A well-articulated primatology. Reflexions of a fellow-traveller*

Bruno Latour

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The lively interactions at the meeting in Teresopolis started badly and ended up beautifully. At least three unbridgeable gaps threatened the discussion to an early dead end: Is science a social fabrication or an asymptotic access to reality? Do gender, theoretical biases and methodological principles inevitably distort the quality of science or is a good scientist the one able to escape from these shackles? Finally, is it culture or nature, sociology or biology, that determinates most of our (human and non-human) behavior? The first debate could pit primatologists against "science studies", the former scholars asking the latter: "do you believe in reality?" while the science studies people would retort: "do you really believe monkeys to be squashed flat inside the pages of your articles about them?". The second debate could degenerate in an endless purification rite, everyone insisting on being protected or polluted by the "biases" of gender and paradigms ("I learned a lot from you as a woman", said one silverback, to which the other retorted pointedly: "you learned a lot from me because I am a good scientist, that's all".). The third debate would have subjected the participants to another reel of the nature-nurture fallacy, the "biologist" people defending the universality and constraints of their type of causality, while the "culture" people would have insisted *ad nauseam* on the variability, pliability, historicity of human and animal behaviour.

Fortunately for all of us, we did not get stuck in these three different, but interrelated, false debates, and slowly and painfully moved toward another agenda that I want to outline in this chapter by following, in three different sections, how each of those old and tired discussions was reconfigured, thanks to the extraordinary set up devised by the organizers and for which I am infinitely grateful.

The vascularization of science and society

The first originality of the meeting is to have brought together "science studies" and scientists which at first were presented to each other as two "camps" --which

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appeared to me as bizarre as taking primates as one camp and primatologists as another. Yet it was a matter of time before things got far more complicated: alliances began to shift without obeying party lines. It was soon impossible to consider that there were only two sides, the first made up of those who believed data were fabricated out of thin air, and the second of those who believed they possessed an unmediated and undisputable access to the reality "out there".

The reason for this high learning curve is easy to understand in retrospect: "science studies" is to scientific practice what primatology is to primates. Without primatology, in order to speak of apes and monkeys, we would have to rely on a few anecdotes brought back by missionnaires and explorers; we would have no data, no comparative basis, no more than the shaby representations of wilderness and savagery with which the Western culture has equipped us from the beginnings. The change in the quality of our representation of apes and monkeys might not be terribly important to the primates --although it certainly does have an effect on the conservation of many animal troops (see chapter by Strum)-- but it is certainly of great import to us, as a human community. To deprive ourselves of all the knowledge accumulated on primates in the last fifty years (see Fedigan and Strum chapter xx), would be an incredible loss.

The same is largely true for science studies, although the mass of knowledge is much smaller and the data softer (see chapter by Cussins). Without empirical studies of scientists at work, we would be limited to anecdotes and myths about a few stuffed "great scientists" hung on the walls of the university Hall of Champions. Science studies might make little difference to scientists at the bench, since, after all, they know what they do (as well as baboons know how they behave), but it would make a difference to all of us as a human community. To deprive ourselves of the knowledge accumulated for the last twenty years in history, sociology and philosophy of science on the practice of science, in order to go back to a sort of mid XIXth century positivism, would certainly be a pity --although I must admit I preach for my parish. Whatever the assessment of the two disciplines and the differences in quality and status, the discussion cannot be productive if one accepts the empirical revolution it brought about while shrugging off the other as irrelevant. In my opinion, by the end of the meeting, there was some agreement that to return to a world made up of Dr Livingstone's or Lord Zuckerman's baboons would be no more possible than to backpedal into an epistemology peopled by Auguste Comte' or Karl Popper's scientists.

This point being settled, the next problem to arise was that people at the conference were all well read in primatology, but very few had read much of the empirical work of the other discipline, science studies. To ask a sociologist of

science "would you jump out of twenty story building since you believe gravity to be socially constructed?" is like asking a primatologist "monkeys are disgusting and promisucous wild beasts aren't they?" No answer is to be expected and no answer should be given --except the one offered by Donna Haraway: "Push the heckler through the window!" To produce knowledge about scientific practice and to debunk the mythology of Science, capital S, is no more a denial of the reality of the scientific facts themselves, than studying the social complexity of sexual competition and debunking the myth of the "wild beast" is a denial of the reality of the animals out there in the bush. Quite the contrary. To the question raised at one session "what is responsible for the changing views of primates in the last fifty years?", the only answer I could find was: "but the primates themselves of course, it is they who forced us to modify our account of them", exactly as I would have answered "we learn every bit of the new 'science studies' from the scientists themselves", if I had to explain the recent shifts in the definition of what science is and what makes it tick. "Please relax", I was tempted to say to some of my more anxious fellows around the table. "Reality is not in question here." The debate does not oppose reality on one side and irreality on the other, but realities on both sides, or more exactly, it opposes, on the one hand, a realistic vision of what primates (and scientists) are, and, on the other hand, a totally unrealistic or mythical version of what primates (or scientists) do.

If we accept to compare two empirical disciplines, and if we now leave aside the red herring of reality versus "pure social construction", nothing is yet solved because the question becomes: how did we make the primates themselves relevant to the questions we asked about them so that they could have a part in what we say of their behavior, while, before primatology started in earnest, they had so little to say in the representation Westerners had of them. A little summary of science studies is indispensable at this juncture if we are to continue. In order to be clear and alas sketchy, I will use a simple diagram to give an idea of the amount of work necessary to make the primates accountable for the facts produced by the discipline of primatology.

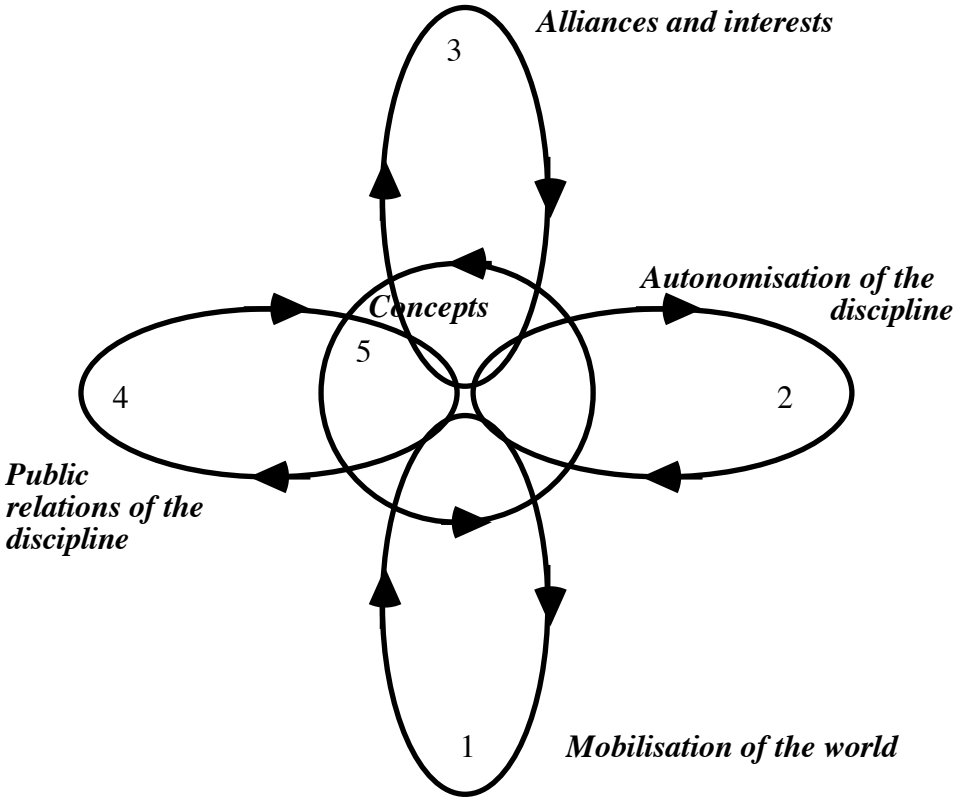

Figure 1
A simplified view of the flows of knowledge
(from (Latour, 1995a)

If we had to study primatology, we would be interested in five different horizons of practice, each of them being simultaneously necessary to make a science productive.

The first horizon --but one can start by any loop of figure 1 since it is a flow-- is what can be called the "mobilization of the world", that is, all the efforts invested in creating a field site --or an enclosure or a laboratory-- a data-producing unit. Every participant at the conference, including of course those in "science studies", knew too well the immense effort that goes on in obtaining a field site, maintaining it for any lenght of time, habituating the animal, mapping the territory, bringing the animals into enclosure, caring for them, feeding them, equipping them with various devices, sampling them, etc. The beauty of primatology being the number of intermediary situations between field sites in unprotected areas all the way to brain surgery into laboratory conditions on animals held in surgical table. The general point is that no matter how much natural historians like to get up early with binoculars and enjoy sunrise in the bush, no matter how neurobiologists like to watch single-neuron firing up, they would immediately stop being scientists if they ceased to <u>return</u> from their instruments --broadly conceived-- with <u>data</u> which have

the peculiarity of being highly transformed information. A practicing scientist is never presented simply with information, but always with a trans-formation that should nonetheless maintain as intact as possible, the features she is most interested in --hence the circular form I gave the loop (Latour, 1995b).

Although this work might account for 80% of the time, sweat and ingenuity of a scientist, this is not enough to produce a discipline. Another vascularization is necessary. A scientist needs colleagues as much as data, and the former is no easier to obtain than the latter. The second loop we must take into account, is that which can be designated as "autonomisation of the discipline", which is as much hard work as tending to the instruments. What is a primatologist? A zoologist first and foremost? An anthropologist interested in early man? A socio-biologist following selfish genes? A psychologist? We all know how difficult it it to solve these questions. Everyone at the conference was deeply aware of the fifty year work which was necessary to produce professional associations, journals, institutions, conferences, evaluation processes, so that the data painfully extracted from the instruments could be made relevant and the various benefits from different experimental or naturalistic set ups would be available for comparison. Without colleagues, no quality control and thus no relevant data could be produced and made to circulate. A scientist who simply enjoyed field work but who had no colleagues, would have no existence and no visibility. He might just as well have stayed in the bush fascinated by the beauty of the sunset.

This is not the end of his work however. Because to have data and colleagues, another enormous amount of work has to be done, this time on the third horizon that can be called "alliances". Field sites are expensive to keep up, zoos are huge organisations, journals cost a fortune if they have good referees, graduate programs to recruit future colleagues are expensive, laboratory tests are time consuming. No matter how much a scientist is interested in her animals, and no matter how many colleagues she enjoys having, she still has to interest non-scientists in her production system. A third vascularisation is necessary which is in no way external or subsidiary, but internal and coextensive with the work to be done and which can lead a primatologist very far away from his colleagues to the strangest people, even the military (Haraway, 1989). Arguments for doing research must be provided, grant applications written, relevant issues outlined. A scientist should interest his agencies and foundations with the same degree of enthousiasm he uses to convince his colleagues, or the same measure of concern he displays for his animals. No one said that being a scientist was an easy job! Again, everyone in the room knew fairly well how many non-primatologists were necessary to make up primatology as an autonomous discipline. A rough but good indicator would be to count the number of

different institutions thanked in the acknowledgements of each of the papers produced by the people assembled in Teresopolis throughout their careers. One would quickly reach the hundreds. For each of them, much "networking" was necessary in order to persuade outsiders that their lives could not go on without first developping primatology. Without the translated interest of all these outsiders, the discipline as a whole would grind to a halt.

But there is a fourth loop which is as essential as the other three especially in the case of primatology: the "public relation" or the "public appeal" of a discipline. In addition to the "science studies" people, the organizers had invited several scholars interested in the public representation of apes and monkeys (see chapters by Mitman and Noble). As with the others, the discussion started off badly, scientists complaining about being either distorted and manipulated by the media or, worse, ignored. But for this issue as well, the learning curve was high. It was soon clear to all that a discipline that claims to be relevant to everything from the origin of man and woman, the genetics of violence, the antiquity of emotion and sex roles, to the necessity of conservation, it was utterly impossible to exclude the public --all the more so since it was public opinions that historically generated the interest of those who had become the discipline's allies in the former loop. Long before Darwin's day, the impact of primatology and the question of the descent of man and its relation to the rest of the animal kingdom, has made primatology an indispensable trading zone between ideologies and sciences. If we had forgotten this essential feature of the discipline, Donna Haraway's Primate visions (Haraway, 1989) would have reminded us that films, museums, advertisements, popular culture, all play an enormous role in activating the whole of primatology, providing muh of its interest, passion and energy. Here too, this vascularization is essential and the comparison offered by the hyenas (see chapter by Glickman) highlights how difficult it is to work on animals which have a "public relation problem" and, by contrast, how difficult it is to deal with animals which are, so to speak, "too much loved"!

A scientist however is not only sweating to produce good data, disputing with her colleagues to have papers evaluated and accepted, convincing agencies and foundations to finance her field site and local authorities not to trap her animals; she is not only making popular films, preparing slide shows and organizing conferences to raise money and redress the image given her argument; she also has to think about how the whole flow of information (assesment, argument, money, image myth) holds together as one coherent whole. The fifth horizon is no more and no less important as the other four. It can be called "concept", "theory" or "paradigm". Contrary to misconceptions, science studies is exactly as interested in this specific type of vascularization as in the four others. Concepts however are not colored

lenses that would distorts our view of things, to use the very unfortunate optic metaphor that will be criticized in the nex section. Nor are they Platonic ideals floating far from the four other loops as if, to take seriously the "cognitive dimension of science", we had to escape to another world. Concepts are more like a beating heart that reoxygenates the blood provided it is connected to the rest of the circulatory system. Yes, concepts are the heart of science, but one has rarely seen a functioning heart cut off from the rest of its body! "Male dominance", "kin selection", "proximate and ultimate causality", "bonobo scenario", "selfish genes", to take a few examples are highly complex integrations of masses of data, hunches, customs, habits of thought, that cannot be easily discarded as irrelevant nor, on the contrary, can they recapitulate what the whole discipline is about. When you isolate them from the rest you have nothing. When you have the rest without them, it is like holding a dishevelled bundle of wool. Theories are highly practical operators that do not constitute an "inner nucleus" of science which could be excised out of a protoplasm.

The point of quickly commenting on this five horizon diagram is not to do the science study of primatology --although it would be a worthy task that Haraway and several others have already started-- but to list the number of elements that should be taken into account to "make primates relevants to what they allow us to say about them". If a scientist was mad enough to brush aside all these loops and scream in exasperation: "but let us get rid of all this sociology and history of science, of all these inpedimenta, instruments, professions, journals, institutions, agencies, TV crews, exhibits, theories, concepts, paradigms, and let us go back to the animal themselves, let us seize them unfettered and unimpeded!", he would not produce a better knowledge; he would produce no knowledge at all, but rather be lost in the contemplation of a troop of fuzzy creatures even the name of whom would escape him --since taxonomical labels reside in books, university training, databanks and museums as well. Such a scientist might be enraptured by primates, but would be lost to science, and so would anyone who would have failed to fulfill at once the contradictory tasks requested by four different horizons. Yes, science is hard work, and each of these mediations necessary to allow primates to have a say in our language. Our discourse can be accurate, but only on the conditions that each of these transformations be carried out. Such is the great lesson of science studies: no one can jump outside of mediations and speak in truth about the outside world. To cut science off from its rich vascularization is equivalent to killing it.

It should be clear by now that the discovery of science studies is not that a science could be influenced or distorted by "outside" factors such as ideologies, politics, cultural biases or psychological passions. The discovery --if this grand

word can used for such a humble discipline-- is much more interesting yet, and slightly resembles that, if I dare say, of the great William Harvey himself! "Facts" are circulating entities. They are like a fluid flowing through a complex network, a rough sketch of which has been given in figure 1. What circulates is a certain type of transformations which allow the world some bearing on what we say about it. Thus, the triple notion of an outside world of nature "out there", an inner core of science "in there", and a political or social domain "down there", can no longer be sustained. When, during the conference, a silverback believes he is stating the obvious by saying: "we should not confuse our representation of the chimps and what the chimps are doing out there", he is in fact asking us to split in two the rich vascularization "science studies" aims at describing without artificial interruption. The only goal of a primatology discipline is precisely to find many ways to mix, confuse, intermingle what "primates are doing out there" and "what we say about them". But to understand this, a second false debate has to be pushed aside[1].

From the metaphor of gaze to that of proposition

The difficulty of integrating science studies and primatology was reinforced, during the meeting, by the organizers' original intention to probe "the role of theory, method and gender" in "the changing images of primates". This earlier agenda, by its very formulation, could do nothing but paralyze the discussion since it imposed on each of us the fruitless task of purifying, in the sentences uttered about our animals, what depended on "them" and what depended on "us". If I have been right in the former section, this would have been tantamount to severing all the vascularizations that make up a discipline, and striving toward the impossible task of having animals, on the one hand, and statements about them, on the other, with nothing in between. The dynamic of the meeting, however, slowly eroded this original intention, and began to nudge us toward a completely different set of metaphors. So as to be able to do the reflexive work required of us, we moved from an optical metaphor to a new one that I will call "proposition".

To be sure, the original intention relied on a perfectly sturdy and venerable intellectual ressource. Like in the "parrallelogram of forces" we all learned at school, any statement about a state of affair can be considered as the "resultant" of two forces: what the world is like, what we are equipped to say about it. If our biases are stronger, the resultant shifts toward one axis, while, if the world is

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[1] I am well aware that this distinction between representation and things, or, to speak more philosophically, epistemological questions and ontological ones, is built in the culture for much stronger political reasons that have nothing to do with primates nor Teresopolis. I have traced elsewhere part of this genealogy (Latour, 1997).

somehow strong enough, the statement about it moves towards the opposite direction. With this classical model, we imagine our statements to be twice constrained, not only by the world but also by our mental and cultural equipment. It thus makes a lot of sense to try to weigh the different components and to measure, for each historical period, which one is stronger and which is weaker.

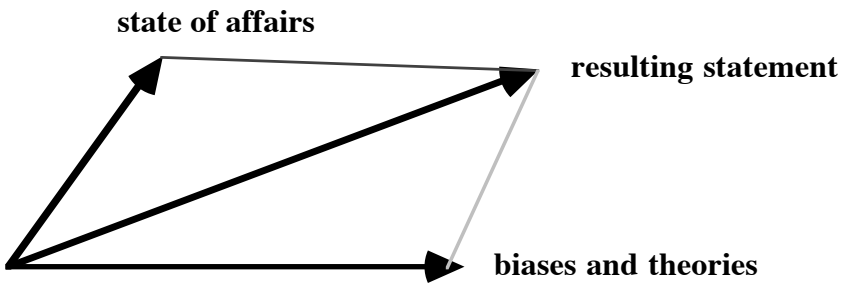

Figure 2: the dualist model
a statement is the resultant between two opposite forces,
what the world is like and what we are equipped to say about it

This model is obviously better than the naive idea of science as an exact replica of the world, and it makes, I must confess, perfect commonsense. Yet, it is utterly wrong, common sense rarely being a trustworthy guide in scientific matters. A simple thought experiment demonstrates this point easily. What would happen if there was no counterforce coming from the axis that I have called "biases and theories"? According to the model, it means that the resultant will be entirely determined by the state of affairs at hand. Thus, if we had no theory, no preconception, no bias, no standpoint whatsoever, we would benefit from an undisputable, unmediated, pristine access to things in themselves. No laboratory scientist would believe that for a minute; the same can be said of a natural historian: they all know too well the work needed to make a feature of the outside world visible. Steve Glickman, during the conference, presented us with a simple and startling example. Ethologists of hyenas had waited for his paper published in 1990 to tell the difference between a male hyena penis and a female clitoris! Since Aristotle's day, this question had been, if I dare say, pending; after reading his paper, the difference could easily be discerned with a few minutes of training, as we all could witness for ourselves.[2] To "make something" visible is thus an entirely different task than to calculate the resultant of a parrallelogram of forces.

The traditionnal model does not work any better if we push the couple of forces in the other direction and imagine a statement that is not counterbalanced by any

[2] Frank, L. G., Glickman, S. E. & Powch, I. (1990) Sexual dimorphism in the spotted hyaena. Journal of Zoology, London, 221, 308-313.

constraint from the outside world. According to the diagram, it would mean that our statement about the world would be solely dependent on our earlier repertoire of myths, prejudices, presuppositions and biases, a pure story without any grounding in the world out there. No practicing scientist would believe this for a minute. How could such exotic notions as kin selection, Machiavellian intelligence, social complexity, matriarchy, social tools, pulses of testosterone, be devised without a long and complete intimacy with the animal themselves? Where in the preexisting culture would these notions reside so as to make these stories up from scratch? No imagination is fertile enough to produce even the most simple facts of primatology. To take up the hyena's example once more, what the millenaries of cultural biases have taught us is nil compared to what we have learned since the opening of the Berkeley enclosure: repetitive slurs, endless rumors, nothing to even begin to articulate what makes hyenas' sexuality so specific. What the dualist model of a resultant between two opposite constraints does not explain is precisely what we want more dearly to understand: how do the animals out there contribute to how we imagine stories that no one without some level of intimate familiarity could dream up? How could the set up we ceaselessly devise elicit features in the life of the animals that were invisible to all before we start making them up? Surely a tug of war between two contrary forces will not do the job.

One sentence by Thelma Rowell will clearly exemplify the alternative model that was slowly seeping through in our discussions. Speaking about her new study on sheep, she stated one of her "biases" in the following way: "I tried to give my sheep the opportunity to behave like chimps, not that I believe that they would be like chimps, but because I am sure that if you take sheep for boring sheep by opposition to intelligent chimps they would not have a chance". What on earth could this little clause mean: "give them the opportunity to behave" by opposition to "not having a chance"?

A whole new philosophy of scientific practice resides in this extraordinary statement: "to give the opportunity to behave" is not the same thing as "imposing a bias onto" animals that cannot say a thing. Rowell states the difference between "a bias" and "an opportunity" very explicitely, since she insists that she does not believe sheep to be "like" chimps and since, left to their own devices, boring sheep will remain boring sheep for ever. What does she mean, in my view? By importing the notion of intelligent behavior from a "charismatic animal" --another one of her treasurable expressions!-- , she might modify, subvert, elicit, in the understanding of sheep behavior features that were until then invisible because of the prejudices with which "boring sheep" have always been treated. She does not oppose, as in the dualist model criticised above, what sheep are really doing, with stories about them.

On the contrary, it is because she artificially and willingly imposes on sheep another ressource coming from elsewhere that "they could have a chance" to behave intelligently. But Thelma Rowell does not say that she is inventing sheep, socially constructing them, making them up at her wishes. On the contrary, it is because of this very artificial collage between unrelated animals --charismatic chimps and boring sheep-- that she can best reveal what sheep really are. Her sentence would make no sense in the dualist paradigm portrayed in figure 2, since she would have to choose features according to an absurd question: are the sheep really intelligent or did you invent what they are? or is it a combination, a resultant of both? "None of the above", she should answer. "By placing them, quite deliberately and quite artificially, into the paradigm of intelligent chimps, I gave them a chance to express features of behaviour hithertho unknown. The more I work at it, the more autonomous my sheep may become."[3]

Thelma Rowell's sentence is in no way exceptional. It is, on the contrary, the common parlance of practicing scientists. For them, intensity of work and autonomy of what their object of study does, is synonymous. The better fabricated a fact, the more independant it is. Scientists behave as if they were "giving an opportunity" to phenomena that, in other settings, would not be "given a chance".[4] However, what makes this very common way of talking disappear from the scientists' own philosophy of science --not to mention philosophy of science itself, safely removed from all the empirical difficulties of benches, enclosures and field sites--, is the pervasive optical metaphor they have been made to use. If you transform all the actions that make the autonomy of scientific facts possible into "filters" that "color", "bias" or "distort" the view that a gaze should have of a phenomenon, then the very originality of scientific work becomes unaccountable. With the optical metaphor, the only reasonable outcome one can strive for is to get rid of all the filters in order "to see things as they are". Thus the work necessary to make things visible has itself been made invisible, and every reminder by sociologists, feminists, anthropologists, epistemologists, psychologists that there are indeed "biases", "filters", "colored glasses", "prejudices", "standpoints", "paradigms", "a prioris" will be considered as so many ways to weaken the quality of a science or to debunk its claim to truth. The only good gaze, according to the optical metaphor, is the one that is interrupted by nothing.

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3 The sentence is all the more interesting since it deals with a purely observational ethology which has none of the usual features of laboratory experiments where it is always easier to show the artificiality of the set up (Hacking, 1992). For a treatment of a similar sentence by Louis Pasteur see (Latour, 1996a).

4 See the beautiful case studied by (Despret, 1996) on the theories devised by A. Zahavi about Arabian babble. See the chapters by Glickman on what happened to laboratory rats in the cages of the behaviourists.

The dynamics of the meeting in Teresopolis were fascinating to watch --difficult not to use the metaphor even when criticizing it!-- because the organisers rang the bell at every session trying to bring us back to a reflexive enquiry about "the role of theory, method and gender in shaping changing images of primates", while the dualist model under which we all operated to answer this question fell apart more and more clearly as the days went on. Gender, for instance, did not appear as a filter that would make male and female scientists see things differently, blinding the males to some features while revealing others to the more perceptive female primatologists (see chapter by Fox-Keller). Gender, in the conference, began to play the same role as "intelligent chimps" in the sentence analyzed above. Not that of a filter or of a bias, but that of a trope to use Donna Haraway's favorite expression. In the striking paper on the respective activity of eggs and sperms (see chapter by Tang-Martinez) the importation to an unpredictable domain --reproductive physiology-- of all the political debates in feminism over the dispatching of passivity and activity, allowed the ovula to "have the opportunity" of entering into a bewildering range of behaviour instead of being considered as a "boring passive egg". The sessions on gender at the conference then shifted from a rather counter-productive soul-searching about whether or not a given primatologist was or was not "biased by gender", to a much more interesting research program: how much activity can be granted any given entity if we accept to use the "indignation against passivity" gained by decades of feminist struggle, as a resource to "give a new chance" to an animal or to one of its components --more of this in the next section.

The same shift quickly consumed the vague notion of theory and method. When it is said that the Japanese method was to stay on the field at least as long as an animal's life span (see chapter by Takasaki), this obviously cannot be considered as a "bias" that would "limit" the vision of the animals. Quite the opposite. This simple decision entails different animals since it allows them to expand their interactions over a much longer period. This does not mean that those who go into the field for no more than a week with the sole purpose of collecting blood samples for their population genetics model are more "biased", but rather that they will give the animals a chance to behave differently. The same is true of important decisions like going to the field in Kenya instead of staying in an enclosure, or naming the animals individually, or following them on foot instead of watching them from the safe haven of a jeep. Each of these moves, allowed for new differences in the animal to be elicited or educed. The *de facto* abandon of the optical metaphor was clear to all in one of the sessions devoted to the role of theory, when the following contradictory verbs were all used to describe what various concepts were making us do: "interact" "focus", "connect", "synthetize", "accelerate", "highlight", "raise a

problem", "provide a solution", "polarise attention", "shift attention", "legitimate", "vindicate", "challenge" "stimulate", etc. Surely all this very rich and active repertoire could not be squeezed under the label of "filter" or "standpoint" for an outside gaze looking at a thing out there. They made a lot of sense, on the contrary, if theories are placed, like the fifth loop of figure 1, at the heart of several flows of data transformations.

What other metaphor would do justice to this practice and replace the old tired optical apparatus that limits the reflexivity of a scientific discipline to such an extent? In the paper prepared in advance for the conference, I had proposed, rather jokingly, to shift from the gaze metaphor to a gas metaphor! This had the advantage of keeping with the fluidity of facts introduced in the first section. When you put gas into the tank of your car, you are indeed connected with the oil fields of Saudi Arabia. These connections can certainly not be construed as so many "biases" which would have "distorted" the "real oil" out there. At the very least, if one wished away all these impedimenta, cracking, transformations, transportations, refineries, in order to gain access to the "oil itself", one would be left with no gas at all... The reality of oil in Saudi Arabia is proven by the number of transformations it undergoes before ending up as gas in your tank. So, with the gas metaphor, it is impossible to use the cracking and transformations of oil against the reality or against the quality of the final product. You have to choose either oil without transformation and thus no gas at all, or a lot of transformations but then you get gas instead of oil!

This gas metaphor, however, does not do justice to the originality of scientific transformation. To be sure, it outlines well the circulation and fluidity of the reference and it nicely emphasizes the impossibility of interrupting the flow, but the relation between what we say about animals and what animals are like, is not that of gas with oil. In the case of primatology, animals are much more than the raw material of our knowledge about them. The more knowledge we have of them, the more visible they become. It is as if the pipeline was bidirectional providing more oil when we have more gas! It is because a scientist fabricates the fact that it becomes independent from his work --hence the puzzling double meaning of this little word "fact": what is made out, what is not made out. Clearly, this bizarre feature can't be sustained by the industrial metaphor.

Another way to better capture the practice of science is to consider preconceptions, biases, theories, methods, *a prioris*, culture, as so many roads that make it possible to gain access to the animals themselves. Surely, no researcher at Gombe, for instance, will call the tiny trails that lead to the field site a "bias". It does not stand "in between" the primatologist and his chimps. More exactly, it does stand in between, but instead of being what hampers the view of the chimp, as in the

optical metaphor, it is, without any doubt, also what allows the chimps to enter into view. The same is true of provisioning crates, clearings, sampling methods, statistical data reductions, binoculars, Jane Goodal's popular films, lectures given to the Leakey society, etc. All of these elements are in between, to be sure, but as so many indispensable mediations without which no knowledge would be produced at all. No one will call the tarmac on which planes are landing a "filter" that distorts what planes are supposed to be in themselves. The tarmac is, very commonly, what allows the plane to land...

The difference between the optical and the trail metaphor comes from the geometry and the position of the scientist. In the gaze paradigm, the observer is fixed and so is the thing to look at. It is a still-life and probably comes from a mistaken interpretation of classical paintings. In such a metaphor, any addition of an intermediary is taken as detrimental to the quality of the view. In the trail metaphor, on the contrary, the observer is not fixed, but moves toward the thing to be seen, itself always in movement, and the more work is done on the intermediary the better the data will be. In the latter metaphor, it is as if the vertical position of the successive filters, had been shifted 90° to turn them into a platform allowing the spectator to move on it. It was soon clear to us, during the meeting in Teresopolis, that all of the possible effects of theory, method and gender on our knowledge of primates would be evaluated differently if, instead of being what cut us off from the animals, it became what gave the animal an opportunity to be seen. The veils that until then had obscured the view of the animals, now became the red carpet allowing us an effortless walk towards them...

The trail metaphor is not without its defects, however, since it maintains the idea that knowledge is vision and that observer and observed are quite independant from the route they take. None of this captures the originality of Thelma Rowell's sentence. It is because she decided to treat sheep as chimps that they were lifted out of their condition of "boring sheep" and allowed the opportunity to demonstrate some intelligence. How can we explain this action of making something else visible? Either it is made, or it is visible, but how can it be made visible? How can we replace the passive resultant of the model we have now discarded by an action that seems to have contradictory features? To be sure, we could use the traditional vocabulary of fabrication and construction, but this might entail artificiality, invention, and even deception. If we say that facts are fabricated or constructed, we clearly imply, in the common parlance at least, that they have some innate vice which makes them forever unable to "fly". It seems that we have no way --in modern Western language at least (Jullien, 1995)-- to entertain the possibility of saying at once, in the same breath, fabricated thus autonomous. We are always

asked to choose "is it real?" or, on the contrary, "is it fabricated?" even though, in practice, we keep saying things like: "I tried to give my sheep the opportunity to behave like chimps".

One way out of this difficulty, might be to talk about propositions.[5] A little bit of philosophy is necessary at this juncture, and I apologize to my colleagues for this little excursus, but it is crucial for allowing me, in the next section, to find another way of discriminating between good and bad science. As everyone will admit, the goal is worthy of a little pain!

Propositions should not be limited to statements made of words uttered by a human "about" a natural thing. As the name indicates, they are offers made by an entity to relate to another under a certain perspective. Propositions are not limited to the human domain of language and consciousness. For instance Uexkull's canonical tick (see chapter by Hinde) can be considered as a proposition, as a certain way of inhabiting the world by eliciting in its multiplicity a tiny number of relevant traits. To use another philosophical word, one could say that the tick "offers an interpretation of the world". But so is a field site with its research assistants on mopeds, its focus sample method, its archives, its portable computers, etc. The field site inhabits the world in a certain way and establishes certain types of connections that will modify the others. The passage of any hot blooded animal will make the tick tick; the appearance of a new animal in the field site will make all the assistants suddenly attentive. A statement says in words what a thing is. A proposition designates a certain way of loading an entity into another by making the second attentive to the first, and by making both of them diverge from their usual path, their usual interpretation. A simple figure might help to grasp the abstract difference between a statement and a proposition.

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5 I have tried to work out this limit of the philosophy of action by devising the concept of "factishes" (Latour, 1996b). For one possible use of this notion in epistemology see (Stengers, 1996). The notion of proposition is a central key concept in Whiteheadian metaphysics (Whitehead, 1929 1978). It has close connection with the debates in (James, 1907) between "saltationist" and "deambulatory" conception of truth making.

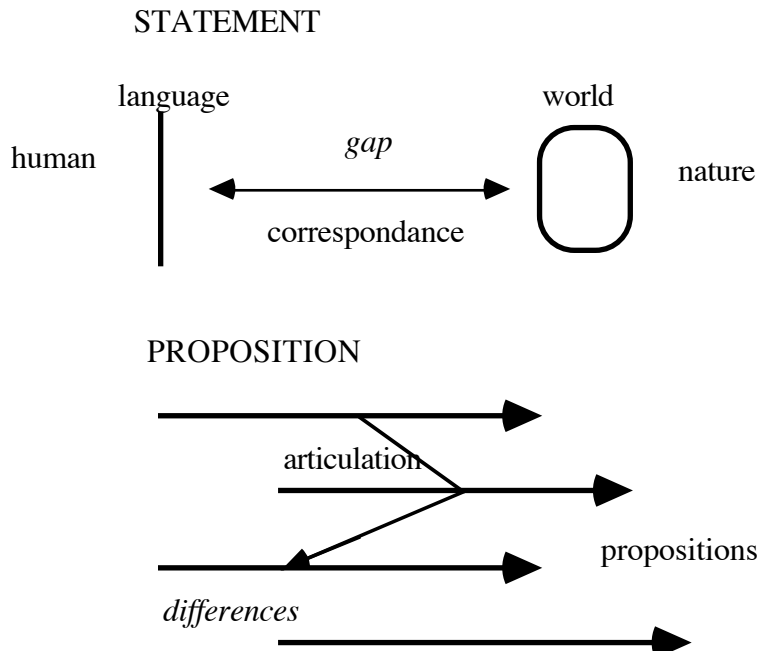

Figure 3

The difference between a statement in language about a reference and a proposition lies in the situation of the two concepts; in the second the difference between word and world is no longer pertinent

A statement pertains to the human language and is utterly separated by an unbridgeable gap from the things it talks about. There is always an abyss between words and world, human and objects. This gap may be bridged however, but by the very mysterious act of establishing a correspondance between words and world so as to provide the statement with a truth value. If, and only if, the cat is on the mat will the sentence "the cat is on the mat" be verified. But since between the dimensionless sentence "the cat is on the mat" and a three dimensional black furry cat on the mat there is no possible ressemblance, the correspondance is always tentative and the gap between the two may never be filled, no matter how many hundreds of thick tomes the philosophers of language have thrown into it. The word "dog" does not bark more than the word "cat" purrs. Because of their infinite distance with things --distance created artifically, for political reason, by the erasure of all the intermediary steps of data construction[6]-- statements are always running the risk of not corresponding to the world in an exact way, thus condemning the human locutor to life imprisonment locked away in the cell of language. Scepticism directly descends from this implausible definition of truth as correspondance between words and the world.

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6 For a more complete demonstration, see (Latour, 199-).

Propositions, on the other hand, do not pertain to language but to the world. This world, however, does not resemble a nature made of things foreign to human consciousness that had been made to stand like an enemy camp opposite the camp of humans. It is made of interpretations, or propositions sent to others so that they might behave differently. In between propositions there is thus not one gap but many differences. Meaning is not obtained by this very implausible correspondance between language and objects which have been made totally foreign to one another to begin with, but by connecting propositions which might or might not be slightly foreign to one another. While the notion of statements provides no possible avenue for the thing to be made relevant to what we say about it --except through the perillous footbridge of a correspondance between words and world--, the notion of propositions allows things to be loaded into words. Whereas a statement implies the existence of a talkative human surrounded by mute things, a proposition implies that we are made to speak in this way by what is talked about. To understand this very tricky point in a less abstract way, we need to turn to the third debate of the conference, the most fascinating and also the most difficult to elaborate.

Articulated or inarticulate propositions

After having circumvented the false debate to decide if primatology is or is not "socially constructed" and the rather sterile discussion on the role of method, theory and gender in "shaping our views" of primates, the conference could have gotten stuck in the traditionnal opposition between "nature" and "nurture", and all the more so given the feelings for and against sociobiology which were as intense as those triggered by science studies. This is where, from my point of view, the meeting was most rewarding. It seems that we avoided the usual pitfalls, by exemplifying in practice a new set of criteria to distinguish good from bad science that completely cuts accross the old tired distinction between biological or cultural determinism. It is this shibboleth that I want to render more explicit in the last section of this chapter.

I hope that Thelma Rowell will forgive me for analysing her assertion about her sheep in more detail, but it provides the essential clue for my demonstration. As I said above, statements are different from propositions. This is especially clear if we consider how we make judgments about their quality. Statements are true or false depending on whether or not there is a state of affairs corresponding to the statement --with all the difficulties outlined by the philosophy of language. I will propose to say, however, that propositions are good or bad depending on whether they are articulate or inarticulate. "Boring sheep are boring sheep" is an inarticulate proposition since it repeats tautologically what a sheep is, as if refusing to enter into

a connection with anything else. “Sheep are intelligent chimps” is an articulated proposition since it offers to establish a connection between two completely different entities that will give meaning to both: in the first sentence, sheep “are not given a chance”, as Thelma said; in the second, they “will be given an opportunity to behave differently”. The first sentence is a repetition --A is A. The second is, to use a philosophical term, a predication --A is B-- that is, something else, on which it now depends to gain its meaning.

The difference between articulate and inarticulate is not the same as between true and false. It is more like the difference between a music made of one note that remains at the same pitch and another that modulates the tone by shifting from one note to the other. Contrary to popular beliefs about science, it is very difficult to capture with some precision what scientists mean when they say that a piece of science is “interesting” and another “rubbish”. If dictionary science is made of statements that are simply true or false, science on the run, that is, research, is made of propositions that also have rythm, color, temperature and tempo. When a scientist says that a proposition is “interesting”, she does not only mean that it is accurate, but that it has a lot of other qualities as well: it can be warm, hot, surprising, fecund, productive, exciting... When we insist on the distinction between bad and good science, we not only distinguish between truth and falsity, but also between repetitive and articulate sentences. When we say that “data are meaningful”, we thus designate another type of circulation than the one between a referent out there and a statement in the language --top of figure 3. We seem to designates a very specific kind of movement among propositions that rearrange themselves into new and unexpected combinations --bottom of figure 3.

We now understand why the debate about “social construction” or “biases” was so fruitless. It was still connected with the linguistic and optical metaphors. The more intermediaries there were between the eye of the observer and the object --or between the statements of the scientists and the thing in itself-- the <u>least</u> direct, and thus, the least accurate was the correspondance and the quality of the information produced. Ideally, according to this view, if there were no intermediary at all, no complication whatsoever, the knowledge would be more complete. The situation is entirely different with propositions. The more activity there is, the more intermediaries there are, the <u>better</u> the chance to articulate meaningful propositions. The difference between settings is not between those where the scientists are inactive, remote, detached, disinterested, autonomous, and those where they are active, constructive, buzy fabricating, or being biased, full of *a prioris* and presuppositions, but between the settings where all the activity ends up producing repetitive data and those where the activity produce interesting articulations. Once

again, Thelma offers an excellent example when she castigates the farmers for constantly separating sheep from one another as soon as they demonstrate behaviour which is not sheepish and Panurgian. Farmers do not give the sheep a chance, Themla's set ups are unusual because she actively counteracts the farmer's customs, allowing the sheep to establish hierarchies and social relations wherin they stand a slightly better chance of being socially complex.

This case also nicely shows that propositions are in no way confined to language. The forms of life, in their entirety, in which farmers interact with sheep by selecting them for their docility and sheepishness, will not allow them, i.e. the farmers, to let them, i.e. the sheep, be talked about in any other way. Speech is not exactly a property of the human phonic apparatus, of the human inner subjectivity, of the human consciousness. It is more a property of the whole setting in which very heterogeneous elements have been gathered and connected: barns, enclosures, grass fields, wool in the farmers' form of life; barns, enclosures, grass fields, libraries, genes, papers, ethological meetings in Thelma's form of life. Whoever enters the farmer's setting will be made to say that boring sheep are boring sheep; whoever enters Thelma's quasi-laboratory will be made to say that sheep too may be "charismatic animals", in spite of the distance between primate primatologists and ruminants!

In practice, it is never the case that we utter statements by merely using the ressource of language and then only afterward check to see if there exist a corresponding thing that will verify or falsify our utterance. No one has ever began by saying the "cat is on the mat" and then turned to the proverbial cat to see whether or not it is sitting grandly on the proverbial mat. Our involvement in what we say is at once much more intimate and much more indirect than that of the traditionnal picture: we are allowed to say new things when we enter well articulated settings. Articulation between propositions is much deeper than speech. We speak because the propositions of the world are themselves articulated, not the other way around. More exactly, we are allowed to speak interestingly by what we allow to speak interestingly (Despret, 1996). The notion of articulated propositions establishes entirely different relations between knower and known than the traditional view, but it captures much more precisely the rich repertoire of scientific practice and is much better adapted to the reflexive task requested by the organizers' brief.

A simple diagram might clarify the shift from one shibboleth to the other. The traditionnal touchstone tries to distinguish scientific statements about primates from non-scientific statements. The first type is said to correspond to a state of affairs "out there", while the other will be elaborated by using only the resources provided at one time by the available stock of presuppositions, clichés, myths, *a prioris* or

paradigms, without the benefit of anything "out there". To be sure, it is a very useful dimension needed to sort out dictionary science. But it does not capture the constant evaluation going on among scientists engaged in doing research before the facts have been well established. The other touchstone, the one that is used in practice to separate out "interesting" from "boring" science, aims at distinguishing well articulated from inarticulate propositions. The key difference between the two dimensions, is that for a statement to be evaluated positively it is no longer enough to be simply scientific, it should also be well articulated. There is a lot of rubbish and non-scientific non-sense in primatology, everyone agrees on that, but there are also many impeccably scientific statements which are utterly useless because they are simply repetitive. Conversely, there are to be sure, many repetitive clichés in the non-scientific litterature about primates, but it is also perfectly possible that practical situations that have none of the characteristics of science provide decisive insights because they allow for a new articulation between original propositions. Community-based conservation offers many examples of a sudden modification into the knowledge produced about animals by people who do not wear white coats (Western and others, 1994) (Cussins, 199-). To be brushed off the sacred domain of science, it is no longer enough for a statement to be simply non-scientific, it must also be inarticulate and repetitive.

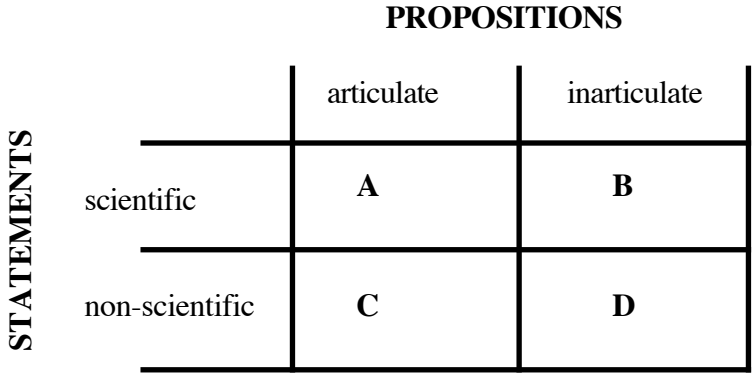

Figure 4

The classical distinction between scientific and non-scientific statement is not enough to capture the most important distinction between articulate and inarticulate propositions

How, if at all, can this new shibboleth bear on discussions around sociobiology? In what way could a different distinction between good and bad science have prevented us from falling into the third false debate between biological and cultural determinism? My gloss on the interactions during the conference is not that we hesitated between zoology and anthropology, determinism and history, necessity and contingence, nature and nurture, science and critique of science, naturalization

and historicization, naiveté and reflexivity --to use some of the common couple of oppositions--, but that we were opposing quite simply, the smallest number of active mediations to the greatest number of active entities. We were not trying to move from a naive science to a critical science, nor from a non-science to an eventually scientific outlook, nor from a pure science to a polluted science, but from a science made from a certain number of active entities to another made of a greater number of them.[7]

Let me give a few examples to illustrate this point. The difference between the classical picture of active spermatozoides trying to penetrate a passive egg, and the new picture provided by Tang-Martinez is not that the second is freed from gender biases while the former had been distorted by them, nor it is that the former was solely scientific while the second brings in external factors borrowed from the larger culture to "contaminate" the scientific facts of physiology. The difference is that the former leave a large number of entities inactive, simply transporting necessity, while the second generates at every point active entities that, in part, modify the causality exerted onto them by the other. An egg that actively selects out spermatozoides differs from a passive egg not because it is more "feminine" or because it is studied by a radical feminist, but because it does more things, it is composed of more elements, of more articles, of more mediations none of which can be reduced to a simple input and output blackbox. The same is true of Glickman's rats before and after the demise of Skinnerian psychology. The post-behaviourist rats are not more scientific than Skinner's rats. They do more things. They are made up of more elements. They are more articulate and so are the psychologists who use more diverse elements to talk about them. Simplification becomes harder; transports of necessity less easy. The same is true of "smart baboons", "charismatic chimps" and "Machiavellian animals". No one claims that they represent more scientifically what the animals are like --although it obviously does; nor does any one pretend that this is a pleasing story that is more in keeping with the prejudices of the age --although it obviously is. Smart animals are more active in their own behaviour and thus allow for more awareness on the part of the scientists studying them, forcing them to take more precautions, obliging them to become in turn more intelligent, more respectful.[8]

The point of this very crude indicator --number of active entities and number of active scientists-- is to point out that articulation was a much more important

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7 The question of why is it that the "greater number of active entities the better" cannot be tackled in the limit of this chapter since it depends on a further redefinition of the difference between science and politics. For a first effort go at it, see (Latour, 199-)

8 This extend, as (Despret, 1996) has so elegantly shown, to those people who watch primatologists or ethologists at work: intelligence, so to speak, is infectious -stupidity too...

element in the discussion than the type of entity each of us tried to deal with. That they pertained to nature or to culture, to environment or to physiology, to science or to the history of science, to genetics or feminist studies, mattered less than their degree of activity and their ability to reconfigure their inputs and outputs. This shibboleth can be applied to various elements, genes, hormones, physiology, brain waves, behavior, without forcing us to resort to the image of "levels" ——the inferior level being considered as more determinant than that preceding it. Reductionism is no longer an interesting issue. Thus, for instance, dealing with smart baboons who live in socially complex groups, does not mean that primatologists coming from anthropological departments will be unwilling to deal with genetic determinism. They will simply be wary of a certain type of genetics that connects elements so as to decrease the number of active mediators. Also, inside genetics the distinction between articulate and inarticulate propositions will sort out which parts of genetics are repetitive and simply scientific, and which parts are articulate and deal with a great many active "smart" genes and "smart" proteins, the pathways of which cannot be used for transporting an indisputable necessity.

Glickman's hyena enclosure at Berkeley offers a magnificent illustration of what it means to deal with articulation. Every possible discipline is brought in from endocrinology to ethology, from genetics to psychology, from anatomy to natural history, from media studies to the history of science, but not one of them is introduced to decrease the number of active entities elicited by another. Quite the contrary: every time a new discipline appears, a new active entity is made visible that complicates the straight path of another. Glickman is not trying to integrate all the disciplines, each of them dealing with a certain "level" and defining an inflexible type of necessity. Instead, he is forcing all of the disciplines brought to the Berkeley enclosure to reconfigure their definition of action at the occasion of new and puzzling features offered them by those hyenas to whom he has offered "the opportunity" to behave intelligently and to be made up of entities, whereby each of them can be described as slighlty smarter than before. "Off the shelf" endocrinology will not do the job any better, than "off the shelf" population genetics, or "off the shelf" history of popular misrepresentations of hyenas. Articulated propositions cannot be easily traversed by indisputable necessities. From this enclosure, where scientists are rendered smarter by smarter hyenas who at last escape the terrible fate of being despisable Disney-like hyenas, no simplification about determinism can escape.

The great advantage of this definition is that it also applies on the critical discourse bearing on primatology itself. When someone says that "women primatologists" see things differently because "they are women", or that "Japanese

scientists see things differently because they are Japanese", it can mean two different things that are easy to distinguish if we use the new touchstone. This introduction of an outside element might play exactly the same repetitive inarticulate role than the one I've used thus far as an example: "boring sheep are boring sheep", "women are women", "Japanese are Japanese". The new entity is introduced --sex, culture, etc-- not in order to elicit a new feature into the other, but so as to maintain the essentialist character of the causality that has been brought in. It gives no more critical edge to the proposition, than to say that "genes are genes" or that "Westerners always see things as Westerners". Tautology is always a tautology, no matter if it comes from the inside of science or from the outside, no matter if it deals with nature or culture, from ultimate or proximate mechanism.

But these new features may also mean something entirely different. By bringing in women scientists or Japanese researchers, this new original standpoint will introduce a difference that will lead away from the standpoint. Once again, it is Thelma Rowell who provides the best example of this shift, when to ridicule the notion of standpoint she said in passing: "If a female scientists studies female baboons and sticks to them she will end up studying males because female baboons are very much interested in males!". That's exactly the quality of a standpoint: they allow movement in a different way than what was intended. Standpoints never stay still! Because of the new attention given by female researchers to female baboons, a new attention will be given to males that differs entirely from the original focus on the domineering males, and that differs also from the focus on the female which was originally intended. When we contend that primatology "betrays" many Western cultural biases about animals, monkeys, apes or Dark Africa, that's exactly it: primatology betrays these original standpoints by turning them into something completely different. What is true for the gene, for hormone levels, for aggression is also true for history and sociology of science: the carrying over of undisputable necessity is always less interesting, and in the end, less scientific than the revelation of active mediators all the way down.

If I am allowed some Gallic exageration, I would be tempted to say that the meeting in Teresopolis had an historical significance. Too often, scientists believe that their science will be better served if they ignore as much as possible all the untidy connections that make it work, with which they deal on a daily basis. They might be gathered to reflexively evaluate their discipline, but this evaluation, in their eyes, can only be carried out by using an off-the-shelf philosophy of science that dates back sixty years --to be charitable. The aim of such reflexive gathering could only be to purify the discipline ever more from the last remnants of adherence to

subjectivity, politics, mythology, ideology or biases. Shirley and Linda led us along an entirely different trail, which I have tried to map in dotted lines using my own system of projection. What would happen to the collective understanding of a discipline, if scientists were no longer trying to extirpate themselves from the sin of being connected, but accepted the vascularization as so many positive features that would turn their science into a well articulated one? Primatology would not only be crucially important as a trading zone between anthropology, zoology, evolutionary theory, ethics, conservation and ecology, but also an exemplary site for the renewal of philosophy of science.

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