From: Henri Poincaré, *The Foundations of Science*, George Bruce Halsted, tr. (New York: The Science Press, 1913)

… [27]

SCIENCE AND HYPOTHESIS

INTRODUCTION

FOR a superficial observer, scientific truth is beyond the possibility of doubt; the logic of science is infallible, and if the scientists are sometimes mistaken, this is only from their mistaking its rules.

"The mathematical verities flow from a small number of self-evident propositions by a chain of impeccable reasonings; they impose themselves not only on us, but on nature itself. They fetter, so to speak, the Creator and only permit him to choose between some relatively few solutions. A few experiments then will suffice to let us know what choice he has made. From each experiment a crowd of consequences will follow by a series of mathematical deductions, and thus each experiment will make known to us a comer of the universe."

Behold what is for many people in the world, for scholars getting their first notions of physics, the origin of scientific certitude. This is what they suppose to be the rôle of experimentation and mathematics. This same conception, a hundred years ago, was held by many savants who dreamed of constructing the world with as little as possible taken from experiment.

On a little more reflection it was perceived how great a place hypothesis occupies; that the mathematician can not do without it, still less the experimenter. And then it was doubted if all these constructions were really solid, and believed that a breath would overthrow them. To be skeptical in this fashion is still to be superficial. To doubt everything and to believe everything are two equally convenient solutions; each saves us from thinking.

Instead of pronouncing a summary condemnation, we ought therefore to examine with care the rôle of hypothesis; we shall then recognize, not only that it is necessary, but that usually it is [28] legitimate. We shall also see that there are several sorts of hypotheses; that some are verifiable, and once confirmed by experiment become fruitful truths; that others, powerless to lead us astray, may be useful to us in fixing our ideas; that others, finally, are hypotheses only in appearance and are reducible to disguised definitions or conventions.

These last are met with above all in mathematics and the related sciences. Thence precisely it is that these sciences get their rigor; these conventions are the work of the free activity of our mind, which, in this domain, recognizes no obstacle. Here our mind can affirm, since it decrees; but let us understand that while these decrees are imposed upon *our* science, which, without them, would be impossible, they are not imposed upon nature. Are they then arbitrary? No, else were they sterile. Experiment leaves us our freedom of choice, but it guides us by aiding us to discern the easiest way. Our decrees are therefore like those of a prince, absolute but wise, who consults his council of state.

Some people have been struck by this character of free convention recognizable in certain fundamental principles of the sciences. They have wished to generalize beyond measure, and, at the same time, they have forgotten that liberty is not license. Thus they have reached what is called *nominalism*, and have asked themselves if the savant is not the dupe of his own definitions and if the world he thinks he discovers is not simply created by his own caprice.[1] Under these conditions science would be certain, but deprived of significance.

[1] See Le Roy, 'Science et Philosophie,' *Revue de Métaphysique et de Morale*, 1901.

If this were so, science would be powerless. Now every day we see it work under our very eyes. That could not be if it taught us nothing of reality. Still, the things themselves are not what it can reach, as the naïve dogmatists think, but only the relations between things. Outside of these relations there is no knowable reality.

Such is the conclusion to which we shall come, but for that we must review the series of sciences from arithmetic and geometry to mechanics and experimental physics. [29]

What is the nature of mathematical reasoning? Is is really deductive, as is commonly supposed? A deeper analysis shows us that it is not, that it partakes in a certain measure of the nature of inductive reasoning, and just because of this is it so fruitful. None the less does it retain its character of rigor absolute; this is the first thing that had to be shown.

Knowing better now one of the instruments which mathematics puts into the hands of the investigator, we had to analyze another fundamental notion, that of mathematical magnitude. Do we find it in nature, or do we ourselves introduce it there? And, in this latter case, do we not risk marring everything? Comparing the rough data of our senses with that extremely complex and subtile concept which mathematicians call magnitude, we are forced to recognize a difference; this frame into which we wish to force everything is of our own construction; but we have not made it at random. We have made it, so to speak, by measure and therefore we can make the facts fit into it without changing what is essential in them.

Another frame which we impose on the world is space. Whence come the first principles of geometry? Are they imposed on us by logic? Lobachevski has proved not, by creating non-Euclidean geometry. Is space revealed to us by our senses? Still no, for the space our senses could show us differs absolutely from that of the geometer.

Is experience the source of geometry? A deeper discussion will show us it is not. We therefore conclude that the first principles of geometry are only conventions; but these conventions are not arbitrary and if transported into another world (that I call the non-Euclidean world and seek to imagine), then we should have been led to adopt others.

In mechanics we should be led to analogous conclusions, and should see that the principles of this science, though more directly based on experiment, still partake of the conventional character of the geometric postulates. Thus far nominalism triumphs; but now we arrive at the physical sciences, properly so called. Here the scene changes; we meet another sort of hypotheses and we see their fertility. Without doubt, at first blush, the theories seem to us fragile, and the history of science proves to us how ephemeral they are; yet they do not entirely perish, [30] and of each of them something remains. It is this something we most seek to disentangle, since there and there alone is the veritable reality.

The method of the physical sciences rests on the induction which makes us expect the repetition of a phenomenon when the circumstances under which it first happened are reproduced. If *all* these circumstances could be reproduced at once, this principle could be applied without fear; but that will never happen; some of these circumstances will always be lacking. Are we absolutely sure they are unimportant? Evidently not. That may be probable, it can not be rigorously certain. Hence the important rôle the notion of probability plays in the physical sciences. The calculus of probabilities is therefore not merely a recreation or a guide to players of baccarat, and we must seek to go deeper with its foundations. Under this head I have been able to give only very incomplete results, so strongly does this vague instinct which lets us discern probability defy analysis.

After a study of the conditions under which the physicist works, I have thought proper to show him at work. For that I have taken instances from the history of optics and of electricity. We shall see whence have sprung the ideas of Fresnel, of Maxwell, and what unconscious hypotheses were made by Ampère and the other founders of electrodynamics.

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PART III

THE OBJECTIVE VALUE OF SCIENCE

CHAPTER X

Is Science Artificial?

1. *The Philosophy of M. LeRoy*

THERE are many reasons for being sceptics; should we push this scepticism to the very end or stop on the way? To go to the end is the most tempting solution, the easiest, and that which many have adopted, despairing of saving anything from the shipwreck.

Among the writings inspired by this tendency it is proper to place in the first rank those of M. LeRoy. This thinker is not only a philosopher and a writer of the greatest merit, but he has acquired a deep knowledge of the exact and physical sciences, and even has shown rare powers of mathematical invention. Let us recapitulate in a few words his doctrine, which has given rise to numerous discussions.

Science consists only of conventions, and to this circumstance solely does it owe its apparent certitude; the facts of science and, *a fortiori*, its laws are the artificial work of the scientist; science therefore can teach us nothing of the truth; it can only serve us as rule of action.

Here we recognize the philosophic theory known under the name of nominalism; all is not false in this theory; its legitimate domain must be left it, but out of this it should not be allowed to go.

This is not all; M. LeRoy's doctrine is not only nominalistic; it has besides another characteristic which it doubtless owes to M. Bergson, it is anti-intellectualistic. According to M. LeRoy, the [322] intellect deforms all its touches, and that is still more true of its necessary instrument 'discourse.' There is reality only in our fugitive and changing impressions, and even this reality, when touched, vanishes.

And yet M. LeRoy is not a sceptic; if he regards the intellect as incurably powerless, it is only to give more scope to other sources of knowledge, to the heart, for instance, to sentiment, to instinct or to faith.

However great my esteem for M. LeRoy's talent, whatever the ingenuity of this thesis, I can not wholly accept it. Certes, I am in accord on many points with M. LeRoy, and he has even cited, in support of his view, various passages of my writings which I am by no means disposed to reject. I think myself only the more bound to explain why I can not go with him all the way.

M. LeRoy often complains of being accused of scepticism. He could not help being, though this accusation is probably unjust. Are not appearances against him? Nominalist in doctrine, but realist at heart, he seems to escape absolute nominalism only by a desperate act of faith.

The fact is that anti-intellectualistic philosophy in rejecting analysis and 'discourse,' just by that condemns itself to being intransmissible; it is a philosophy essentially internal, or, at the very least, only its negations can be transmitted; what wonder then that for an external observer it takes the shape of scepticism?

Therein lies the weak point of this philosophy; if it strives to remain faithful to itself, its energy is spent in a negation and a cry of enthusiasm. Each author may repeat this negation and this cry, may vary their form, but without adding anything.

And yet, would it not be more logical in remaining silent? See, you have written long articles; for that, it was necessary to use words. And therein have you not been much more 'discursive' and consequently much farther from life and truth than the animal who simply lives without philosophizing? Would not this animal be the true philosopher?

However, because no painter has made a perfect portrait, should we conclude that the best painting is not to paint? When a zoologist dissects an animal, certainly he 'alters it.' Yes, in dissecting it, he condemns himself to never know all of it; but in [323] not dissecting it, he would condemn himself to never know anything of it and consequently to never see anything of it.

Certes, in man are other forces besides his intellect; no one has ever been mad enough to deny that. The first comer makes these blind forces act or lets them act; the philosopher must speak of them; to speak of them, he must know of them the little that can be known, he should therefore *see* them act. How? With what eyes, if not with his intellect? Heart, instinct, may guide it, but not render it useless; they may direct the look, but not replace the eye. It may be granted that the heart is the workman, and the intellect only the instrument. Yet is it an instrument not to be done without, if not for action, at least for philosophizing? Therefore a philosopher really anti-intellectualistic is impossible. Perhaps we shall have to declare for the supremacy of action; always it is our intellect which will thus conclude; in allowing precedence to action it will thus retain the superiority of the thinking reed. This also is a supremacy not to be disdained.

Pardon these brief reflections and pardon also their brevity, scarcely skimming the question. The process of intellectualism is not the subject I wish to treat: I wish to speak of science, and about it there is no doubt; by definition, so to speak, it will be intellectualistic or it will not be at all. Precisely the question is, whether it will be.

2. *Science, Rule of Action*

For M. LeRoy, science is only a rule of action. We are powerless to know anything and yet we are launched, we must act, and at all hazards we have established rules. It is the aggregate of these rules that is called science.

It is thus that men, desirous of diversion, have instituted rules of play, like those of tric-trac for instance, which, better than science itself, could rely upon the proof by universal consent. It is thus likewise that, unable to choose, but forced to choose, we toss up a coin, head or tail to win.

The rule of tric-trac is indeed a rule of action like science, but does any one think the comparison just and not see the difference? The rules of the game are arbitrary conven[324]tions and the contrary convention might have been adopted, *which would have been none the less good*. On the contrary, science is a rule of action which is successful, generally at least, and I add, while the contrary rule would not have succeeded.

If I say, to make hydrogen cause an acid to act on zinc, I formulate a rule which succeeds; I could have said, make distilled water act on gold; that also would have been a rule, only it would not have succeeded. If, therefore, scientific 'recipes' have a value, as rule of action, it is because we know they succeed, generally at least. But to know this is to know something and then why tell us we can know nothing?

Science foresees, and it is because it foresees that it can be useful and serve as rule of action. I well know that its previsions are often contradicted by the event; that shows that science is imperfect, and if I add that it will always remain so, I am certain that this is a prevision which, at least, will never be contradicted. Always the scientist is less often mistaken than a prophet who should predict at random. Besides the progress though slow is continuous, so that scientists, though more and more bold, are less and less misled. This is little, but it is enough.

I well know that M. LeRoy has somewhere said that science was mistaken oftener than one thought, that comets sometimes played tricks on astronomers, that scientists, who apparently are men, did not willingly speak of their failures, and that, if they should speak of them, they would have to count more defeats than victories.

That day, M. LeRoy evidently overreached himself. If science did not succeed, it could not serve as rule of action; whence would it get its value? Because it is 'lived,' that is, because we love it and believe in it? The alchemists had recipes for making gold, they loved them and had faith in them, and yet our recipes are the good ones, although our faith be less lively, because they succeed.

There is no escape from this dilemma; either science does not enable us to foresee, and then it is valueless as rule of action; or else it enables us to foresee, in a fashion more or less imperfect, and then it is not without value as means of knowledge. [325]

It should not even be said that action is the goal of science; should we condemn studies of the star Sinus, under pretext that we shall probably never exercise any influence on that star? To my eyes, on the contrary, it is the knowledge which is the end, and the action which is the means. If I felicitate myself on the industrial development, it is not alone because it furnishes a facile argument to the advocates of science; it is above all because it gives to the scientist faith in himself and also because it offers him an immense field of experience where he clashes againat forces too colossal to be tampered with. Without this ballast, who knows whether he would not quit solid ground, seduced by the mirage of some scholastic novelty, or whether he would not despair, believing he had fashioned only a dream!

3. *The Crude Fact and the Scientific Fact*

What was most paradoxical in M. LeRoy's thesis was that affirmation that *the scientist creates the fact;* this was at the same time its essential point and it is one of those which have been most discussed.

Perhaps, says he (I well believe that this was a concession), it is not the scientist that creates the fact in the rough; it is at least he who creates the scientific fact.

This distinction between the fact in the rough and the scientific fact does not by itself appear to me illegitimate. But I complain first that the boundary has not been traced either exactly or precisely; and then that the author has seemed to suppose that the crude fact, not being scientific, is outside of science.

Finally, I can not admit that the scientist creates without restraint the scientific fact, since it is the crude fact which imposes it upon him.

The examples given by M. LeRoy have greatly astonished me. The first is taken from the notion of atom. The atom chosen as example of fact! I avow that this choice has so disconcerted me that I prefer to say nothing about it. I have evidently misunderstood the author's thought and I could not fruitfully discuss it.

The second case taken as example is that of an eclipse where the crude phenomenon is a play of light and shadow, but where [326] the astronomer can not intervene without introducing two foreign elements, to wit, a clock and Newton's law.

Finally, M. LeRoy cites the rotation of the earth; it has been answered: but this is not a fact, and he has replied: it was one for Galileo, who affirmed it, as for the inquisitor, who denied it. It always remains that this is not a fact in the same sense as those just spoken of and that to give them the same name is to expose one's self to many confusions.

Here then are four degrees:

1°. It grows dark, says the clown.

2°. The eclipse happened at nine o'clock, says the astronomer.

3°. The eclipse happened at the time deducible from the tables constructed according to Newton's law, says he again.

4°. That results from the earth's turning around the sun, says Galileo finally.

Where then is the boundary between the fact in the rough and the scientific fact? To read M. LeRoy one would believe that it is between the first and the second stage, but who does not see that there is a greater distance from the second to the third, and still more from the third to the fourth.

Allow me to cite two examples which perhaps will enlighten us a little.

I observe the deviation of a galvanometer by the aid of a movable mirror which projects a luminous image or spot on a divided scale. The crude fact is this: I see the spot displace itself on the scale, and the scientific fact is this: a current passes in the circuit.

Or again: when I make an experiment I should subject the result to certain corrections, because I know I must have made errors. These errors are of two kinds, some are accidental and these I shall correct by taking the mean; the others are systematic and I shall be able to correct those only by a thorough study of their causes. The first result obtained is then the fact in the rough, while the scientific fact is the final result after the finished corrections.

Reflecting on this latter example, we are led to subdivide our second stage, and in place of saying:

2. The eclipse happened at nine o'clock, we shall say:

2*a*. The eclipse happened when my clock pointed to nine, and [327]

2*b*. My clock being ten minutes slow, the eclipse happened at ten minutes past nine.

And this is not all: the first stage also should be subdivided, and not between these two subdivisions will be the least distance; it is necessary to distinguish between the impression of obscurity felt by one witnessing an eclipse, and the affirmation: It grows dark, which this impression extorts from him. In a sense it is the first which is the only true fact in the rough, aud the second is already a sort of scientific fact.

Now then our scale has six stages, and even though there is no reason for halting at this figure, there we shall stop.

What strikes me at the start is this. At the first of our six stages, the fact, still completely in the rough, is, so to speak, individual, it is completely distinct from all other possible facts. From the second stage, already it is no longer the same. The enunciation of the fact would suit an infinity of other facts. So soon as language intervenes, I have at my command only a finite number of terms to express the shades, in number infinite, that my impressions might cover. When I say: It grows dark, that well expresses the impressions I feel in being present at an eclipse; but even in obscurity a multitude of shades could be imagined, and if, instead of that actually realized, had happened a slightly different shade, yet I should still have enunciated this *other* fact by saying: It grows dark.

Second remark: even at the second stage, the enunciation of a fact can only be *true or false*. This is not so of any proposition; if this proposition is the enunciation of a convention, it can not be said that this enunciation is *true*, in the proper sense of the word, since it could not be true apart from me and is true only because I wish it to be.

When, for instance, I say the unit for length is the meter, this is a decree that I promulgate, it is not something ascertained which forces itself upon me. It is the same, as I think I have elsewhere shown, when it is a question, for example, of Euclid's postulate.

When I am asked: Is it growing dark? I always know whether I ought to reply yes or no. Although an infinity of possible facts may be susceptible of this same enunciation, it grows dark, [328] I shall always know whether the fact realized belongs or does not belong among those which answer to this enunciation. Facts are classed in categories, and if I am asked whether the fact that I ascertain belongs or does not belong in such a category, I shall not hesitate.

Doubtless this classification is sufficiently arbitrary to leave a large part to man's freedom or caprice. In a word, this classification is a convention. *This convention being given*, if I am asked: Is such a fact true? I shall always know what to answer, and my reply will be imposed upon me by the witness of my senses.

If therefore, during an eclipse, it is asked: Is it growing dark? all the world will answer yes. Doubtless those speaking a language where bright was called dark, and dark bright, would answer no. But of what importance is that?

In the same way, in mathematics, *when I have laid down the definitions, and the postulates which are conventions,* a theorem henceforth can only be true or false. But to answer the question: Is this theorem true? it is no longer to the witness of my senses that I shall have recourse, but to reasoning.

A statement of fact is always verifiable, and for the verification we have recourse either to the witness of our senses, or to the memory of this witness. This is properly what characterizes a fact. If you put the question to me: Is such a fact true? I shall begin by asking you, if there is occasion, to state precisely the conventions, by asking you, in other words, what language you have spoken; then once settled on this point, I shall interrogate my senses and shall answer yes or no. But it will be my senses that will have made answer, it will not be *you* when you say to me: I have spoken to you in English or in French.

Is there something to change in all that when we pass to the following stages? When I observe a galvanometer, as I have just said, if I ask an ignorant visitor: Is the current passing? he looks at the wire to try to see something pass; but if I put the same question to my assistant who understands my language, he will know I mean: Does the spot move? and he will look at the scale.

What difference is there then between the statement of a fact [329] in the rough and the statement of a scientific fact? The same difference as between the statement of the same crude fact in French and in German, The scientific statement is the translation of the crude statement into a language which is distinguished above all from the common German or French, because it is spoken by a very much smaller number of people.

Yet let us not go too fast. To measure a current I may use a very great number of types of galvanometers or besides an electrodynamometer. And then when I shall say there is running in tbis circuit a current of so many amperes, that will mean: if I adapt to this circuit such a galvanometer I shall see the spot come to the division *a*; but that will mean equally: if I adapt to this circuit such an eleetrodynamometer, I shall see the spot go to the division *b*. And that will mean still many other things, because the current can manifest itself not only by mechanical effects, but by effects chemical, thermal, luminous, etc.

Here then is one same statement which suits a very great number of facts absolutely different. Why? It is because I assume a law according to which, whenever such a mechanical effect shall happen, such a chemical effect will happen also. Previous experiments, very numerous, have never shown this law to fail, and then I have understood that I could express by the same statement two facts so invariably bound one to the other.

When I am asked: Is the current passing? I can understand that that means: Will such a mechanical effect happen? But I can understand also: Will such a chemical effect happen? I shall then verify either the existence of the mechanical effect, or that of the chemical effect; that will be indifferent, since in both caaes the answer must be same.

And if the law should one day be found false? If it was perceived that the concordance of the two effects, mechanical and chemical, is not constant? That day it would be necessary to change the scientific language to free it from a grave ambiguity.

And after that? Is it thought that ordinary language by aid of which are expressed the facts of daily life is exempt from ambiguity?

Shall we thence conclude that the facts of daily life are the work of the grammarians? [330]

You ask me: Is there a current? I try whether the mechanical effect exists, I ascertain it and I answer: Yes, there is a current You understand at once that that means that the mechanical effect exists, and that the chemical effect, that I have not investigated, exists likewise. Imagine now, supposing an impossibility, the law we believe true, not to be, and the chemical effect not to exist. Under this hypothesis there will be two distinct facts, the one directly observed and which is true, the other inferred and which is false. It may strictly be said that we have created the second. So that error is the part of man's personal collaboration in the creation of the scientific fact.

But if we can say that the fact in question is false, is this not just because it is not a free and arbitrary creation of our mind, a disguised convention, in which case it would be neither true nor false. And in fact it was verifiable; I had not made the verification, but I could have made it. If I answered amiss, it was because I chose to reply too quickly, without having asked nature, who alone knew the secret.

When, after an experiment, I correct the accidental and systematic errors to bring out the scientific fact, the case is the same; the scientific fact will never be anything but the crude fact translated into another language. When I shall say: It is such an hour, that will be a short way of saying: There is such a relation between the hour indicated by my clock, and the hour it marked at the moment of the passing of such a star and such another star across the meridian. And this convention of language once adopted, when I shall be asked: Is it such an hour? it will not depend upon me to answer yes or no.

Let us pass to the stage before the last: the eclipse happened at the hour given by the tables deduced from Newton's laws. This is still a convention of language which is perfectly clear for those who know celestial mechanics or simply for those who have the tables calculated by the astronomers. I am asked: Did the eclipse happen at the hour predicted? I look in the nautical almanac, I see that the eclipse was announced for nine o'clock and I understand that the question means: Did the eclipse happen at nine o'clock? There still we have nothing to change in our conclusions. *The scientific fact is only the crude fact translated into a convenient language*. [331]

It is true that at the last stage things change. Does the earth rotate? Is this a verifiable fact? Could Galileo and the Grand Inquisitor, to settle the matter, appeal to the witness of their senses? On the contrary, they were in accord about the appearances, and whatever had been the accumulated experiences, they would have remained in accord with regard to the appearances without ever agreeing on their interpretation. It is just on that account that they were obliged to have recourse to procedures of discussion so unscientific.

This is why I think they did not disagree about a *fact:* we have not the right to give the same name to the rotation of the earth, which was the object of their discussion, and to the facts crude or scientific we have hitherto passed in review.

After what precedes, it seems superfluous to investigate whether the fact in the rough is outside of science, because there can neither be science without scientific fact, nor scientific fact without fact in the rough, since the first is only the translation of the second.

And then, has one the right to say that the scientist creates the scientific fact? First of all, he does not create it from nothing, since he makes it with the fact in the rough. Consequently he does not make it freely and *as he chooses*. However able the worker may be, his freedom is always limited by the properties of the raw material on which he works.

After all, what do you mean when you speak of this free creation of the scientific fact and when you take as example the astronomer who intervenes actively in the phenomenon of the eclipse by bringing his clock? Do you mean: The eclipse happened at nine o'clock; but if the astronomer had wished it to happen at ten, that depended only on him, he had only to advance his clock an hour?

But the astronomer, in perpetrating that bad joke, would evidently have been guilty of an equivocation. When he tells me: The eclipse happened at nine, I understand that nine is the hour deduced from the crude indication of the pendulum by the usual series of corrections. If he has given me solely that crude indication, or if he has made corrections contrary to the habitual rules, he has changed the language agreed upon without fore[332]warning me. If, on the contrary, he took care to forewarn me, I have nothing to complain of, but then it is always the same fact expressed in another language.

In sum, *all the scientist creates in a fact is the language in which he enunciates it*. If he predicts a fact, he will employ this language, and for all those who can speak and understand it, his prediction is free from ambiguity. Moreover, this prediction once made, it evidently does not depend upon him whether it is fulfilled or not.

What then remains of M. LeRoy's thesis? This remains: the scientist intervenes actively in choosing the facts worth observing. An isolated fact has by itself no interest; it becomes interesting if one has reason to think that it may aid in the prediction of other facts; or better, if, having been predicted, its verification is the confirmation of a law. Who shall choose the facts which, corresponding to these conditions, are worthy the freedom of the city in science? This is the free activity of the scientist.

And that is not all. I have said that the scientific fact is the translation of a crude fact into a certain language; I should add that every scientific fact is formed of many crude facts. This is sufficiently shown by the examples cited above. For instance, for the hour of the eclipse my clock marked the hour α at the instant of the eclipse; it marked the hour β at the moment of the last transit of the meridian of a certain star that we take as origin of right ascensions; it marked the hour γ at the moment of the preceding transit of this same star. There are three distinct facts (still it will be noticed that each of them results itself from two simultaneous facts in the rough; but let us pass this over). In place of that I say: The eclipse happened at the hour 24 (α-β) / (β-γ), and the three facts are combined in a single scientific fact. I have concluded that the three readings α, β, γ made on my clock at three different moments lacked interest and that the only thing interesting was the combination (α-β)/(β-γ) of the three. In this conclusion is found the free activity of my mind.

But I have thus used up my power; I can not make this combination (α-β)/(β-γ) have such a value and not such another, since I can not influence either the value of α, or that of β, of that of γ, which are imposed upon me as crude facts. [333]

In sum, facts are facts, and *if it happens that they satisfy a prediction, this is not an effect of our free activity*. There is no precise frontier between the fact in the rough and the scientific fact; it can only be said that such an enunciation of fact is *more crude* or, on the contrary, *more scientific* than such another.

4. *'Nominalism' and 'the Universal Invariant'*

If from facts we pass to laws, it is clear that the part of the free activity of the scientist will become much greater. But did not M. LeRoy make it still too great? This is what we are about to examine.

Recall first the examples he has given. When I say: Phosphorus melts at 44°, I think I am enunciating a law; in reality it is just the definition of phosphorus; if one should discover a body which, possessing otherwise all the properties of phosphorus, did not melt at 44°, we should give it another name, that is all, and the law would remain true.

Just so when I say: Heavy bodies falling freely pass over spaces proportional to the squares of the times, I only give the definition of free fall. Whenever the condition shall not be fulfilled, I shall say that the fall is not free, so that the law will never be wrong. It is clear that if laws were reduced to that, they could not serve in prediction; then they would be good for nothing, either as means of knowledge or as principle of action.

When I say: Phosphorus melts at 44°, I mean by that: All bodies possessing such or such a property (to wit, all the properties of phosphorus, save fusing-point) fuse at 44°. So understood, my proposition is indeed a law, and this law may be useful to me, because if I meet a body possessing these properties I shall be able to predict that it will fuse at 44°.

Doubtless the law may be found to be false. Then we shall read in the treatises on chemistry: "There are two bodies which chemists long confounded under the name of phosphorus; these two bodies differ only by their points of fusion." That would evidently not be the first time for chemists to attain to the separation of two bodies they were at first not able to distinguish; such, for example, are neodymium and praseodymium, long confounded under the name of didymium. [334]

I do not think the chemists much fear that a like mischance will ever happen to phosphorus. And if, to suppose the impossible, it should happen, the two bodies would probably not have *identically* the same density, *identically* the same specific heat, etc., so that after having determined with care the density, for instance, one could still foresee the fusion point.

It is, moreover, unimportant; it suffices to remark that there is a law, and that this law, true or false, does not reduce to a tautology.

Will it be said that if we do not know on the earth a body which does not fuse at 44° while having all the other properties of phosphorus, we can not know whether it does not exist on other planets? Doubtless that may be maintained, and it would then be inferred that the law in question, which may serve as a rule of action to us who inhabit the earth, has yet no general value from the point of view of knowledge, and owes its interest only to the chance which has placed us on this globe. This is possible, but, if it were so, the law would be valueless, not because it reduced to a convention, but because it would be false.

The same is true in what concerns the fall of bodies. It would do me no good to have given the name of free fall to falls which happen in conformity with Galileo's law, if I did not know that elsewhere, in such circumstances, the fall will be *probably* free or *approximately* free. That then is a law which may be true or false, but which does not reduce to a convention.

Suppose the astronomers discover that the stars do not exactly obey Newton's law. They will have the choice between two attitudes; they may say that gravitation does not vary exactly as the inverse of the square of the distance, or else they may say that gravitation is not the only force which acts on the stars and that there is in addition a different sort of force.

In the second case, Newton's law will be considered as the definition of gravitation. This will be the nominalist attitude. The choice between the two attitudes is free, and is made from considerations of convenience, though these considerations are most often so strong that there remains practically little of this freedom.

We can break up this proposition: (1) The stars obey Newton's [335] law, into two others; (2) gravitation obeys Newton's law; (3) gravitation is the only force acting on the stars. In this case proposition (2) is no longer anything but a definition and is beyond the test of experiment; but then it will be on proposition (3) that this check can be exercised. This is indeed necessary, since the resulting proposition (1) predicts verifiable facts in the rough.

It is thanks to these artifices that by an unconscious nominalism the scientists have elevated above the laws what they call principles. When a law has received a sufficient confirmation from experiment, we may adopt two attitudes: either we may leave this law in the fray; it will then remain subjected to an incessant revision, which without any doubt will end by demonstrating that it is only approximative. Or else we may elevate it into a *principle* by adopting conventions such that the proposition may be certainly true. For that the procedure is always the same. The primitive law enunciated a relation between two facts in the rough, A and B; between these two crude facts is introduced an abstract intermediary C, more or less fictitious (such was in the preceding example the impalpable entity, gravitation). And then we have a relation between A and C that we may suppose rigorous and which is the *principle*; and another between C and B which remains a *law* subject to revision.

The principle, henceforth crystallized, so to speak, is no longer subject to the test of experiment. It is not true or false, it is convenient.

Great advantages have often been found in proceeding in that way, but it is clear that if *all* the laws had been transformed into principles *nothing* would be left of science. Every law may be broken up into a principle and a law, but thereby it is very clear that, however far this partition be pushed, there will always remain laws.

Nominalism has therefore limits, and this is what one might fail to recognize if one took to the very letter M. LeRoy's assertions.

A rapid review of the sciences will make us comprehend better what are these limits. The nominalist attitude is justified only when it is convenient; when is it so? [336]

Experiment teaches us relations between bodies; this is the fact in the rough; these relations are extremely complicated. Instead of envisaging directly the relation of the body A and the body B we introduce between them an intermediary, which is space, and we envisage three distinct relations: that of the body A with the figure A' of space, that of the body B with the figure B' of space, that of the two figures A' and B' to each other. Why is this detour advantageous? Because the relation of A and B was complicated, but differed little from that of A' and B', which is simple; so that this complicated relation may be replaced by the simple relation between A' and B' and by two other relations which tell us that the differences between A and A', on the one hand, between B and B' on the other hand, are *very small*. For example, if A and B are two natural solid bodies which are displaced with slight deformation, we envisage two movable *rigid* figures A' and B' The laws of the relative displacement of these figures A' and B' will be very simple; they will be those of geometry. And we shall afterward add that the body A' which always differs very little from A', dilates from the effect of heat and bends from the effect of elasticity. These dilatations and flexions, just because they are very small, will be for our mind relatively easy to study. Just imagine to what complexities of language it would have been necessary to be resigned if we had wished to comprehend in the same enunciation the displacement of the solid, its dilatation and its flexure?

The relation between A and B was a rough law, and was broken up; we now have two laws which express the relations of A and A' of B and B' and a principle which expresses that of A' with B'. It is the aggregate of these principles that is called geometry.

Two other remarks. We have a relation between two bodies A and B, which we have replaced by a relation between two figures A' and B' ; but this same relation between the same two figures A' and B' could just as well have replaced advantageously a relation between two other bodies A'' and B'', entirely different from A and B. And that in many ways. If the principles and geometry had not been invented, after having studied the relation of A and B, it would be necessary to begin again *ab ovo* the study of the relation of A'' and B''. That is why geometry is so [337] precious. A geometrical relation can advantageously replace a relation which, considered in the rough state, should be regarded as mechanical, it can replace another which should be regarded as optical, etc.

Yet let no one say: Bat that proves geometry an experimental science; in separating its principles from laws whence they have been drawn, you artificially separate it itself from the sciences which have given birth to it. The other sciences have likewise principles, but that does not preclude our having to call them experimental.

It must be recognized that it would have been difficult not to make this separation that is pretended to be artificial. We know the rôle that the kinematics of solid bodies has played in the genesis of geometry; should it then he said that geometry is only a branch of experimental kinematics? But the laws of the rectilinear propagation of light have also contributed to the formation of its principles. Must geometry be regarded both as a branch of kinematics and as a branch of optics? I recall besides that our Euclidean space which is the proper object of geometry has been chosen, for reasons of convenience, from among a certain number of types which preexist in our mind and which are called groups.

If we pass to mechanics, we still see great principles whose origin is analogous, and, as their 'radius of action,' so to speak, is smaller, there is no longer reason to separate them from mechanics proper and to regard this science as deductive.

In physics, finally, the rôle of the principles is still more diminished. And in fact they are only introduced when it is of advantage. Now they are advantageous precisely because they are few, since each of them very nearly replaces a great numher of laws. Therefore it is not of interest to multiply them. Besides an outcome is necessary, and for that it is needful to end by leaving abstraction to take hold of reality.

Such are the limits of nominalism, and they are narrow.

M. LeRoy has insisted, however, and he has put the question under another form.

Since the enunciation of our laws may vary with the conventions that we adopt, since these conventions may modify even the [338] natural relations of these laws, is there in the manifold of these laws something independent of these conventions and which may, so to speak, play the rôle of *universal invariant*? For instance, the fiction has been introduced of beings who, having been educated in a world different from ours, would have been led to create a non-Euclidean geometry. If these beings were afterward suddenly transported into our world, they would observe the same laws as we, but they would enunciate them in an entirely different way. In truth there would still be something in common between the two enunciations, but this is because these beings do not yet differ enough from us. Beings still more strange may be imagined, and the part common to the two systems of enunciations will shrink more and more. Will it thus shrink in convergence toward zero, or will there remain an irreducible residue which will then be the universal invariant sought?

The question calls for precise statement. Is it desired that this common part of the enunciations be expressible in words? It is clear, then, that there are not words common to all languages, and we can not pretend to construct I know not what universal invariant which should be understood both by us and by the fictitious non-Euclidean geometers of whom I have just spoken; no more than we can construct a phrase which can be understood both by Germans who do not understand French and by French who do not understand German. But we have fixed rules which permit us to translate the French enunciations into German, and inversely. It is for that that grammars and dictionaries have been made. There are also fixed rules for translating the Euclidean language into the non-Euclidean language, or, if there are not, they could be made.

And even if there were neither interpreter nor dictionary, if the Germans and the French, after having lived centuries in separate worlds, found themselves all at once in contact, do you think there would be nothing in common between the science of the German books and that of the French books? The French and the Germans would certainly end by understanding each other, as the American Indians ended by understanding the language of their conquerors after the arrival of the Spanish.

But, it will be said, doubtless the French would be capable of [339] understanding the Germans even without having learned German, but this is because there remains between the French and the Germans something in common, since both are men. We should still attain to an understanding with our hypothetical non-Euclideans, though they be not men, because they would still retain something human. But in any case a minimum of humanity is necessary.

This is possible, but I shall observe first that this little humanness which would remain in the non-Euclideans would suffice not only to make possible the translation of *a little* of their language, but to make possible the translation of *all* their language.

Now, that there must be a minimum is what I concede; suppose there exists I know not what fiuid which penetrates between the molecules of our matter, without having any action on it and without being subject to any action coming from it. Suppose beings sensible to the influence of this fluid and insensible to that of our matter. It is clear that the science of these beings would differ absolutely from ours and that it would be idle to seek an 'invariant' common to these two sciences. Or again, if these beings rejected our logic and did not admit, for instance, the principle of contradiction.

But truly I think it without interest to examine such hypotheses.

And then, if we do not push whimsicality so far, if we introduce only fictitious beings having senses analogous to ours and sensible to the same impressions, and moreover admitting the principles of our logic, we shall then be able to conclude that their language, however different from ours it may be, would always be capable of translation. Now the possibility of translation implies the existence of an invariant. To translate is precisely to disengage this invariant. Thus, to decipher a cryptogram is to seek what in this document remains invariant, when the letters are permuted.

What now is the nature of this invariant it is easy to understand, and a word will suffice us. The invariant laws are the relations between the crude facts, while the relations between the 'scientific facts' remain always dependent on certain conventions.

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