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Give Me a Laboratory and I Will Raise the World

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In this chapter, I would like to propose a simple line of enquiry: that is, to stick with the methodology developed during laboratory field studies, focusing it not on the laboratory itself but on the construction of the laboratory and its position in the societal milieu (Callon, 1982). Indeed, I hope to convince the reader that the very difference between the "inside" and the "outside," and the difference of scale between "micro" and "macro" levels, is precisely what laboratories are built to destabilize or undo. So much so, that without keeping back the discoveries we made while studying laboratory practices we can reassess the so-called "macro" problems much more clearly than before and even throw some light on the very construction of macroactors themselves. I simply beg the readers to put aside for a time their belief in any real difference between micro- and macroactors at least for the reading of this paper (Callon and Latour, 1981).

I. "GIVE ME A PLACE TO STAND AND I WILL MOVE THE EARTH"

To illustrate my argument I will extract an example from a recent study done in the history of science (Latour, 1988). We are in the year 1881, the French semipopular and scientific press is full of articles about the work being done in a certain laboratory, that of Monsieur Pasteur at the École Normale Supérieure. Day after day, week after week, journalists, fellow scientists, physicians, and hygienists focus their attention on what is happening to a few colonies of microbes in different mediums, under the microscope, inside inoculated animals, in the hands of a few scientists. The mere existence of this enormous interest shows the irrelevance of too sharp a distinction between the "inside" and the "outside" of Pasteur's lab. What is relevant is the short circuit established between many groups usually uninterested by what happens inside laboratory walls, and laboratories usually isolated and insulated from such attention and passion. Somehow, something is happening in these dishes that seems directly essential to the projects of these many groups expressing their concern in the journals.

This interest of outsiders for lab experiments is not a given: it is the result of Pasteur's work in enrolling and enlisting them. This is worth emphasizing since there is a quarrel among sociologists of science about the possibility of imputing interests to people. Some, especially the Edinburgh school, claim that we can impute interests to social groups given a general idea of what the groups are, what society is made of, and even what the nature of man is like. But others (Woolgar, 1981) deny the possibility of such imputation on the grounds that we do not have any independent way of knowing what the groups are, what society is after, and what the nature of man is like. This dispute, like most, misses the fundamental point. Of course there is no way of knowing which are the groups, what they want, and what man is, but this does not stop anyone from convincing others of what their interests are and what they ought to want and to be. He who is able to translate others' interests into his own language carries the day. It is especially important *not* to rely on any science of society or science of man to impute interests because, as I will show, sciences are one of the most convincing tools to persuade others of who they are and what they should want. A sociology of science is crippled from the start if it believes in the results of one science, namely sociology, to explain the others. But it is still possible to follow how sciences are used to transform society and redefine what it is made of and what are its aims. So it is useless to look for the profit that people can reap from being interested in Pasteur's laboratory. Their interests are a consequence and not a cause of Pasteur's efforts to translate what they want or what he makes them want. They have no a priori reason to be interested at all, but Pasteur has found them more than one reason.

1. MOVE ONE: CAPTURING OTHERS' INTERESTS

How has Pasteur succeeded in capturing the interests of other indifferent groups? By the same method he has always used (Geison, 1974; Salomon-Bayet, 1982). He transfers himself and his laboratory into the midst of a world untouched by laboratory science. Beer, wine, vinegar, diseases of silk worms, antisepsy and later asepsy, had already been treated through these moves. Once more he does the same with a new problem: anthrax. The anthrax disease was said to be terrible for French cattle. This "terrible" character was "proven" by statistics to officials, veterinarians, and farmers, and their concerns were voiced by the many agricultural societies of the time. This disease was studied by statisticians and veterinarians, but laboratory practice had no bearing on it before Pasteur, Koch, and their disciples. At the time, diseases were local events that were to be studied with all possible attention by taking into account all the possible variables—the soil, the winds, the weather, the farming system, and even the individual fields, animals, and farmers. Veterinary doctors knew these idiosyncrasies, but it was a careful, variable, prudent, and uncertain knowledge. The disease was unpredictable, and recurred according to no clear pattern, reinforcing the idea that local idiosyncrasies had to be taken into account. This multifactorial approach made everyone extremely suspicious of any attempt to cut through all these idiosyncrasies and to link one disease with any single cause, such as a microorganism. Diseases like anthrax, with all their variations, were typically what was thought not to be related to laboratory science. A lab in Paris and a farm in Beauce have nothing in common. They are mutually uninteresting.

But interests, like everything else, can be constructed. Using the work of many predecessors who had already started to link laboratories and anthrax disease, Pasteur goes one step further and works in a makeshift laboratory right on the farm site. No two places could be more foreign to one another than a dirty, smelly, noisy, disorganized nineteenth-century animal farm and

the obsessively clean Pasteurian laboratory. In the first, big animals are parasited in seemingly random fashion by invisible diseases; in the second, microorganisms are made visible to the observer's eye. One is made to grow big animals, the other to grow small animals. Pasteur (the "shepherd" in French) is often seen in the enthusiasm of the moment as the inventor of a new animal husbandry and a new agriculture, but at the time these two forms of livestock have little relation to one another. Once out in the field, however, Pasteur and his assistants learn from the field conditions and the veterinarians and start creating these relations. They are interested in pinpointing all the variations in the onset and timing of the outbreaks of anthrax and in seeing how far these could fit with their one living cause, the anthrax bacillus. They learn from the field, translating each item of veterinary science into their own terms so that working on their terms is also working on the field. For instance, the spore of the bacillus (shown by Koch) is the translation through which dormant fields can suddenly become infectious even after many years. The "spore phase" is the laboratory translation of the "infected field" in the farmer's language. The Pasteurians start by learning this language and giving one of their own names for each of the relevant elements of the farmer's life. They are interested in the field but still useless and uninteresting for the farmers and their various spokesmen.

2. MOVE TWO: MOVING THE LEVERAGE POINT FROM A WEAK TO A STRONG POSITION

At this point Pasteur, having situated his laboratory on the farm, is going to transfer it back to his main workplace at the École Normale Supérieure, taking with him one element of the field, the cultivated bacillus. He is the master of one technique of farming that no farmer knows, microbe farming. This is enough to do what no farmer could ever have done: grow the bacillus in isolation and in such a large quantity that, although invisible, it becomes visible. Here again we have, because of laboratory practice, a variation of scale: outside, in the "real" world, inside the bodies, anthrax bacilli are mixed with millions of other organisms with which they are in a constant state of competition. This makes them doubly invisible. However, in Pasteur's laboratory something happens to the anthrax bacillus that never happened before (I insist on these two points: something happens *to the bacillus* that *never* happened before). Thanks to Pasteur's methods of culture it is freed from all competitors and so grows exponentially, but, by growing so much, ends up, thanks to Koch's later method, in such large colonies that a clear-cut pattern is made visible to the watchful eye of the scientist. The latter's skills are not miraculous. To achieve such a result you only need to extract one microorganism and to find a suitable milieu. Thanks to these skills, the asymmetry in the scale of several phenomena is modified: a microorganism can kill vastly larger cattle; one small laboratory can learn more about pure anthrax cultures than anyone before; the invisible microorganism is made visible; the until now uninteresting scientist in his lab can talk with more authority about the anthrax bacillus than veterinarians ever have before.

The translation that allows Pasteur to transfer the anthrax disease to his laboratory in Paris is not a literal, word-for-word translation. He takes only one element with him, the microorganism, and not the whole farm, the smell, the cows, the willows along the pond, or the farmer's pretty daughter. With the microbe, however, he also draws along with him the now interested agricultural societies. Why? Because having designated the microorganism as the living and pertinent cause, he can now reformulate farmers' interests in a new way: if you wish to solve *your* anthrax problem you have to pass through *my* laboratory first. Like all translations there is a real

displacement through the various *versions*. To go straight at anthrax, you should make a detour through Pasteur's lab. The anthrax disease *is* now at the École Normale Supérieure.

But this version of the translation is still a weak one. In Pasteur's lab, there is a microbe, but anthrax infection is too disorderly a thing to be explained with a single cause only. So the outside interests could as well say that the laboratory has no real bearing on the spread of anthrax disease, and that it is just plain arrogance for a scientist to claim that he holds the key to a real disease "out there." But Pasteur is able to make a more faithful translation than that. Inside the walls of his laboratory, he can indeed inoculate animals he has chosen with pure, much-diluted culture of anthrax. This time, the outbreak of an epizootic is mimicked on a smaller scale entirely dominated by the charting and recording devices of the Pasteurians. The few points deemed essential are imitated and reformulated so as to be scaled down. The animals die of the microbes, and only of that, and epizootics are started at will. It can now be said that Pasteur has inside his laboratory, on a smaller scale, the "anthrax disease." The big difference is that "outside" it is hard to study because the microorganism is invisible and strikes in the dark, hidden among many other elements, while "inside" the lab clear figures can be drawn about a cause that is there for all to see, due to the translation. The change of scale makes possible a reversal of the actors' strengths; "outside" animals, farmers, and veterinarians were *weaker* than the invisible anthrax bacillus; inside Pasteur's lab, man becomes stronger than the bacillus, and as a corollary, the scientist in his lab gets the edge over the local, devoted, experienced veterinarian. The translation has become more credible and now reads: "If you wish to solve your anthrax problem, come to my laboratory, because that's where the forces are reversed. If you don't (veterinarians or farmers), you will be eliminated."

But even at this point, the strength is so disproportionate between Pasteur's single lab and the multiplicity, complexity, and economic size of the anthrax outbreaks, that no translation could last long enough to keep the aggregation of interest from falling apart. People readily give their attention to someone who claims that he has the solution to their problems but are quick to take it back. Especially puzzling for all practitioners and farmers, is the *variation* of the disease. Sometimes it kills, sometimes not, sometimes it is strong, sometimes weak. No contagionist theory can account for this variety. So Pasteur's work, although interesting, could soon become a curiosity or more precisely, a laboratory curiosity. It would not be the first time that scientists attract attention, only to have nothing come out of it in the end. Microstudies remain "micro," the interests captured for a time soon go to other translations from groups that succeed in enrolling them. This was especially true of medicine which at the time was tired of continuous fashions and fads (Leonard 1977).

But here Pasteur does something on chicken cholera and on anthrax bacillus inside his laboratory that definitively modifies the hierarchy between veterinary science and microbiology. Once a great many microbes are cultivated in pure forms in laboratories and submitted to numerous trials to make them accelerate their growth or die, a now practical know-how is developed. In a few years, experimenters acquire skills in manipulating sets of materials that never existed before. This is new but not miraculous. Training microbes and domesticating them is a craft like printing, electronics, blue-ribbon cooking, or video art. Once these skills have accumulated inside laboratories, many crossovers occur that had no reason to occur anywhere else before. This is not because of any new cognitive attitude, or because suddenly people become conscious of microorganisms they were unaware of before. It is simply that they are manipulating new objects and so acquiring new skills in a new idiosyncratic setting (Knorr, 1981).

The chance encounter that made possible the first attenuated culture of chicken cholera is well known (Geison, 1974), but chance favors only well-prepared laboratories. Living causes of man-made diseases undergo so many various trials that it is not that surprising if some of these trials leave some microbes alive but weak. This modification would have been invisible if the laboratory had not tried to imitate the salient features of epizootics by inoculating many animals. The invisible modification of the invisible microbes is then made visible; chickens previously inoculated with the modified strain don't get cholera, but they resist inoculation of intact microbes. Submitting cultures of chicken cholera to oxygen is enough to make them less virulent when they are inoculated into the animals. What is made visible through the lab statistics is the chain of weakened microbes, then strengthened microbes and eventually, strengthened animals. The result is that laboratories are now able to imitate the *variation of virulence.*

It is important to understand that Pasteur now does more and more things inside his laboratory which are deemed relevant by more and more groups to their own interests. Cultivating the microbes was a curiosity; reproducing epizootics in labs was interesting; but varying at will the virulence of the microbes is fascinating. Even if they believed in contagion, no one could with this one cause explain the randomness of the effects. But Pasteur is not only the man who has proved the relation of one microbe/one disease, he is also the one who has proved that the infectiousness of microbes could vary under conditions that could be controlled, one of them being, for instance, a first encounter of the body with a weakened form of the disease. This variation in the laboratory is what makes the translation hard for others to dispute: the variation was the most puzzling element that previously justified the skepticism towards laboratory science, and made necessary a clear differentiation between an outside and inside, between a practical level and a theoretical level. But it is precisely this variation that Pasteur can imitate most easily. He can attenuate a microbe; he can, by passing it through different species of animals, on the contrary, exalt its strength; he can oppose one weak form to a strong one, or even one microbial species to another. To sum up, he can do inside his laboratory what everyone tries to do outside, but where everyone fails because the scale is too large, Pasteur succeeds because he works on a small scale. Hygienists who comprise the largest relevant social movement of that time are especially fascinated by this imitated variation. They deal with whole cities and countries, trying to pinpoint why winds, soil, climates, diets, crowding, or different degrees of wealth accelerate or stop the evolution of epidemics. They all see—they are all led to see—in the Pasteurian microcosmos what they are vainly trying to do at the macroscopic level. The translation is now the following: "If you wish to understand epizootics and soon thereafter epidemics, you have one place to go, Pasteur's laboratory, and one science to learn that will soon replace yours: microbiology."

As the reader is aware, I am multiplying the words "inside" and "outside," "micro" and "macro," "small scale" and "large scale," so as to make clear the destabilizing role of the laboratory. It is through laboratory practices that the complex relations between microbes and cattle, the farmers and their cattle, the veterinarians and the farmers, the veterinarians and the biological sciences, are going to be transformed. Large interest groups consider that a set of lab studies talk to them, help them, and concern them. The broad concerns of French hygiene and veterinary sciences will be settled, they all say, inside Pasteur's laboratory. This is the dramatic short circuit I started with: everyone is interested in lab experiments which a few years before had not the slightest relation to their fields. This attraction and capture were made by a double movement of Pasteur's laboratory to the field and then from the field to the laboratory where a fresh source of know-how has been gained by manipulating a new material: pure cultures of microbes.

3. MOVE THREE: MOVING THE WORLD WITH THE LEVER

But even at this stage, what was in the laboratory could have stayed there. The macrocosmos is linked to the microcosmos of the laboratory, but a laboratory is never bigger than its walls and "Pasteur" is still only one man with a few collaborators. No matter how great the interests of many social groups for what is being done in one laboratory, there is nothing to stop interests from fading and dispersing if nothing more than laboratory studies happens. If Pasteur stays too long inside his laboratory and, for instance, shifts his research programme using the anthrax microbe to learn things in biochemistry, like his disciple Duclaux, people could say: "Well after all, it was just an interesting curiosity!" It is only by hindsight that we say that in this year 1881 Pasteur invented the first artificial vaccination. By doing so we forget that to do so it was necessary to move still further, this time from the laboratory to the field, from the microscale to the macroscale. As for all translations it is possible and necessary to distort the meanings but not to betray them entirely. Groups that accepted to pass through Pasteur's hands in order to solve their problems nevertheless only go through him to their own ends. They cannot stop in his laboratory.

Pasteur, from the start of his career, was an expert at fostering interest groups and persuading their members that their interests were inseparable from his own. He usually achieved this fusion of interests (Callon, 1981) through the common use of some laboratory practices. With anthrax he does just that but on a more grandiose scale, since he is now attracting the attention of groups that are the mouthpiece of larger social movements (veterinary science, hygiene, soon medicine), and about issues that are the order of the day. As soon as he has performed vaccinations in his laboratory he organizes a field trial on a larger scale.

This field experiment was organized under the auspices of the agricultural societies. Their attention had been captured by Pasteur's former moves, but the translation ("solve your problems through Pasteur's lab") implied that *their* problems could be solved and not only Pasteur's. So the translation is also understood in part as a contract, the counterpart of which is now expected from Pasteur. "We are ready to displace all our interests through your methods and practices so that we can use them to reach our own goals." This new translation (or displacement) is as hard to negotiate as the first one. Pasteur has a vaccine for anthrax in his laboratory at Paris. But how can laboratory practice be extended? In spite of all the niceties written by epistemologists on that point, the answer is simple: only by extending the laboratory itself. Pasteur cannot just hand out a few flasks of vaccine to farmers and say: "OK, it works in my lab, get by with that." If he were to do that, it would *not* work. The vaccination can work only on the condition that the farm chosen in the village of Pouilly le Fort for the field trial be in some crucial respects transformed according to the prescriptions of Pasteur's laboratory. A hard negotiation ensues between Pasteurians and agricultural interests on the conditions of the experiment. How many inoculations? Who will be the umpire? And so on. This negotiation is symmetrical to the initial one when Pasteur came to the farm site, trying to extract the few pertinent elements of the disease that he could imitate inside his laboratory. Here, the problem is to find a compromise that extends Pasteur's laboratory far enough—so that the vaccination can be repeated and work—but which is still acceptable to the farming representatives so that it is seen as an extension of lab science outside. If the extension is overreached, the vaccination will fail and Pasteur will be thrown back inside his laboratory by the disappointed farmers. If the extension is too modest, the same thing will happen: Pasteur will be considered to be a lab scientist uninteresting for others' outside use.

The Pouilly le Fort field trial is the most famous of all the dramatic proofs that Pasteur staged in his long career. The major mass media of the time were assembled on three successive occasions to watch the unfolding of what was seen as Pasteur's prediction. "Staging" is the right word because, in practice, it is the public showing of what has been rehearsed many times before in his laboratory. It is strictly speaking a repetition, but this time in front of an assembled public which has previously invested so much interest and is now expecting its rewards. Even the best performer has stage fright, even if everything has been well rehearsed. Indeed this is what happened (Geison, 1974). But for the media it was not seen as a performance, it was seen as a prophecy. The reason behind this belief shows us exactly why the distinction between inside and outside of the laboratory is so misleading. If you isolate Pasteur's laboratory from the Pouilly le Fort farm, so that one is the inside and the other is the outside world, then of course there is a miracle for all to see. In his lab Pasteur says, "all vaccinated animals will be alive by the end of May; all the untreated animals will have died by the end of May; and outside the lab the animals die or survive." Miracle. Prophecy, as good as that of Apollo. But if you watch carefully the prior displacement of the laboratory to capture farmers' interest, then to learn from veterinary sciences, then to transform the farm back into the guise of a laboratory, it is still interesting, extraordinarily clever, and ingenious, but it is *not* a miracle. I will show later that most of the mystified versions of scientific activity come from overlooking such displacements of laboratories.

But there is still one step to make so that we reach our point of departure: the anthrax outbreaks and their impact on French agriculture. Remember that I said it was a "terrible" disease. While saying this I heard my ethnomethodologist friends jumping on their chairs and screaming that no analyst should say that "a disease is terrible" or that "French agriculture" exists, but rather that these are social constructions. Indeed they are. Watch now how the Pasteur group is going to use these constructions to their advantage and to France's. Pouilly le Fort was a staged experiment to convince the investors—in confidence and later in money—that the translation made by Pasteur was a fair contract. "If you want to solve your anthrax problem go through my microbiology." But after Pouilly le Fort, everyone is convinced that the translation is now: "If you want to save your animals from anthrax, order a vaccine flask from Pasteur's laboratory, École Normale Supérieure, rue d'Ulm, Paris." In other words, on the condition that you respect a limited set of laboratory practices—disinfection, cleanliness, conservation, inoculation gesture, timing, and recording—you can extend to every French farm a laboratory product made at Pasteur's lab. What was at first a capture of interests by a lab scientist is now extending through a network much like a commercial circuit—not quite since Pasteur sends his doses free of charge—that spreads laboratory products all over France.

But is "all over France" a social construction? Yes indeed; it is a construction made by statistics-gathering institutions. Statistics is a major science in the nineteenth century, and is what "Pasteur," now the label for a larger crowd of Pasteurians, is going to use to watch the spread of the vaccine, and to bring to the still uncertain public a fresh and more grandiosely staged proof of the efficacy of the vaccine. Throughout France as it is geographically marked out by its centralized bureaucracy, one can register on beautifully done maps and diagrams the decrease of anthrax wherever the vaccine is distributed. Like an experiment in the Pasteur lab, statisticians inside the offices of the agricultural institutions are able to read on the charts the decreasing slopes that mean, so they say, the decrease of anthrax. In a few years, the transfer of the vaccine produced in Pasteur's lab to all farms was recorded in the statistics as the cause of the decline of anthrax. Without

these statistical institutions it would of course have been utterly impossible to say whether the vaccine was of any use, as it would have been utterly impossible to detect the existence of the disease to begin with. We have now reached the point we started from. French society, in some of its important aspects, has been transformed through the displacements of a few laboratories.

II. TOPOLOGY OF LABORATORY POSITIONING

I have chosen one example but many could be found in Pasteur's career and I am confident that every reader has many more of these in mind. The reason why we do not acknowledge these many examples is to be found in the way we treat science. We use a model of analysis that respects the very boundary between micro- and macroscale, between inside and outside, that sciences are designed to not respect. We all see laboratories but we ignore their construction, much like the Victorians who watched kids crawling all over the place, but repressed the vision of sex as the *cause* of this proliferation. We are all prudish in matters of science, social scientists included. Before drawing some general conclusions about laboratories in the third part, let me propose a few concepts that would make us become less prudish and would help to liberate all the information that we cannot help having.

1. DISSOLUTION OF THE INSIDE/OUTSIDE DICHOTOMY

Even in the brief outline given above, the example I have chosen is enough to show that, at worst, the categories of inside and outside are totally shaken up and fragmented by lab positioning. But what word can be used that could help us to describe what happened, including this reversion leading to the breaking down of inside/outside dichotomies? I have used several times the words "translation" or "transfer," "displacement" or "metaphor," words that all say the same thing in Latin, Greek, or English (Serres, 1974; Callon, 1975). One thing is sure throughout the story told above: every actor you can think of has been to some extent *displaced* (Armatte, 1981). Pasteur's lab is now in the middle of agricultural interests with which it had no relation before; in the farms an element coming from Paris, vaccine flasks, has been added; veterinary doctors have modified their status by promoting "Pasteur's" science and the vaccine flasks: they now possess one more weapon in their black bags; and sheep and cows are now freed from a terrible death: they can give more milk and more wool to the farmer and be slaughtered with greater profit. In McNeil's terms (McNeil, 1976), the displacement of microparasites allows the macroparasites—here the farmers—to grow fatter by feeding off healthier cattle. By the same token all the macroparasitic chain of tax collectors, veterinarians, administrators, and landlords prosper by feeding off the richer farmers (Serres, 1980). One last element is pushed out—the anthrax bacillus. Wherever the veterinarian comes the small parasite has to go. In this succession of displacements, no one can say *where the laboratory is* and *where the society is*. Indeed the question "Where?" is an irrelevant one when you deal with *displacements* from a lab in Paris to some farms then back to Paris, drawing along with it the microbes and the farmers' interests; then to Pouilly le Fort where an extended repetition is staged, then to the whole agricultural system through statistics and bureaucracy. But it is clear that the situation of the farms after the moves is not the same as before. Through the leverage point of the lab, which is a moment in a dynamic process, the farm system has been displaced. It now

includes a routine annual gesture, part of which used to be a laboratory practice and still is a lab product. Everyone has changed, including the "whole society," to use common terms. This is why I used in the title a parody of Archimedes' famous motto: "give me a laboratory and I'll move the earth." This metaphor of the lever to move something else is much more in keeping with observation than any dichotomy between a science and a society. In other words, it is the same set of forces that drives people inside Pasteurian labs to strengthen microbiology and outside to stage the Pouilly le Fort experiment or to modify French agriculture. What we will have to understand later is why in this *moment* the laboratory gains strength to modify the state of affairs of all the other actors.

Another reason why the inside/outside notion is irrelevant, is that in this example the laboratory positions itself precisely so as to reproduce inside its walls an event that seems to be happening only outside—the first move—and then to extend outside to all farms what seems to be happening only inside laboratories. As in some topological theorem, the inside and the outside world can reverse into one another very easily. Naturally, the three relations outside, inside, outside again, are in no way identical. Only a few elements of the macroscopic epizootics are captured in the lab, only controlled epizootics on experimental animals are done in the lab, only specific inoculation gestures and vaccine inoculants are extracted out of the lab to be spread to farms. That this metaphorical drift, which is made of a succession of displacements and changes of scale (see below), is the source of all innovations is well known (Black, 1961). For our purpose here, it is enough to say that each translation from one position to the next is seen by the captured actors to be a faithful translation and not a betrayal, a deformation, or something absurd. For instance, the disease in a Petri dish, no matter how far away from the farm situation, is seen as a faithful translation, indeed *the* interpretation of anthrax disease. The same thing is true when hygienists see as equivalent the trials microbes undergo in Pasteur's lab, and the variations of epidemics that masses of people undergo in a large city like Paris. It is useless trying to decide if these two settings are really equivalent—they are not, since Paris is not a Petri dish—but they are deemed equivalent by those who insist that if Pasteur solves his microscale problems the secondary macroscale problem will be solved. The negotiation on the equivalence of nonequivalent situations is always what characterizes the spread of a science, and what explains, most of the time, why there are so many laboratories involved every time a difficult negotiation has to be settled.

For the vaccine to be effective, it has to spread outside in the "real world out there," as people say. This is what best shows the absurdity of the dichotomy between inside/outside and the usefulness of microstudies of science in understanding macroissues. Most of the difficulties associated with science and technology come from the idea that there is a time when innovations are in laboratories, and another time when they are tried out in a new set of conditions which invalidate or verify the efficacy of these innovations. This is the "adequatio rei et intellectus" that fascinates epistemologists so much. As this example shows, the reality of it is more mundane and less mystical.

First, the vaccine works at Pouilly le Fort and then in other places only if in all these places the same laboratory conditions are extended there beforehand. Scientific facts are like trains, they do not work off their rails. You can extend the rails and connect them but you cannot drive a locomotive through a field. The best proof of this is that every time the method of extension of the anthrax vaccine was modified, the vaccine did *not* work and Pasteur got bogged down in bitter controversy, for instance with the Italians (Geison, 1974). His answer was always to check, and see if everything was done according to the prescriptions of his lab. That the same thing can

be repeated does not strike me as miraculous, but it does seem to be for all the people who imagine that facts get out of laboratories without the extension of lab practices.

But there is a second reason why the laboratories have no outside. The very existence of the anthrax disease in the first place, and the very efficacy of the vaccine at the end of the story, are not "outside" facts given for all to see. They are, in both cases, the result of the prior existence of statistical institutions having built an instrument (statistics in this case), having extended their network through the whole French administration so as to gather data, and having convinced all the officials that there was a "disease," a "terrible" one, and that there was a "vaccine," an "efficient" one. Most of the time when we talk about the outside world *we are simply taking for granted the prior extension of a former science* built on the same principle as the one we are studying. This is why lab studies in the end hold the key to the understanding of macroproblems, as I will show at the end of this chapter.

2. PLAYING HAVOC WITH DIFFERENCES OF SCALE

But if the inside/outside dichotomy does not hold true, what are we going to say about differences of scale which, the reader should be reminded, are at the origin of many discussions in sociology of science, since it is because of this belief in differences of scale that microstudies are accused of missing some essential points? In the example I sketched out above, we are never confronted with a social context on one hand and a science, laboratory, or individual scientist on the other. We do *not* have a context influencing, or not influencing, a laboratory immune from social forces. This view, which is the dominant view among most sociologists, is exactly what is untenable. Of course, many good scholars like Geison could show why the fact that Pasteur is a Catholic, a conservative, a chemist, a Bonapartist, etc., do count (Farley and Geison, 1979). But this sort of analysis, no matter how careful and interesting, would entirely miss the main point: *in his very scientific work, in the depth of his laboratory, Pasteur actively modifies the society of his time and he does so directly*—not indirectly—*by displacing some of its most important actors.*

Here again Pasteur is a paradigmatic example. As a politician he failed so completely that he was unable to get more than a few votes the few times he tried to get elected senator. But he has along with Carnot, and the Republic itself, the greatest number of streets bearing his name in all French villages and towns. This is also a nice symbol of the studies about Pasteur. If you look for examples of his "politicking" politics, you will of course find them but they are poor, disappointing, and never in keeping with the importance of his scientific work. The poverty of your findings will make readers say that "there is something else in Pasteur, in his scientific achievements, that escapes all social or political explanation." People who would utter this cliché would indeed be right. A poor critical explanation always protects science. This is why the more radical scientists write against science, the more science is mystified and protected.

To study Pasteur as a man acting on society, it is not necessary to search for political drives, for some short-term monetary or symbolic profits, or for long-term chauvinistic motives. It is no use looking for unconscious ideologies or devious drives (drives which, by some mystery, are clear only to the analyst's eyes). It is no use muckraking. You just have to look at what he does in his laboratory as a scientist. To summarize a long study in a nutshell (Latour, 1988), Pasteur adds to all the forces that composed French society at the time a new force for which he is the only credible spokesman—the microbe. You cannot build economic relations without this "tertium quid" since the microbe, if unknown, can bitter your beer, spoil your wine, make the mother of your vinegar sterile, bring back cholera with your goods, or kill your factotum sent to India. You

cannot build a hygienist social movement without it, since no matter what you do for the poor masses crowded in shanty towns, they will still die if you do not control this invisible agent. You cannot establish even innocent relations between a mother and her son, or a lover and his mistress, and overlook the agent that makes the baby die of diphtheria and has the client sent to the mad house because of syphilis. You do not need to muckrake or look for distorted ideologies to realize that a group of people, equipped with a laboratory—the only place where the invisible agent is made visible—will easily be situated everywhere in all these relations, wherever the microbe can be seen to intervene. If you reveal microbes as essential actors in all social relations, then you need to make room for them, and for the people who show them and can eliminate them. Indeed the more you want to get rid of the microbes, the more room you should grant Pasteurians. This is not false consciousness, this is not looking for biased world views, this is just what the Pasteurians *did* and the way they were *seen* by all the other actors of the time.

The congenital weakness of the sociology of science is its propensity to look for obvious stated political motives and interests in one of the only places, the laboratories, where sources of fresh politics as yet unrecognized as such are emerging. If by politics you mean elections and law, then Pasteur, as I have said, was not driven by political interests, except in a few marginal aspects of his science. Thus his science is protected from enquiry and the myth of the autonomy of science is saved. If by politics you mean to be the spokesman of the forces you mould society with and of which you are the only credible and legitimate authority, then Pasteur is a fully political man. Indeed, he endows himself with one of the most striking fresh sources of power ever. Who can imagine being the representative of a crowd of invisible, dangerous forces able to strike anywhere and to make a shambles of the present state of society, forces of which he is by definition the only credible interpreter and which only he can control? Everywhere Pasteurian laboratories were established as the only agency able to kill the dangerous actors that were until then perverting efforts to make beer, vinegar, to perform surgery, to give birth, to milk a cow, to keep a regiment healthy, and so on. It would be a weak conception of sociology if the reader were only to say that microbiology "has an influence" or "is influenced by the nineteenth-century social context." *Microbiology laboratories are one of the few places where the very composition of the social context has been metamorphosed.* It is not a small endeavour to transform society so as to include microbes and microbe-watchers in its very fabric. If the reader is not convinced, then he can compare the sudden moves made at the same time by socialist politicians, talking on behalf of another crowd of new, dangerous, undisciplined, and disturbing forces for whom room should be made in society: the laboring masses. The two powers are comparable in this essential feature: they are fresh sources of power for modifying society and cannot be explained by the state of the society at the time. Although the two powers were mixed together at the time (Rozenkranz, 1972), it is clear that in political terms the influence of Pasteurian laboratories reached further, deeper, and more irreversibly since they could intervene in the daily details of life—spitting, boiling milk, washing hands—and at the macroscale—rebuilding sewage systems, colonizing countries, rebuilding hospitals—without ever being clearly seen as a stated political power.

This transformation of what is the very composition of society can in no way be defined through distinctions of scales and of levels. Neither the historian nor the sociologist can distinguish the macrolevel of French society and the microlevel of the microbiology laboratory, since the latter is helping to redefine and displace the former. The laboratory positioning, as I insisted on earlier, was in no way inevitable. Pasteur could have failed to link his work on microbes to his many clients' interests. Had he failed, then I agree that the distinction of levels would hold true:

there would indeed be French agricultural, medical, social, political interests on the one hand, and the insulated laboratory of a disinterested scientist at the École Normale Supérieure on the other. Claude Bernard had such a laboratory. But this was in no way Pasteur's strategy, and still less that of the larger Institut Pasteur, which was always situated in such a way that all the interested commercial, colonial, and medical interests had to pass through their laboratories to borrow the techniques, the gestures, the products, the diagnostic kits that were necessary to further their own desires. Laboratories were set up everywhere: on the front line during the First World war in the trenches they largely made possible; before the colonists arrived in the tropics, allowing the very survival of the white colonists and their soldiers; in the surgery ward that was transformed from a teaching amphitheatre into a laboratory (Salomon-Bayet, 1982); in the plants of the food industries in many public health services; inside the small offices of general practitioners; in the midst of farms, and so on. Give us laboratories and we will make possible the Great War without infection, we will open tropical countries to colonization, we will make France's army healthy, we will increase the number and strength of her inhabitants, we will create new industries. Even blind and deaf analysts will see these claims as "social" activity, but on condition that laboratories are considered places where society and politics are renewed and transformed.

III. HOW THE WEAKEST BECOMES THE STRONGEST

What I have said about the example treated in Part I now leads us to the more general problem of laboratory practice and of the relevance of microstudies for understanding the "large-scale" problems raised by the field known as Science, Technology, and Society (STS). If I were to summarize the argument presented in Part II, I could say that a sociology of science hamstrings itself from the start: if, that is, it takes for granted the difference of levels or of scale between the "social context" on the one hand and the laboratory or the "scientific level" on the other; and if it fails to study *the very content* of what is being done inside the laboratories. I claim that, on the contrary, laboratories are among the few places where the differences of scale are made irrelevant and where the very content of the trials made within the walls of the laboratory can alter the composition of society.

The most difficult problem for understanding this positioning laboratory practice is to define precisely why it is that in the laboratory and only there new sources of strength are generated. Using the metaphor of the lever, why is a laboratory a solid lever and not a soft straw? In asking this question we are back to the problem of understanding what has been achieved through microstudies of science. Many answers were given by epistemologists before lab studies started pouring in. It was said that scientists had special methods, special minds, or in more culturalist forms of racism, some kind of special culture. It was always in something "special," usually of a cognitive quality, that this source of strength was explained. Of course, the moment sociologists walked into laboratories and started checking all these theories about the strength of science, they just disappeared. Nothing special, nothing extraordinary, in fact nothing of any cognitive quality was occurring there. Epistemologists had chosen the wrong objects, they looked for mental aptitudes and ignored the material local setting, that is, laboratories. The same thing happened with most of the so-called Mertonian sociology. No special sociological relations could explain anything about the strength of science. The "norms" faded away like the "invisible

college" and the "precapitalist recognition of debt," and went into the limbo where "falsification," and the "angels' sexes" are put for a well-deserved eternal rest. The first sociologists made the same mistake as the epistemologists. They looked for something special everywhere except in the most obvious and striking place: the settings. Even scientists themselves are more aware of what makes them special than many analysts. Pasteur, for instance, a better sociologist and epistemologist than most, wrote a kind of treatise on sociology of science simply pointing to the laboratory as the cause of the strength gained by scientist over society (Pasteur, 1871).

Laboratory studies have been successful, but so far only in the negative sense of dissipating previous beliefs surrounding science. Nothing special is happening in the cognitive and in the social aspect of laboratory practice. Knorr-Cetina has reviewed this and there is nothing much else to add, nothing except that we now have to explain what happens in laboratories that makes them such an irreplaceable source of political strength, strength which is *not* explained by any cognitive or social peculiarities.

How do a few people gain strength and go inside some places and the life of the multitudes? Pasteur, for instance, and his few collaborators cannot tackle the anthrax problem by moving all over France and gathering an intimate knowledge of all the farms, farmers, animals, and local idiosyncrasies. The only place where they are able and good workers is in their laboratory. Outside they are worse at farming than the farmers and worse at veterinary medicine than the veterinarians. But they are expert inside their own walls at setting up trials and instruments so that the invisible actors—which they call microbes—show their moves and development in pictures so clear that even a child would see them. The invisible becomes visible and the "thing" becomes a written trace they can read at will as if it were a text. This expertise, in their case, is already obtained by a complete modification of the scale. As has been previously explained, the microbe is invisible as long as it is not cultivated in isolation from its other competitors. As soon as it grows uninhibited on an aptly chosen medium, it grows exponentially and makes itself large enough to be counted as small dots on the Petri dish. I don't know what a microbe is, but counting dots with clear-cut edges on a white surface is simple. The problem now is to link this expertise to the health field. I showed the solution earlier by these three-pronged movements that displace the laboratory. The consequence is clear. By these moves an epizootic occurs inside the laboratory walls that is deemed relevant to the macroproblems outside. Again the scale of the problem is reversed, but this time it's the "macro" that is made small enough to be dominated by the Pasteurians. Before this displacement and inversion that allowed Pasteurians to hook an expertise in setting up inscription devices onto the health field, no one had ever been able to master the course of an epidemic. This "mastery" means that each event—the inoculation, the outbreak of an epidemic, the vaccination, the counting of the dead and of the living, the timing, the places—becomes entirely readable by a few men who could agree among themselves because of the simplicity of each perceptive judgment they were able to make about simple diagrams and curves.

The strength gained in the laboratory is not mysterious. A few people much weaker than epidemics can become stronger if they change the scale of the two actors—making the microbes big, and the epizootic small—and others dominate the events through the inscription devices that make each of the steps readable. The change of scale entails an acceleration in the number of inscriptions you can get. Obtaining data on anthrax epidemics on the scale of France was a slow, painstaking, and uncertain process. But in a year Pasteur could multiply anthrax outbreaks. No wonder that he became stronger than veterinarians. For every statistic they had, he could

mobilize ten of them. Before Pasteur, their statements could be interrupted by any number of other statements just as plausible as theirs. But when Pasteur comes out of his lab with these many figures, who is able to mount a serious attack against him? Pasteur has gained strength simply by modifying the scale. So, in discussions about anthrax, Pasteur has two sources of strength: the epizootic and the microbes. His opponents and predecessors had to work "outside" on a "large scale," constantly stabbed in the back haphazardly by the invisible agent that made their statistics look random. But Pasteur, by building his laboratory and inserting it in the farms as we have seen, dominates the microbe—that he made bigger—and the epizootic—that he made smaller—and multiplies the experiments at small cost *without leaving his laboratory.* This concentration of forces makes him so much stronger than his competitors that they cannot even think of a counterargument except in the few cases where, like Koch, they are equipped as well as he is.

To understand the reason why people pay so much for laboratories which are actually ordinary places, one just has to consider these places as nice technological devices to invert the hierarchy of forces. Thanks to a chain of displacements—both of the laboratory and of the objects—the scale of what people want to talk about is modified so as to reach this best of all possible scales: the inscription on a flat surface written in simple forms and letters. Then everything they have to talk about is not only visible but also readable, and can be easily pointed at by a few people who by doing this dominate. This is as simple and as sufficient as Archimedes' point about moving the earth and making the weakest the strongest. It is simple indeed because making simple moves is what this device is about. "Accumulated knowledge" people say with admiration, but this acceleration is made possible by a change of scale, which in turn makes possible the multiplication of trials and errors. Certainty does not increase in a laboratory because people in it are more honest, more rigorous, or more "falsificationist." It is simply that they can make as many mistakes as they wish or simply more mistakes than the others "outside" who cannot master the changes of scale. Each mistake is in turn archived, saved, recorded, and made easily readable again, whatever the specific field or topic may be. If a great many trials are recorded and it is possible to make a sum of their inscriptions, that sum will always be more certain if it decreases the possibility of a competitor raising a statement as plausible as the one you are defending. That is enough. When you sum up a series of mistakes, you are stronger than anyone who has been allowed fewer mistakes than you.

This vision of the laboratory as a technological device to gain strength by multiplying mistakes, is made obvious if one looks at the difference between a politician and a scientist. They are typically contrasted on cognitive or social grounds. The first is said to be greedy, full of self-interest, short-sighted, fuzzy, always ready to compromise, and shaky. The second is said to be disinterested, far-sighted, honest, or at least rigorous, to talk clearly and exactly and to look for certainty. These many differences are all artificial projections of one, simple, material thing. The politician has no laboratory and the scientist has one. So the politician works on a full scale, with only one shot at a time, and is constantly in the limelight. He gets by, and wins or loses "out there." The scientist works on scale models, multiplying the mistakes inside his laboratory, hidden from public scrutiny. He can try as many times as he wishes and comes out only when he has made all the mistakes that have helped him gain "certainty." No wonder that one does not "know" and the other "knows." The difference, however, is not in "knowledge." If you could by chance reverse the positions, the same greedy, short-sighted politician, once in a laboratory, is going to churn out exact scientific facts, and the honest, disinterested, rigorous scientist put at the helm of a political structure that is full scale and with no mistakes allowed will become fuzzy,

uncertain, and weak like everyone else. The specificity of science is not to be found in cognitive, social, or psychological qualities, but in the special construction of laboratories in a manner which reverses the scale of phenomena so as to make things readable, and then accelerates the frequency of trials, allowing many mistakes to be made and registered.

That the laboratory setting is the cause of the strength gained by scientists is made still clearer when people want to establish elsewhere conclusions as certain as those reached in the laboratory. As I have shown above, it can be said that there is no outside to laboratories. The best thing one can do is to extend to other places the "hierarchy of forces" that was once favourable inside the first laboratory. I showed this for anthrax but it is a general case. The mystification of science comes most often from the idea that scientists are able to make "predictions." They work in their labs and, sure enough, something happens outside that verifies these predictions. The problem is that no one has ever been able to verify these predictions without extending first the conditions of verification that existed in the laboratory. The vaccine extends on the condition that farms are transformed into an annex of Pasteur's lab and that the very statistical system that made anthrax visible in the first place is used to verify if the vaccine had any effect. We can watch the extension of laboratory conditions, and the repetition of the final trial that was favourable, but we cannot watch predictions of scientists extending themselves beyond laboratory walls (Latour and Woolgar, 1979: ch. 4).

If this seems counterintuitive to the reader, a little reasoning will convince him that every counterexample he can think of in fact conforms to the position stated here. No one has ever seen a laboratory fact move outside unless the lab is first brought to bear on an "outside" situation and that situation is transformed so that it fits laboratory prescriptions. Every counterexample is a belief that such a thing is possible. But a belief is not a proof. If the proof is given then the two conditions I stated will always be verified. My confidence in this answer is not based on presumption but on a simple scientific belief, shared by all my fellow scientists, that magic is impossible and that action at a distance is always a misrepresentation. Scientists' predictions or previsions are always postdictions or repetitions. The confirmation of this obvious phenomenon is shown in scientific controversies when scientists are forced to leave the solid ground of their laboratories. The moment they really get "outside" they know nothing, they bluff, they fail, they get by, they lose all possibility to say anything that is not immediately counterattacked by swarms of equally plausible statements.

The only way for a scientist to retain the strength gained inside his laboratory by the process I have described is not to go outside where he would lose it at once. It is again very simple. The solution is in *never going out.* Does that mean that they are stuck in the few places where they work? No. It means that they will do everything they can to extend to every setting some of the conditions that make possible the reproduction of favourable laboratory practices. Since scientific facts are made inside laboratories, in order to make them circulate you need to build costly networks inside which they can maintain their fragile efficacy. *If this means transforming society into a vast laboratory, then do it.* The spread of Pasteurian laboratories to all the places that a few decades before had nothing to do with science is good example of this network building. But a look at systems of Standard Weights and Measures, called "métrologie" in French, is still more convincing. Most of the work done in a laboratory would stay there forever if the principal physical constants could not be made constant everywhere else. Time, weight, length, wavelength, etc., are extended to ever more localities and in ever greater degrees of precision. Then and only then, laboratory experiments can be brought to bear on problems occurring in factories, the tool

industry, economics, or hospitals. But if you just try in a thought experiment to extend the simplest law of physics "outside," without first having extended and controlled all the main constants, you just could not verify it, just as it would have been impossible to know the existence of anthrax and to see the efficacy of the vaccine without the health statistics. This transformation of the whole of society according to laboratory experiments is ignored by sociologists of science.

There is no outside of science but there are long, narrow networks that make possible the circulation of scientific facts. Naturally the reason for this ignorance is easy to understand. People think that the universality of science is a given, because they forget to take into account the size of the "métrologie." Ignoring this transformation that makes all displacement possible is like studying an engine without the railway or the freeway networks. The analogy is a good one since the seemingly simple work of maintaining the physical constants constant in a modern society is evaluated to be three times more than the effort of all the science and technology themselves (Hunter, 1980). The cost of making society conform to the inside of laboratories so that the latter's activity can be made relevant to the society is constantly forgotten, because people do not want to see that universality is a social construction as well.

Once all these displacements and transformations are taken into account, the distinction between the macrosocial level and the level of laboratory science appears fuzzy or even nonexistent. Indeed, laboratories are built to destroy this distinction. Once it is dissolved, a few people can, inside their insulated walls, work on things that can change the daily life of the multitudes. No matter if they are economists, physicists, geographers, epidemiologists, accountants, microbiologists, they make all the other objects on such a scale—maps, economic models, figures, tables, diagrams—that they can gain strength, reach incontrovertible conclusions, and then extend on a larger scale the conclusions that seem favourable to them. *It is* a political process. *It is not* a political process. It is since they gain a source of power. It is not since it is a source of fresh power that escapes the routine and easy definition of a stated political power. "Give me a laboratory and I will move society," I said, parodying Archimedes. We now know why a laboratory is such a good lever. But if I now parody Clausewitz's motto, we will have a more complete picture: "science is politics pursued by other means." It is not politics since a power is always blocked by another counterpower. What counts in laboratory sciences are the other means, the fresh, unpredictable sources of displacements that are all the more powerful because they are ambiguous and unpredictable. Pasteur, representing the microbes and displacing everyone else, is making politics, but by other, unpredictable means that force everyone else out, including the traditional political forces. We can now understand why it was and is so important to stick to laboratory microstudies. In our modern societies most of the really fresh power comes from sciences—no matter which—and not from the classical political process. By staking all social explanations of science and technology on the classical view of politics and economics—profit, stated power, predictable evils or goods—analysts of science who claim to study the macrolevels fail to understand precisely what is strong in science and technology. In speaking of scientists who make politics by other means, their boring and repetitive critique is always that they "just make politics," period. Their explanation falls short. The shortness of it is in the period—they stop where they should start. Why though are the means different? To study these other means, one must get inside the contents of the sciences, and inside the laboratories where the future reservoirs of political power are in the making. The challenge of laboratories to sociologists is the same as the challenge of laboratories to society. They can displace society and recompose it by the very content of what is done inside them, which seemed at first irrelevant or too technical. The careful

scrutiny of laboratory scientists cannot be ignored and no one can jump from this "level" to the macropolitical level since the latter gets all its really efficient sources of power from these very laboratories that have just been deemed uninteresting or too technical to be analyzed.

But we can also understand why students of laboratory practices should not be shy and accept a vision of their own method that would limit them to the laboratory, whereas the laboratory is just a moment in a series of displacements that makes a complete shambles out of the inside/outside and the macro/micro dichotomies. No matter how divided they are on sociology of science, the macroanalysts and the microanalysts share one prejudice: *that science stops or begins at the laboratory walls.* The laboratory is a much trickier object than that, it is a much more efficient transformer of forces than that. That is why by remaining faithful to his method, the microanalyst will end up tackling macroissues as well, exactly like the scientist doing lab experiments on microbes who ends up modifying many details of the whole of French society. Indeed, I think an argument could be made to show that the existence of the macrolevel itself, the famous "social context," is a consequence of the development of many scientific disciplines (Callon and Latour, 1981). It is already clear to me that this is the only way that sociology of science can be rebuilt in keeping with the constraints now set by laboratory studies. I also think that it is one of the few ways that sociology of science can teach something to sociology instead of borrowing from it categories and social structures that the simplest laboratory is destroying and recomposing. It would be high time, since the laboratory is more innovative in politics and in sociology than most sociologists (including many sociologists of science). We are only just starting to take up the challenge that laboratory practices present for the study of society.

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