person who accepts the idea of 'exact' sciences is supposed to admit their superiority to disciplines which are obviously 'inexact'; opposing them, the person who challenges this superiority should conversely deny the illusions of truth and progress. Exactness exists, in short, in absolute terms or not at all. The irony is precisely that no science operates in absolute terms (even if it may sometimes dream of doing so) and that the diversity of sciences survives in so far as the carefully limited

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targets of their enquiries remain incommensurable. Analogy alone a priori allows thought to link these fields. But analogy, because it is liberated from the inherent constraints of a defined frame of reference, is only able to supply inspirations which deepen understanding, not completed arguments. The grading by degrees of scientific exactitude has, therefore, this in common with the philosophical negation of exactitude that it discreetly passes by a 'metaphysical' idea, the conclusions of which science, on principle, must never servilely ratify.

The obligation of an initial ideological choice between rigid scientific hierarchy and anarchic and disparate relativism disappear when we unearth the preceding question which this epistemological polarity masks: should we define science before concerning ourselves with the sciences, or should we list the sciences before focusing on their possible common characteristics? Ultimately, the two programmes are undoubtedly interdependent, but we should be on our guard against any overhasty denunciation of an additional absurdity in this new dilemma: we would in fact risk being unaware that, beneath the inadequate formulation, a very real issue was in the process of resolution.

All science has a duty to study the constants and variables which animate its field: biology is defined as the study of life forms and the study of its universal constituents. And, although there is rivalry between, on the one hand, social anthropology focused on invariables and, on the other, an ethnology setting great store by an inventory of differences, no one can be unaware that this strategic competition could not end in the elimination of one of these perspectives without bringing down the entire edifice. But what was the position on this precise epistemological point during the twentieth century?

Although the history of science has not been unaware of disparity in scientific investigations, as far as epistemology is concerned – with the exception of some authors driven back fairly rapidly to the fringes of their communities – it has allowed itself to become fascinated by the quest for an ultimate essence of scientificity or of a synthetic representation of 'scientific progress'. This is to the detriment of attention directed towards the responsibility and authority which an academic collectivity should assume for the field over which it claims charge.

Starting from the idea that anthropology has a specific task to fulfil, that this task is necessary, that it is scientifically justifiable and it cannot be appropriated by any other existing discipline – even if, periodically, such and such a sector of biology announces that it has discovered the way which will enable it to do so 'one day' – this radically alters the way of seeing the problem: the apparent lacunae in relation to physics and chemistry no longer assume an eminently instructive character and, in this perspective, anthropological scientificity is differentiated in a relationship between the end which it sets itself and the means it acquires in the course of the undertaking.

In this sense, to ask whether we should first examine the unity of science or the diversity of the sciences certainly reflects an incompetence, but we should perhaps ask whether, paradoxically, it does not above all characterize a broadly dominant trend in epistemology throughout the twentieth century. To put it more bluntly: the exclusion of anthropology from the scientific world might be the obsessional delusion of a type of epistemology that refuses to incorporate the framework of the humanistic sciences.[2] Denying them became, in short, the solution for refusing their participation, while continuing to assert its own ambitions – loftily rationalist and nevertheless . . . anthropological.

Epistemology versus anthropology? Assessment of 'the Sokal affair'

When we consider it a little, the emergence of a discipline specifically turned towards the conditions of existence of 'the' science are not obvious. Since all researchers are supposed to ask themselves the point of what they are doing (and to discuss this with their colleagues), the construction of an autonomous and relatively independent field across these subjects is not an option which emerges spontaneously. Unless, that is, the end is to put forward theories or laws the application of which would extend to all science without distinction. And it is clear that this wish has played a more important role in the history of epistemology than the more pragmatic desire to promote a dialogue between equals among the sciences and to co-ordinate their moves.

In this respect, a global view makes it possible to separate roughly two periods in terms of dominance. The first extended from the middle of the nineteenth century to the 1930s: or, to locate it between two events, from the publication of George Boole's *An Investigation into the Laws of Thought* (1854) to Kurt Gödel's theorem (1931), via such striking contributions as those of Gottlob Frege, Bertrand Russell and Henri Poincaré. It was the period when a 'metamathematics' was taking shape which believed itself alone was capable of elucidating the mechanisms of 'pure thought' (to use an expression of Frege's). Physics was therefore neither excluded nor immaterial (take Pierre Duhem, for example), but it had as it were a secondary role. The brake Gödel put on positivist dreams and the formidable disruption of relativity and quantum mechanics were gradually to invert this interrelation of influences and give pride of place to the analysis of discoveries about matter: Gaston Bachelard, Karl Popper, Thomas Kuhn, Paul Feyerabend or even Gérald Holton are the notable figures in this movement. All speak of science 'in general', with physics as its only springboard.

In the final analysis, mathematicians refer readily to physics and physicists do not neglect mathematics, but, with epistemological relations added to the normal interdependence of research, the growing complicity between these two great academic fields formed a deep gulf between them and the other life sciences, and still more the humanistic sciences. And isolation has been transformed into an unshakeable hierarchical position, as if it was henceforth obvious that anthropology and biology should come as close as possible to the models of scientificity which mathematicians admire in physics and to the mathematical approaches to proof which dazzle the physicists. All this thus leads us unconsciously to adopt an attitude according to which an anthropological or biological problem having no counterpart in physics thereby manifests itself as a non-scientific enigma which must be reformulated until it is reinforced by the revelation of an analogous difficulty in the sciences of matter.

The proposition will, a priori, be felt to be excessively rough or summary, but it does no more than state an obvious fact: the fundamental epistemological culture which today supports dialogue between researchers in different sectors is entirely the product of works devoted to physics and mathematics. Now, the latter are free to be seduced by the 'anarcho-relativist' principle of 'anything goes' (*tout marche, or tout se vaut*), dear to Feyerabend, but, even at the cost of apparent insolence, anthropology is then entitled to maintain that this dissolving of science into belief does not affect it: if epistemology comes to think of itself as a science of certain beliefs, called 'scientific' and yet no better nor worse than any others, it should not only expect that anthopology will be unprepared passively to

tolerate its distant gaze but that, as the general science of beliefs, it will logically demand authority over this subset.

That might seem a joke. Nevertheless, it is not so easy to rid oneself of this serious perplexion (*aporie*) which one regularly stumbles upon as soon as epistemology is confronted with anthropology. Except, of course, by deploying great reserves of cleverness in pretending not to see it. A recent polemic furnished a very symptomatic illustration: 'the Sokal affair' and its consequences. Let us recall the facts briefly: Alan Sokal, a professor of theoretical physics, succeeded in having published in the journal *Social Text* a spoof article stuffed with ridiculous citations on quantum mechanics and mathematical logic stemming from authors belonging more or less to the 'postmodern' intellectual movement. The revelation of the hoax had extensive repercussions and attracted the support of a great number of researchers hostile to the current relativist frenzy. Alas, Alan Sokal afterwards joined forces with another physicist, Jean Bricmont, to make the most of the experiment in a pamphlet-length essay[3] and the result was catastrophic because of its epistemological ambiguities: opposing a 'scientific' culture against another impudently combining philosophy and the social sciences, the denunciation of abuses surreptitiously changed direction and became a lesson addressed from on high to the anthropological disciplines.[4] Dissent could only aggravate the initial antagonism, forcing the defeated adversaries of relativism to move against an abuse the converse of that which, with their full approbation, had first been envisaged.

The preface and notes which were added in the second edition of the book to reply to the objections have accentuated the trend. Thus, to the reproach levelled at them for speaking of Sokal's hoax as a 'non-scientific experiment' with the sole aim of escaping the consequences of a declaration that it was, they responded that the expression was justified by the non-reproducible character of the experiment.[5] However, since this 'defect' served to preclude its scientific treatment, and taking account of the fact that the social sciences do not have access to such procedures, it is futile to go further: the question of their scientificity has been resolved straightaway.

The attempted experiment most definitely deserved scientific treatment, but the follow up, instead of confining itself to the technical competence of physicists, required that of history and of sociology. They preferred to carry on regardless and substitute for this examination a 'non-scientific' analysis, brought from physics and tainted with embryonic epistemology: why then claim that they respected the humanistic sciences and their specific methods? Refutation of the correct use of knowledge put forward by the postmodernists is Sokal and Bricmont's unbridled right, but taking global responsibility for its interpretation called for a broader framework of problem-solving than they wished to take into consideraton.

A single observation is not enough measure in order to serve as a comparison. One experiment cannot equally reproduce its goal. A non-reproducible experiment is worthy of 'scientific' attention if the intention of the event is likely to improve the comparison. Alan Sokal's parody lent itself to plenty of comparisons less easy to observe than those coming from reproducible experimentation 'accessible in the long term': the experiment was successful because the approval of the spurious text by the editorial board of *Social Text* made minute comparison with the real possible.

However, this confrontation required a skill other than that available to these authors. Rather than call an epistemologist to the rescue, rather than surround himself with social scientists and historians of science ready to use his daring coup for a well-ordered criticism of postmodernism (which would have done nothing to remove his glory), Sokal preferred to join forces with another physicist and they both leaped into a muddled philosophical criticism which enabled them to maintain their advantageous hierarchical position beyond their disciplinary competence: whether they admitted it or not, their aim clearly went beyond the framework of a strict scientific refutation of unauthorized reprocessing of physics and mathematics by people in the humanities. It left the knowledge which it was their responsibility to defend and went on the counter-attack. This was their right provided they accepted it and this was not actually the case. In the preface to the second edition, they justifiably complain of the personal insults they have suffered, but they are not absolutely right when they assert that they, for their part, have remained civil: this was true before this preface, no longer afterwards. The value judgements which they bring to the heterogenous mass of critics, the times when they cite their opponents and those when they reply to them without citation (because this permits then to disguise the objection targeted at them so that there is leisure to counter another argument without distinguishing it from the original), all this is sufficient indication that, at certain levels, card-carrying scientists can ape the shadiest intellectuals by scattering fraudulent tricks throughout the debate.

Sokal and Bricmont call to mind the valiant knights who rushed out of their castle to kill boors and rascals crying, 'Death to the rogues,' and who then beat a hasty retreat in the face of the common people's wish that they should do battle, taking refuge high on their ramparts and shouting that their position was one of legitimate defence. The second preface to their book adds nothing, except that, now, they stick their tongues out at all of their enemies because some of them were rude. Of two possibilities, only one can stand: either Sokal and Bricmont want to participate in epistemology, perhaps even in the humanistic sciences, and they have only to say so, accepting without hesitation the debate that follows; or, alternatively, they want to denounce a collection of abuses, culpable ignorance or impostures and, in that case, they should not take their criticism outside the bounds of what they claim to understand. The clarity of style of which they are so proud contrasts with the confusion of their attitude: the mix up effected between those who indicate their differences and those who defend themselves against their accusations is not something to be very proud of. The same goes for an aggression which alternates with prevarication and a hierarchical prejudice which gives way, at opportune moments, to tactical proclamations of humility which bring irresistibly to mind an obnoxious but entertaining reply by Albert Einstein to a lowly colleague: 'Stop! You are not good enough to be so modest!'

Does, moreover, the scientism of the humanistic sciences registered in 'certain trends of behavioural science, psychoanalysis or Marxism'[6] not call irresistibly to mind the youthful aversions recounted by Karl Popper concerning the 'closed' frames of reference erected by the Marxists, the Freudians and the Adlerians? It is not insignificant that the Cambridge teacher confessed to having examined the sources of truth in physics in response to a dissatisfaction with the humanities. Let us attempt to see whether the road can be travelled in the opposite direction.

Refutability, experience and comparison

Let us begin with the famous 'criterion of demarcation' set out by Popper at the beginning of *The Logic of Scientific Discovery*:

> If we wish to avoid the positivist's mistake of eliminating, by virtue of our criterion of demarcation, the theoretical systems of natural science, then we must choose a criterion which allows us to admit to the domain of empirical science even statements which cannot be verified.
>
> But I shall certainly admit a system as empirical or scientific only if it is capable of being tested by experience. These considerations suggest that not the verifiability but the falsifiability of a system is to be taken as the criterion of demarcation. In other words, I shall not require of a scientific system that it shall be capable of being singled out once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience.*[7]

It should be understood that the critique which follows does not remotely intend to diminish the importance of the event that makes up this thesis. It must certainly be granted to Alan Sokal that in science it is sometimes essential to attack incompetence, deception or mediocrity: it is, in sum, a 'technical' or 'ideological' duty in the face of the attendant risk of regression. But as far as scientific progress is concerned, it could only be achieved through calling into question a considerable advance: Popper's protest is the opposite of disrespect.

The beautiful thing about the key which Popper offers his readers in the passage just quoted is that it is absolutely devoid of hypocrisy: it is not shielded by any obscure convolution. The author thus instructs us in plain language that his desire to include 'natural science' presides over the formulation of the criterion of demarcation. He adds off his own bat and without special pleading that experience is an indispensable factor in refutability.

In these circumstances, there is no single criterion but two, for if refutation is obtained other than by experimentation – an a priori plausible eventuality – it would be disallowed. Popper does not only wish to contain 'natural science', he also wants to exclude all anthropology and it is thanks to a decree that he achieves his ends, not by following an argument. From then on, whatever the value of his criterion for physics and biology, it is null and void for the humanistic sciences which are not bound docilely to accept this arbitrary decision. From their point of view, the philosopher's theory should be taken as belonging to an external epistemology, exclusively philosophical: they are free to be interested in it and, assuredly, they would be mistaken not to be so, but its methodological incorporation in their domain calls for a translation reformulating the idea as the exclusive responsibility of those that make use of it.

In the event, the distortion of the reformulation consists of restoring the query that has been obliterated: is it proven that refutation requires recourse to experimentation? Certainly not: this is a postulate. As soon as the question is posed, it will be noted with legitimate alarm that, during the last half-century, epistemology has achieved unwitting feats of ingenuity so as not to face this problem and so as not to solve this enigma – significant proof of a bias which has constantly guided its preoccupations.

What is an experiment? A comparison planned in such a way that its course is controlled by technical procedures aiming to eliminate inopportune variables and working to give measurable results. The definition of the experiment and the evaluation of its competences are the fief *par excellence* of a 'sectorial' dialogue between logic and physics, and it would be appropriate at this point to comment upon the assertions of logical positivism, the 'post-positivist' reaction of 'Popperians' dissident to varying degrees (Kuhn, Feyerabend, Lakatos) and Duhem-Quine's thesis if only to convince the reader that the author, despite his lamentable ignorance in matters of physics, is not totally naïve at this level. For want of space – such an account cannot be brief – we will confine ourselves to one essential point: as Anastasios Brenner wrote in an excellent exposition of this notion, 'it is a question, at any rate, of a negative process'[8] and *reductio ad absurdum* does not permit us to conclude the truth of a proposition other than in exceptional circumstances. The function of the experiment is to invalidate hypotheses or clusters of hypotheses. Let us bear this point in mind and add two observations:

- Experimentation is a subset of the comparison, since it designates a type of method which aims to produce comparisons. The experiment seeks to establish a systematic correspondence among several possibilities. One is compared before and one afterwards, a test object and a control object that has not been 'treated', measurements gathered and measurements presented as a model: in short, situations are compared which the technique attempts to render perfectly equivalent, except in relation to a narrowly defined problem.
- Experimental epistemology has not maintained any permanent historical relationship with comparative epistemology. The latter has remained in the pool of the internal epistemology accepted by each discipline: indistinct, out of its depth in a 'primitive soup' of epistemology (in humanistic sciences, it is thus glaringly obvious that the most astute comparative methodologists are generally workers in the field, otherwise often hostile to the exercise of theory).

The monitoring of experimentation has become a theoretical field, while monitoring of comparison is, today as ever, left to the great fund of knowledge declared 'empirical'.

That appears to be vast and I should like to collect counter-examples from some thinker or other to mitigate an absurdity which, going beyond epistemology, touches all the 'hard' sciences which have given their backing to it. However, by admitting that this enquiry gleans some grounds for consolation here and there, one fact remains intact: the experiment/comparison relation has never penetrated collective thought upon scientificity as such, and has never been implicated in the exclusion of anthropology from the realm of the true sciences. We are dealing with an instance of ostracism here: a case of extreme ideological domination based on a blinding illusion causing the debilitation (strictly speaking, *political*) of the opposite interpretation. A sophism forged by an underground conviction has constructed a community – contemporary epistemology – around an arbitrary reduction of its object of study, forbidding debate between equals with the problematical questions remaining on the truncated area of the field.

To this day there is no epistemological theory explaining why non-experimental comparison is incapable of producing a refutation. And for good reason . . . Now, as this theory has not been developed, the scientific powerlessness of anthropology should be considered as an opinion: gratuitous, spontaneous and sometimes malicious.

Refutable science and refuting science

This comparative epistemology is cruelly lacking in interdisciplinary communication. It follows that it would be fantastically presumptuous to start a sketch now: the field is at least as vast and as littered with pitfalls as that of the experimental sciences. By contrast, from this point of view the question of knowing whether it is correct that social anthropology is not refutable becomes perceptibly more approachable: the irrefutability of this discipline (and of its neighbours) has always been asserted with reference to the great theories of synthesis, leaving to one side the results accumulated in the comparative sector.

What would happen today if a student gave his director of studies a thesis where myths were retranscribed as in the days of Franz Boas, kinship systems were described as in the time of Alfred Radcliffe-Brown, ecology treated as in the time of Edward Evans-Pritchard, or the economy perceived as in the time of Bronislaw Malinowski? There is no doubt as to the reply: the supervisor would not allow his pupil to submit the work to the examiners.

This shows that ethnography has made great progress in a few decades and the exhaustive monograph written in the 'heroic' decades on a local group is no longer an option. One thousand, two thousand, five thousand pages can no longer claim to define a culture – or twenty years in the field. To do that, one has to make a critique of previous inadequacies and list the descriptive imperatives the disastrous absence of which has gradually been revealed. In short, a chronic work of refutation has to be produced – methodically, even when not methodologically done (that is, explicated in the right order).

Relativism has been heavily ironic in recent times about the famous studies whose subject has been revisited by a sneering younger worker: these investigations in reverse devalue the observations of the masters and some have wished to see this as proof that, all in all, they produced only a 'literature' that was remarkable in varying degrees. The temptation to accept this point of view runs up against an embarrassing detail, however: these criticisms are refutations that are realizable thanks to the advances which the incriminated texts have, to a greater or lesser degree, facilitated or encouraged. Anyone who has glanced even just once at the authentic utterances of a mathematician or a physicist of the eighteenth century would be in a comparable position, except that they would clearly not be allowed to draw provocative conclusions about the discipline concerned.

Whatever the situation with the unofficial, indeed secret, hierarchy that permeates anthropology (following the example of many other sciences), ethnographers are not anthropoplogy's tecnhicians, even when they spurn ostensibly theoretical debate, for their descriptions cannot be confined within a purely technical activity: it is profoundly transformed under the impact of debates and criticisms from within, admittedly less spectacular than the theoretical discussion, but just as conclusive.

Epistemology come down from Olympus has not seen this facet: as far as it is concerned, it is at the level of synthesizing theories that the irrefutability of anthropological research is obvious. However, even at this level, its blinkers prevent it from noticing the main point. When Claude Lévi-Strauss was interested in totemism,[9] epistemology could question the scientific value of his conclusions: for instance, the fact that totemism is not an anthropological reality, with all that implies for universality in our species, is

insufficient to deny its ethnographical reality for a certain group of social systems, and it is appropriate to consider the drastic reduction of the anthropological agenda which would follow from a curiosity prompted exclusively by universals. That having been said, the conclusion is not all: in this study Lévi-Strauss produced a refutation of propositions accepted until then, and the position of the subject was not identical before and after his contribution.

Another illustration, still clearer: the construction by the same author of a model of the elements of kinship[10] can be abandoned without refutation *stricto sensu* only if a new formulation expands the problem and weighs the factors analysed differently. The essential feature of the relationship with the maternal uncle is thus worth considering by contrast with the 'familial' relationships of primates. Human societies are distinct in that an individual does not only exist within the group to which he belongs: contrary to monkeys who are always strangers to a band other than their own, man can visit communities to which he 'belongs' secondarily because his kinship is not reduced to daily cohabitation. As a general rule, the mother's brother is a man who lives elsewhere while being a crucial relative. This perspective can lead a researcher to study kinship relations without doing so by means of formalization. Nevertheless, there too, Lévi-Strauss's analysis is not completely devoid of significance and one part has retained its scientific efficacity: namely, the refutation of the functionalist problematic previously expressed by Alfred Radcliffe-Brown. Let us imagine that the primacy of kinship over filiation were questioned and there was a return to a concept which gave preponderance to filiation: this could no longer ever be expressed in the same terms as those once used by Radcliffe-Brown. The former theoretical framework was ultimately a thing of the past because it had been correctly refuted: its rehabilitation would require additional elements and would necessarily be partial, since it would be written in a frame of reference incommensurable with the original.

The alternation of conjectures and refutation, beloved of Popper, is thus manifest in anthropology (but Popper subtitled his book, *The Growth of Scientific Knowledge*, light years away from the accumulation of *disillusions* produced by science).[11] In some places, and at some points in time, the swing is obvious: that is sufficient to deny the supposed 'literary' inevitability denying us any hope of scientific status (for, if a science is judged by the yard-stick of its setbacks or the idiocies uttered in its name, there are surely experimental sciences whose discomfiture would be no less). The confusion comes from the fact that, in expressing a conjecture, not everything necessarily lends itself to refutability and it is important to differentiate at this level the different authorities involved in the construction of a theory. But it would be very surprising if one did not encounter comparable heterogeneities in physics: who, then, would not therefore be tempted to wager that classical epistemology has treated them infinitely more leniently than it has anthropology? Manifestly, the asymmetry in epistemology's view of the 'hard' sciences on the one hand and of social sciences on the other cannot be explained solely by an asymmetry in the methods practised by these disciplines.

That is not all. In returning to Popper's criterion of demarcation, its oustanding weakness will now be understood. He caught a fleeting glimpse of it himself in the page following the passage quoted earlier: 'Not for nothing do we call the laws of nature "laws": the more they prohibit, the more they say.'[12] Quite. And that is precisely what is lacking in his analysis of scientificity.

Science is refutable and *refuting*. In its way, alchemy was demonstrably refutable: it did not refute. Science, too, manifests itself as debatable by actually debating: the two facets are too closely interdependent to isolate one without producing a distortion in the enigma to be resolved. Examining scientificity in the *state* of refutability and outside the *power* of refutation would be equivalent to severing logic and process, conceiving form without evolution, coherence without history. Scientific discovery then loses its moorings and is cast adrift: its significance is sought down in the hold rather than in its wake.

Einstein was neither 'refuting' nor refutable when he baulked at the idea of God playing at dice: but he was when he targeted Newton. He saw, contrary to Planck, that Planck's constant was incompatible with Newtonian physics and, if, some time later, he was demonstrably entirely serene faced with the experiment (reproducible?) which the astronomers devised to put his theory of relativity to the test, it was because he was persuaded that he had deduced the only possible path for circumventing the obstacle. Niels Bohr was later to be 'against' Einstein at the atomic level while remaining beside him 'against' Newton.

Science is in permanent denial. It does not accumulate positive knowledge, it acquires denials. Its true progress consists in a contraction of the spectrum of the possible. Its growth is that of an unbelief: denial is permanent and vision provisional. At its furthest extent, the positive proposition is reduced to an instrument for the manufacture of future repudiations, to a provisional compromise with technical authority (which for its part needs to rely on assertions) and to the result of a dialogue with ideological inspiration. Within their blatant antagonism, a partial complicity emerges here between Popper and relativism: a common scorn with regard to the methodological demands of frames of reference (such as the spatial, temporal and thematic delimitations of the reality to be described), this concept being jointly necessary for a treatment of refutable / refuting interdependence and for a discrimination between science and belief. It is only in the pure absolute – an environment alien to science strictly speaking – that the criterion of the refutable achieves dissociation from the practices of refutation and that rationalism resembles a belief.

Persistant advances towards a 'normal' anthropology

The preceding pages clearly only involve their author: they will have achieved their end if they have convinced the reader that, despite the lofty ironies and the damning verdicts, anthropological scientificity is much less easily rejected than fashionable fantasy claims. This brainwashing is unfortunately manufactured by the intelligentsia of affluent countries who have just passed the end of the millennium in a deplorable state of exhaustion, while making a pretence of taking it for a starting-line. We can be certain that in disadvantaged or more disrupted countries the idea of reducing social science to an alternative literature is infinitely less attractive.

The point of departure for this issue can then be summed up as this: renouncing the constraints of science is the best means which a university community could devise to rid itself of its responsibilities. With or without the backing of the recognized sciences – 'hard' perhaps, but not necessarily just – the persistance of this ambition remains the sole

horizon acceptable to an anthopological discipline conscious of its social function. Or, to use a high-flown phrase, its duty.

The basic idea was to ask researchers to react on this point while ensuring that the greatest possible diversity of viewpoints was presented. With such a broad theme, there was no question of treating the subject systematically, nor of guaranteeing any theoretical consistency whatsoever. The aim was not polemical, in the sense that it did not aim to attack any given school: it was on the contrary to defend the wish to maintain a certain type of programme and show that this wish, although disparate and unspectacular, remains present and active in many forms in social anthropology.

We immediately thought that three representatives from the major traditions of this discipline should be brought in, that is to say the nations who throughout the twentieth century financed an ethnological programme regularly visiting the five continents: the United States, the United Kingdom and France. And, as far as possible, they should be 'authorities' whose speech would have a symbolic significance. The first three individuals contacted, Sydney Mintz, Jonathan Benthall and Michel Panoff immediately accepted the proposal and we thank them all the more warmly since this fact is in itself a genuine token of optimism. I should add that each one of them welcomed with keen pleasure the name and the particpation of the two others.

Sidney Mintz, at least since the publication of *Sweetness and Power*,[13] has come to serve as a favoured bench-mark for American anthropologists wishing to differentiate themselves from the intellectual crazes which, in his country, follow each other in quick succession. He reminds us that one can be an innovator without throwing overboard all the richness of one's history. In derisive acknowledgement, the reviewer of one of his recent books marvelled that Mintz did not feel himself compelled to cite Michel Foucault! He shared this role as a dependable reference-point for a somewhat disorientated and unselective generation with a close friend, recently deceased: Eric Wolf, whose grim prognosis given to him in confidence at the time of their last meeting he recalls at the beginning of his article. Following his usual practice – but this custom rightly prides itself on being the worst enemy of routine – Mintz decided to approach the problem posed by aiming directly at an exceptionally sensitive area: the living material of culture, the material life of culture, the culture which lives through materialization and which materializes in life. The reader is asked not to skim too speedily over some passages where the limpidity and apparent simplicity of the reasoning suffer from the disadvantage of insufficiently signalling that a threshold is being crossed.

Jonathan Benthall, long Director of the highly prestigious Royal Anthropological Institute and editor of *Anthropology Today* (undoubtedly the discipline's only bi-monthly periodical) is essential both as a witness and as a crucially important actor in the very distinctive and extraordinarily determinant microcosm of British anthropology. No one is unaware that the latter at once constitutes the major temple of the profession where an attentive watch is kept over its primary values and a bustling agora which constantly oversees or challenges their continuing *raison d'être*. His text is a magnificent overview of different areas ripe for renewed scientific zeal, with that remarkable elegance which reaches out far beyond the English-speaking context. Like Mintz, he begins his survey by calling to mind a great figure of the past, Edmund Leach, which we hoped for without daring to suggest. To continue a discussion with a friend or teacher silenced by death is surely the finest form of homage one can pay them.

In a French university constantly resounding with 'clan' tensions, Michel Panoff is a rare case of a personality at once influential and on the fringe: a former President of the Association Française des Anthropologues, and the author of works on the position of his profession,[14] his comments rarely lapse into mollifying ecumenism. As may easily be observed in his contribution to this issue, they are not gratuitously acidic: they strip the questions which the historical hypocrisies of the surrounding society, and indeed the ethnologists' own unacknowledged desire for an intellectual 'quiet life', have covered with a wordy and inconsistent froth to disguise its original content. This is a third way of calling to mind the methodological requirements for travelling between the causes for alarm and subjects for hope.

The remaining articles, requested from younger scholars, each simultaneously highlight one source of support and one challenge for an interdependent anthropology today. Thomas Schippers good-naturedly embarks on the problem via teaching and describes the avatars of a professor discovering an audience for whom ethnology is no longer a passion but a transitory distraction. He then considers the most terrible exoticism an ethnologist could encounter: the administrative constraints which a bureaucracy looking for an impossible lack of education stresses by means of fairly startling terms ('employability', *l'employabilité*). Béatrice Ruiz, seriously affected by the difficulties of applied research, embarks here on an intrepid epistemological study: not only are the two not incompatible, but the perspective of utility is one of the most rigorous guardians of epistemological quandaries. Her observations at times intersect with or complete those made in this introduction, something which is definitely not superfluous: let us only hope that her forays into the realms of physics and mathematics will not bring down the thunderbolts of Alan Sokal and Jean Bricmont on her head. As for Bojan Baskar, his intervention was most certainly necessary and long-awaited: in effect he gives us an analysis concerning an area of recent numerous bloody upheavals (the Balkans) which should stimulate anthropology to organize its reasons for existence rather than writing it off.

Finally, probably stung by Umberto Eco's claim that if Italy were to sink in to the Mediterranean the French would be the last to notice, we wanted to have an Italian author here: although the humanistic sciences in Italy do not display a tradition as structured as in France or in England, they have often acted as a brief spur to lasting results in the great European debates, compensating for the inconsistency of these contributions with the intellectual spark of crowds of propositions that are unexpected and 'disturbing' in the best sense of the word. Following in Ernesto de Martino's remarkable wake, everything indicated that it should be Clara Gallini who should remind us of the frequent efficacity of the Italian presence and here she fulfils our wishes by defying the rational asthenia of relativism in its own principal stronghold: literature, but a literature which, through the intermediary of Émile Zola, seeks anthropological truths and means to respect the reality of suffering.

This document is thus completed by stressing a kind of colouring that has suffused it throughout: can one imagine a worse insult coming from a position of comfort to those which are precariously fragile than the composition of a dilettante anthropology which flits freely under the camouflage of a general esotericism?

Georges Guille-Escuret
C.N.R.S., Paris
[translated from the French by Juliet Vale]

Notes

1. This expression, which prompted many reactions, featured in an interview given to the newspaper *Le Monde* (8 October 1991).
2. It goes without saying that this suspicion does not affect a more classical philosophy of knowledge which does not cultivate ambiguity of thought in relation to the 'extension' of science.
3. Alan Sokal and Jean Bricmont (1997), *Impostures intellectuelles*, Paris: Odile Jacob. This book reprints as an appendix the article which appeared in *Social Text*.
4. See Georges Guille-Escuret (1998), 'Desmodèles aux patrons: les sciences humaines en tenaille'. *Les temps modernes*, **600**, 265–284.
5. Sokal and Bricmont, *op. cit.* (2nd edn; 1999), 34.
6. Sokal and Bricmont, *op. cit.* (1997), 193.
7. Karl Popper (1959), *The Logic of Scientific Discovery*, London: Hutchinson, 40–41; French edition (1973) *La logique de la découverte scientifique*, Paris: Payot, 37. The French edition uses the barbaric term *falsifiabilité* ('falisifiability'), for which, except in the quotations, I have preferred to substitute the word *refutabilité* ('refutability' with, however, less hesitation than the numerous authors who have already made this rectification).
8. Anastasios Brenner (1999), 'Experience', in Dominique Lecourt (ed.), *Documents d'histoire et de philosophie des sciences*, Paris: PUF, 400–404.
9. Claude Lévi-Strauss (1962), *Le totémisme aujourd'hui*, Paris: PUF.
10. Claude Lévi-Strauss (1958), *Anthropologie structurale*, Paris: Plon.
11. Karl Popper (1985), *Conjecture et réfutations. La croissance du savoir scientifique*, Paris: Payot.
12. Popper *op. cit.* (1959), 41; (1973), 38.
13. Sidney Mintz (1985), *Sweetness and Power*, New York: Penguin Books; French translation (1991), *Sucre blanc, misère noire. Le goût et le pouvoir*, Paris: Nathan.
14. See especially Michel Panoff (1977), *Ethnologie: le deuxième souffle*, Paris, Payot.

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