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The Origin of Modern Physical Science: Some Passages from *A Theory of Wonder*

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Abstract: The triumph of the Copernican revolution is commonly associated with the introduction of the scientific method, mainly by Galileo. The nature of science presumably depends on the way observation passes judgment on theory. This is how, according to empiricism, the practice of science improves our worldviews. Some historically inclined philosophers of science, most notably Kuhn and Feyerabend, have insisted on paying attention to what Galileo actually said and did. Shockingly, he drives a dagger through the heart of empiricism: observation does not have such priority over theory, because observation itself *assumes* theory. This is what he argues when dismantling Aristotle's Tower Argument, according to which a stone dropped from a tower falls straight down to the base of the tower. If this is so, the Earth cannot rotate, for it would carry the tower with it, making our observation of the stone's flight wildly different. According to Galileo, to conclude that the stone *really* falls vertically requires the assumption that the Earth does not move – the theoretical issue in question. Given Galileo's proper understanding of the nature of science, I view Feyerabend's principle of proliferation as the realization that a good strategy for the latter is to elaborate radical alternatives and, on their basis, reconsider what counts as evidence. Moreover, a science produced by human brains should be analyzed on the basis of evolutionary theory and neuroscience. From that perspective, we may be able to defend a sensible notion of relativism. These considerations have led me to the main arguments of my new book, **A Theory of Wonder: Evolution, Brain, and the Radical Nature of Science** (*Philosophy of Science*, Vernon Press, Wilmington — Malaga 2021). I hope to entice the

Keywords:

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reader into a discussion of some of the issues developed there.

Empiricism tells us that science succeeds because it follows the scientific method: Observation passes judgment on Theory — supports or rejects it. And much credit is given to the inventor of the method, Galileo. This is a very common account of the origin of modern science. But when historically minded philosophers of science like Kuhn and Feyerabend called our attention to what Galileo actually wrote and did, we were shocked to find out that Galileo instead drives a dagger through the heart of empiricism: he strikes down the distinction between theory and observation. Plain facts like the vertical fall of a stone ruled out the motion of the Earth. But, Galileo argued, to conclude that the stone *really* falls vertically, *we must assume* that the Earth does not move. If it does move, then the stone only *seems* to fall vertically. Galileo thus replaced the facts against the motion of the Earth with facts in agreement with the motion of the Earth.

Moreover, this process is typical during scientific revolutions: i.e., at the origin of new sciences, or new ways of doing science. As it turns out, a good strategy for science is to elaborate radical alternatives then, and on their basis reconsider what counts as evidence. Feyerabend was called irrational for this suggestion. Nevertheless, looking at the practice of science from the perspective of evolution and neuroscience shows that his suggestion is quite rational instead, and explains why science works best as a radical form of knowledge. It also leads to a sensible form of relative truth. And we need that biological perspective because the ways we perceive and are able to conceive of the universe depend on the central nervous system. But evolution could have gone a different way. In terms of perception, it certainly has for many creatures on Earth. And in terms of developing science, it might have done so as well on faraway planets. Completely different forms of thought might be comparable only in terms of how they allow biological beings to perform.

This theory, which I have called evolutionary relativism in some writings, is fully explained in my new book **A Theory of Wonder: Evolution, Brain, and the Radical Nature of Science**. [1] This book aims to determine the best way science can satisfy our sense of wonder by exploring the world. Since a great many new

[1] See Gonzalo Munévar, **A Theory of Wonder: Evolution, Brain, and the Radical Nature of Science,** *Philosophy of Science,* Vernon Press, Wilmington — Malaga 2021.

ideas and issues come up in the book, it would be useful to bring them to the attention of readers in this paper. What follows is not, however, a summary of the book. It is rather a way of enticing readers with new approaches to understanding science. If enticed enough, readers may consult the book for the full arguments.

The Critique of Falsificationism

Falsificationism appeals to the intuitions of many working scientists, particularly Karl Popper's notion that as scientists we come up with hypotheses about the world and then derive predictions from them: if the predictions are wrong, we reject the hypothesis under test; if the predictions are true, then we accept the hypothesis tentatively, until the next text. Nevertheless, Imre Lakatos points out that, at least in its most common version, falsificationism is untenable. Let us consider the example of Halley's Comet. Halley made a detailed record of the path of the comet. On the basis of this path and Newton's physics, he was able to calculate the orbit of the comet and to predict when it would be back. Now, what was relevant to his calculations? Among other things, he took into account the gravitational influences of other bodies on the comet, mainly the Sun's, since the forces that move the comet in its orbit are gravitational. The comet and those other bodies together form a system. From a certain state of that system Halley was able to predict what a future state of the system would be like. Notice, however, that the prediction does not follow unless we assume that *nothing interferes* with the system, as Imre Lakatos pointed out. [2]

Let me explain. Comets typically have very long orbits that take them to the outer reaches of the solar system. In its long journey, Halley's comet might have passed near Neptune, and this would have falsified Halley's prediction, for Neptune's gravity would have thrown it off its orbit, as Neptune is a very massive planet while Halley's comet is barely a few miles across. But Halley could not have taken this into account for the simple reason that Neptune had not yet been discovered. There were many other possible factors that with a bit of bad luck would have kept the comet from its famous appointed round. And the point is that the

[2] See Imre Lakatos, "Methodology of Scientific Research Programmes", in: Imre Lakatos and Alan Musgrave (eds.), **Criticism and the Growth of Knowledge**, *Proceedings of the International Colloquium in the Philosophy of Science*, Vol. 4, Cambridge University Press, London 1970, pp. 91–196.

prediction would have failed then even though Halley's hypothesis — that the comet would behave in accordance with Newton's physics — was correct.

It is clear that Halley's prediction must assume that nothing other than the factors he considered will affect the future state of the system. That is, the prediction requires that the system be closed to outside interference. This is the case for most scientific predictions. We expect a certain event on the basis of a theory, *all other things being equal*. This is a simple point, so what is the problem for the method of falsification? The problem is that when the prediction fails we cannot blame the theory, for things might not have been equal: something might have interfered with the system.

Suppose Neptune had thrown Halley's comet off its regular orbit. Halley's prediction would have failed, but his hypothesis should not be blamed. Falsificationism's mode of inference cannot then be as straightforward as is usually supposed. Three things come together to make the prediction:

If (1) the comet behaves according to Newton's physics

And (2) the path of the comet through the solar system in 1682 has been properly described, and

And (3) only the Sun and the (then known) planets will affect the orbit of the comet

Then (4) the comet will be back in December of 1758.

But if (then unknown) Neptune had interfered, the comet would not have returned on that date.

Therefore, something would be false. But what? Pointing the finger at the theory surely would be arbitrary. And wrong. In this case the failure was caused by the violation of requirement (3).

The falsification advice to scientists on what to do turns out to be too simple-minded. So does the advice on what not to do. The falsificationist believes that nothing could be further from the scientific spirit than *ad hoc* moves to safeguard a theory that conflicts with experience. But let us see how this falsificationist advice would have served the makers of scientific history. According to the Copernican view, for example, the Earth orbits around the Sun. If this is so, the famous fifteenth-century observer Tycho Brahe reasoned, the positions of the stars relative to one another should change as seen from the Earth at different points in its orbit. In other words, the stars should exhibit what is called "parallax motion". But neither Tycho Brahe nor anyone else was able to detect such motion for another one hundred fifty years (and then only with far more powerful instruments). Such failure led Brahe to reject the Copernican system. His conclusion accords with the method of falsification: parallax motion is a logical consequence of the heliocentric view, but it cannot be found, therefore the view is refuted. The same objection had been directed against Aristarchus, a Greek precursor of Copernicus, almost two millennia earlier.

How did the Copernicans preserve their view from refutation? They advanced the notion that the universe was far larger than Tycho had assumed: immensely larger, for if the parallax motion of the stars went undetected, then the size of the Earth's orbit would be insignificant compared to the distance to the fixed stars — just as a man walking in circles around his desk in his study can notice the relative changes of position of the chairs and bookcases in the room, but cannot detect any such changes in a line of trees that he can see far away through his window. We now believe that the Copernicans were right. But at the time the idea seemed inconceivable to Tycho, for by the standards of his day the proposed axis of the Earth's orbit was an extraordinary distance already. Moreover, and this is the most important point, he could not see any motivation for advancing this notion except to save the Copernican view from refutation. Tycho could tell an *ad hoc* move when he saw one.

To clinch the matter, Tycho Brahe also argued that if the universe were of a Copernican size, it would not be possible for the stars to be seen! His argument was based on a relationship between the size of stars and their apparent brightness. Today we know that Tycho was wrong in assuming that the perceived brightness of a point of light against a dark background can tell us much about the size of its source. But as far as anyone could tell at that time, Tycho was right. The Copernicans had no convincing rebuttal to offer. In time their hunch was justified. But their chance had to be purchased at the price of flouting the falsificationist ideal of science. *Ad hoc* moves saved the Copernican view from refutation, and thus gave it the breathing room it needed to develop and eventually show its superiority.

These considerations may serve as warm-up exercises leading up to the highlights Feyerabend points to in his **Against Method**, concerning the distinction between theory and observation, which played itself out in the scientific revolution carried out by Copernicus and Galileo, allegedly the first great triumph of empiricism. [3]

The view that the Earth moves may seem commonsensical to many of us today. But that is only because we are the heirs to a revolution in scientific thought. When the battle was fought, victory was by no means easy. Among the many telling objections against the movement of the Earth perhaps the Tower Argument, presented by Aristotle, in his **On the Heavens**, almost two thousand years earlier, was the strongest of all. [4] It goes as follows. Suppose that you let go of a stone from the top of a tall tower. If the world moves, by the time the stone hits the ground, the tower being stuck in the Earth, will have moved considerably (back then the velocity of rotation of the Earth would have been calculated to be about one milion miles per hour). Thus, there will be a perceptible difference between the initial and final distances from stone to tower. But when we actually look, there is practically no difference at all! We plainly see the stone fall straight down. For the distance to remain constant, if the Earth did move, the stone would have to fall in a parabolic path — something that any fool with even less than average sight can see is not so. Therefore, it is as plain as plain can be that the Earth does not move. The idea that it does makes no sense.

It does no good to talk about gravity and the like, for the appropriate concepts were not developed until years later, and then partly as a result of Galileo's success. Presented with the Tower Argument, what could Galileo say? First, in "The Second Day" of his **Dialogue Concerning the Two Chief World Systems**, he made the argument against his view as strong as possible. [5] For example, equal cannons shooting east and west will send their cannonballs pretty much the same distance, but if the Earth moved, the cannon shooting East should go a lot further. If you shoot a cannon straight up, and the Earth moves, by the time the cannonball falls to the ground, the cannon should have moved a great distance and the cannonball will hit the ground far from it; but obviously that is not so: the cannonball

[3] See Paul K. Feyerabend, **Against Method: Outline of an Anarchistic Theory of Knowledge**, Verso Books, London 1978 (first published by New Left Books in 1975).

[4] See Aristotle, **On the Heavens**, Book II, trans. and ed. Stuart Leggatt, *Aris & Phillips Classical Texts*, Liverpool University Press, Liverpool 1995, Ch. 14, 296b7–24 (written around 350 BCE).

[5] See Galileo, "Dialogues Concerning the Two Chief World Systems: The Second Day", in: Michael R. Matthews (ed.), **The Scientific Background to Modern Philosophy: Selected Readings**, Hackett, Indianapolis 1989, pp. 61–81 (first published in 1623).

will fall back straight down towards the cannon. Galileo then acknowledges that *all the experiments* are on Aristotle's side! This greatly pleases Simplicio, Aristotle's representative in the dialogue, who admiringly tells Salviati, Galileo's representative, that it would appear to be "an impossible feat to contradict such palpable experiences". If these experiments were false, Simplicio asks, "[…] what true demonstrations were ever more elegant?". [6]

That is quite an admission from someone who is introduced as the inventor of the empirical or scientific method in the first chapter of many science textbooks, because of his presumed insistence that observation and experiment are to have precedence over theory. According to Newton's **Third Law for Reasoning in Philosophy** (in those days people made no distinction between science and philosophy), the qualities of bodies determined by experiment ought to be considered universal, therefore the good (natural) philosopher does not consider alternative accounts of the phenomena: "We are certainly not to relinquish the evidence of experiments for the sake of dreams and vain fictions of our own devising". [7]

Nevertheless, Galileo does entertain hypotheses contrary to such powerful experimental results (contrary to Rule III), and without having produced any "other phenomena" (as Rule IV requires) — i.e., no new observations or experimental results. What did Galileo do instead? *He offered a theoretical argument*. He begins by asking what may seem to be a silly question: How do we know that the rock falls vertically? We see it, obviously, as Simplicio points out ("by means of the senses"). But what if the Earth did rotate? How would the rock move then? Galileo's move here anticipates Feyerabend's advice to imagine "*a dream-world in order to discover the features of the real world we think we inhabit*". [8] Salviati gives the answer: The motion would then be a compound of two motions, "one with which it measures the tower, and the other with which it follows it". [9] The real motion would thus be a compound of a vertical and a circular motion. Of course, this is implied: we only observe the vertical motion, since we share, with the rock and the tower, the motion of the Earth. A few pages earlier Galileo had pointed out

[6] GALILEO, "Dialogues Concerning…", p. 73.

[7] Isaac NEWTON, **Principia**, in: MATTHEWS (ed.), **The Scientific Background to Modern Philosophy…**, p. 146 [137–153] (first published in 1687).

[8] FEYERABEND, **Against Method…**, p. 32, my italics.

[9] GALILEO, "Dialogues Concerning…", p. 77.

that any motions that may be attributed to the Earth "must necessarily remain imperceptible to us […] for as inhabitants of the Earth, we consequently participate in the same motions". [10]

It follows, then, that from *seeing* the motion of the stone "you could not say for sure that it described a straight and perpendicular line, *unless you first assumed the Earth to stand still*". [11] But whether the Earth stands still is precisely what is in question. The evidence adduced to show that the Earth stands still *assumes* that the Earth stands still! Aristotle, the great logician, has committed the fallacy of *petitio principii*. [12] His "facts" assumed the theory in question.

In a few pages, then, and without providing one single piece of new empirical evidence, Galileo disposes of the main objection against the very possibility that the Earth rotates, thus creating the stage for the eventual triumph of the Copernican Revolution. By relinquishing the evidence of experiments for the sake of a dream of his own devising, he was able not only to discover important features of the world we thought we inhabited, but eventually to show that such a world was itself a dream.

What conclusions can we draw about the Tower Argument, then? According to Feyerabend, people noticed a phenomenon and *interpreted* it in what they thought was the most *natural* way, i.e., the stone *moves* only straight down. [13] It was this "natural" *interpretation* of the phenomenon, not the phenomenon itself, which contradicted the Copernican view. Galileo did away with the contradiction by providing a *different set of interpretations*. Thus, he constructed a new empirical basis! This new empirical basis, furthermore, is constituted by a *new theory of interpretation*. It is fair to conclude, therefore, that when confronted by facts that refuted his theory, Galileo changed the facts!

On the surface, there was a clash between theory and fact — for, clearly, that the stone falls straight down looked like a fact, if anything did. But what we really were dealing with was a clash between a rather explicit theory (i.e. Copernicus') and a covert theory of interpretation. After close analysis, it turns out that instead

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[10] Galileo, "Dialogues Concerning…", p. 69.

[11] Galileo, "Dialogues Concerning…", p. 77, my italics.

[12] See Galileo, "Dialogues Concerning…", p. 77.

[13] Feyerabend, **Against Method…**, pp. 69–98.

of theory vs. fact, we have theory vs. theory. In any event, the main moral of the story is that observations make theoretical assumptions, and thus it is arbitrary to *always* go along with the judgment of experience, no matter how careful, intersubjective, etc., it may be. (Again, all those requirements were met in the case of the Tower Argument).

Galileo's admiration for Copernicus did not decrease, even though Copernicus, "with reason as his guide […] resolutely continued to affirm what sensible experience seemed to contradict". [14] Reason, it appears, can overturn the verdict of experience. In Galileo's case, as Feyerabend reminds us, [15] he came upon "the existence of a superior and better sense than natural and common sense": the telescope, which then joins "forces with reason". [16]

The second, and major, difficulty for empiricism, as Feyerabend points out, is that Galileo's trust in the telescope required the granting of several *theoretical* assumptions. Images from the heavens would travel immense distances, enter a different medium upon hitting the Earth's atmosphere, work their way through the telescope, and finally be handled by a brain that had never perceived anything like them. To be assured that those images were not significantly distorted, Galileo needed supporting theories about optics, about the nature of light, about the atmosphere, about the interaction between light and a variety of gases, about the telescope, and about perception. We may realize, then, that it was not Galileo's telescopic *observations* that challenged the geocentric view of the universe, but his observations together with a host of assumptions from many supporting or auxiliary sciences that had not yet been invented, and were thus only theoretical guesses at that time. The crucial question was: could experience alone have reconciled the magnitudes of the planets with Copernicus' thesis? If, by experience, we mean sensory experience, the answer is "no". If we allow telescopic experience, then we should remember that such experience could be taken as reliable only if interpreted on the basis of certain theories. The answer, again, is "no".

To make matters worse, most of the auxiliary sciences in question were not within Galileo's reach. Some of them required hundreds of years of development

[14] Galileo, **Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican**, trans. Stillman Drake, *Modern Library Science*, Modern Library, New York 2001, p. 381.

[15] See Feyerabend, **Against Method…**, p. 103.

[16] Galileo, **Dialogue Concerning…**, p. 381.

before they could fully back Galileo's hunches. Thus, to a good empiricist of the day, many of Galileo's theoretical assumptions should have seemed unwarranted.

If we take a more flexible approach, however, the situation does not look so dismal. We have seen that the telescope could be trusted only if we made certain theoretical assumptions. But the same analysis that leads to this result applies to the eye as much as it does to the telescope. The eye is also an instrument. Visual perception is a complex process in which the brain takes into account the "input" not just from the retina, but also from the inner ear and hundreds of skeletal muscles (to determine the position of the body), *and* from the other senses. Think of how vague images suddenly come into focus when we smell the particular scent of a flower in a forest or hear the growling of a dog in a dark street.

The brain does not merely "copy" or "process" the shapes and colors that objects imprint on the retina, as we can easily tell by noticing how often in our visual perception shape and color remain constant. Once we have identified an object as a red apple, we tend to see it that way even if we look at it from an odd angle and in a yellow light (so the frequency of the light bouncing off the apple and hitting the retina is not that of red). And, as we will see later, the brain also uses memory and imagination in making its "pictures" of the world.

That perception should work in these and other complex ways is the result of the history of adaptations of the brains of our ancestors to a variety of environments. And, as extensive as that ancestral history has been, it is quite limited compared to the range of situations that science considers. The extent to which the senses can be "trusted" is thus not a matter for philosophy alone to determine. Psychology tells us of the richness and complexity of perception; neuroscience may help reveal the structures that make such richness and complexity possible; and evolutionary biology may explain how those structures arose and give us clues about where they apply. [17]

Galileo, incidentally, as Feyerabend quotes him, [18] is quite conscious of what is at stake. Time and again he praises Copernicus for resisting the verdict of experience. "There is no limit to my astonishment", Galileo writes, "when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in

[17] See Munévar, **A Theory of Wonder…**, pp. 133–172.

[18] See Feyerabend, **Against Method…**, p. 101.

defiance of the latter, the former became mistress of their belief". [19]

Feyerabend is often considered in conjunction with Thomas Kuhn, though he often not only differs from, but is also very critical of, the latter — even if his starting point is Kuhn's account of the practice of science. An interesting angle presented by that account is that in Kuhn's "normal science", which is research based on an accepted scientific paradigm, we can learn not only what most scientists normally do, but also the roots of the main models of scientific method offered by philosophers of science. For example, in empirical work undertaken to articulate the paradigm theory, we find:

(a) The determination of physical constants (e.g., the universal gravitational constant, Avogadro's number, coefficients, etc.).

(b) The determination of quantitative laws (e.g., Boyle's law).

There is a striking resemblance between these two types of factual research and inductivism. Boyle, for example, took measurement after measurement until he was satisfied of having a basis broad enough for his famous generalization that the pressure and the volume of a gas are inversely proportional. But even in such cases we should note how the paradigm directs the enterprise of fact collection, for Boyle's research made little sense unless his paradigm had first assured him that air was a fluid to which he could apply the elaborate concepts of hydrostatics.

Since these two types of research comprise so much of normal science, it is not surprising that many have taken them as characteristic of the nature of science. But whereas in inductivism the emphasis is placed on the justification that such investigations provide for the theories of science, in Kuhn's scheme they play no such role. The paradigm is not in question. On the contrary, if it were not assumed, these investigations with their inductivist air would make no sense. Apparently, by examining science in its historical context, we not only gain a new understanding of its nature, but also come to understand some of the fixations in the history of the *philosophy* of science.

With this application in mind, let us consider one last type of factual research aimed at articulating the paradigm:

(c) Experiments designed to choose between alternative ways of extending a paradigm to areas closely related to the area of success of the paradigm, but

[19] Galileo, **Dialogue Concerning...**, p. 328.

where the paradigm gives no specific direction as to the most fruitful approach. There were, for example, many plausible ways of extending the caloric theory of heat, e.g. chemical combination, friction, compression, etc.

Hypotheses are then made, tested, and rejected if they conflict with experience. And in the experiments to choose between the different alternatives we can see a strong resemblance to the crucial experiments of the falsificationist. But this is at best a case of *in-house* falsificationism. The paradigm, once again, is not in question. What is in question is merely a particular proposal to extend or articulate the paradigm. Nevertheless, in this and other aspects of normal science we can find the roots of the tendency to see the testing of hypotheses as *the* fundamental characteristic of science. This tendency is reinforced by the spectacular disputes occurring during revolutionary periods (extraordinary science), when alternatives are openly sought and paradigms really brought into question.

At any rate, we are now in a position to realize how it was that some philosophers could look at science and see inductivism in it, whereas others — in part spurred by the epistemological failures of the inductivists — could find falsification to be of the essence. But when looking at science through Kuhn's spectacles, we see that what resembled inductivism and falsificationism were merely the expected functions of the normal practice of a mature science — that is, of research based on a paradigm.

Nevertheless, these "new" things that the paradigm brings to the scientific community are not just additions to the accomplishments of the old view: they often *replace* such accomplishments. To change paradigms may well involve a change in sets of facts about the world as well. Thus, the growth of science need not be cumulative. A revolution in science, Kuhn says, has the nature of a gestalt-switch, of a change of perception about the universe (in the particular field involved). It would be unusual for it to be otherwise, since what used to be seen as a recalcitrant anomaly in one paradigm is now seen as straightforward, perhaps even obvious, in the new one. Choice between paradigms, Kuhn claims, "proves to be a choice between incompatible modes of community life". [20]

If all this is correct, then it does not seem possible to give reasons that are logically or probabilistically compelling for why the new paradigm is better than the

[20] Thomas S. Kuhn, **The Structure of Scientific Revolutions**, 2nd Edition, Chicago University Press, Chicago — London 1970, p. 94.

old one. It is rather a question of providing a clear exhibit of what the new scientific practice will be like. And this exhibit can be extremely persuasive, persuasive enough to encourage the adherents of old ways of doing science to step into the new circle and evaluate its supporting evidence in its terms.

Within research based on a paradigm, i.e. within normal science, the paradigm supplies the standards for evaluating the merits of competing claims. But when the paradigm itself is in question (thus, when the standards themselves are in question), there is no higher authority to which appeal can be made. A successful scientific revolution, then, brings about the establishment of a new scientific order.

Analytical philosophy, the dominant approach in the English-speaking world, has very often proceeded on the basis of the credo that most philosophy can be reduced to the philosophy of language, whether formal or natural language: if we could just be transparent and rigorous about meaning, our philosophical problems would either be solved or just fall by the wayside. Given Kuhn's approach, though, the meanings of scientific terms would depend on how scientists use them in their respective paradigms. It came to seem, then, that the same terms would have different meanings, and so the problem of the incommensurability of meanings was born. Although the book discusses how, on this issue, philosophers have expended much effort for little gain, I will mention how Feyerabend explains its pointlessness.

The issue of difference in meanings was of monumental significance for philosophers of science, because they thought of the latter in terms of derivations, and a derivation is invalid if the meanings of the terms in the conclusion (e.g., conserved masses in Newton) are different from those in the premises (e.g., relative masses in Einstein). But this is a problem for philosophers only. Scientists are not troubled by it in their disputes. For Feyerabend, "incommensurability" simply means that there are no common standards for measurement. The bottom line is that there may be no common sets of facts with which to judge one theory or paradigm superior to another. But when we put the matter this way, we realize that we are actually talking about the possibility of overthrowing its empirical foundations. And this we have done, without semantic mirrors. [21]

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[21] See MUNÉVAR, **A Theory of Wonder…**, pp. 27–48.

Kuhn correctly claims that a comprehensive view is abandoned not because it has anomalies, but because it is replaced by an alternative. Anomalies thus do not refute a paradigm, but they may bring a crisis about if they are thought to be important enough (for then the failure to assimilate them assumes great significance). And Feyerabend largely agrees. [22]

No anomaly, however, Feyerabend points out, is as important as one which a competitor claims to have explained — no anomaly, that is, accentuates more the loss of confidence in the paradigm. The reason is that, as Kuhn believes, a paradigm is accepted on the basis of its promise of future performance — the promise, that is, that it will prove the best way to conceive of the world. When a competing would-be paradigm seems to be doing better, our faith in the *promise* of our anomaly-besieged paradigm may falter. Thus, Feyerabend thought, we will create more crises, and therefore more fruitful change, in Kuhn's own terms, by providing a mechanism to strengthen the anomalies. To accomplish this goal, science should be organized so as to require the *continuous generation of alternatives*. This Feyerabend calls the *principle of proliferation.* [23]

The principle of proliferation, and within its operation, the principle of tenacity (that scientists should continue to work at what seems promising to them), create the conditions for fruitful change and improvement in science. Moreover, they lead to greater human happiness. Therefore, both humanity and science are the better for their presence.

As Feyerabend argues, counter-inductive hypotheses give us evidence that cannot be obtained in any other way. Prejudice is often discovered not by analysis, but by contrast. If, as we have seen, every fact is already viewed in a certain way, and to progress often requires viewing facts in a different way, then we simply need alternative ways of seeing. As for the conflict between those counter-inductive hypotheses and the facts — and it is that conflict that presumably makes them counter-inductive — we should remember that no theory ever agrees with all the facts in its domain. We have already seen why this should be so (e.g., Kuhn's account of how a paradigm is a promise of results and not a collection of them). If such conflict is grounds for throwing out a theory, then we should throw

[22] See Feyerabend, **Against Method…**, p. 202.

[23] See Paul K. Feyerabend, "Consolations for the Specialist", in: Lakatos and Musgrave (eds.), **Criticism and the Growth of Knowledge…**, pp. 197–230.

out all theories. The main reason for not trembling in the shadow of the facts is that facts are constituted by older ideologies, and thus a clash between facts and theories may actually be an indication of progress, an indication that our probe is coming into contact with some of the principles assumed in familiar observational notions. [24]

It is often said that we cannot step outside science to see whether it represents the world. This simple point is supposed to dog the idea that truth is correspondence to reality. And maybe it does. But we may still observe the relationship between our science and the world by comparing our science with an alternative interpretation of what the world is like. As Feyerabend says, "*We need a dream-world in order to discover the features of the real world we think we inhabit* (and which may actually be just another dream-world)". [25] In this, Feyerabend echoes John Stuart Mill. [26] If our present views are right, by criticizing them from another vantage point we come to understand them better. And if they are not right, we gain the opportunity to replace them.

If this is so, however, we come to realize that any idea, no matter how ancient or absurd, is capable of improving our knowledge. This sounds preposterous at first. For example, we finally got rid of all that Aristotelian nonsense in science. Why bring it back? But then, many of the central ideas of modern science were once considered preposterous. Consider, to name only three, heliocentrism, held by Aristarchus; atomism, held by Democritus; and evolution, held by Lamarck and before him by even more disreputable characters. Of course, the modern versions of those ideas are quite different. But the fact of the matter is that thinkers like Copernicus, Dalton, and Darwin found promise in those discredited ideas and took the trouble to develop them. To those thinkers we owe in large part the glory of modern science. Here we can observe in operation both the principle of proliferation and that of tenacity.

Feyerabend arrives at this position not via a mere examination of historical cases, but through a historical examination backed by "an analysis of the relation

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[24] See Munévar, **A Theory of Wonder...**, pp. 27–48.

[25] Feyerabend, **Against Method...**, p. 32, my italics.

[26] See John Stuart Mill, "On Liberty", in: Michael L. Morgan (ed.), **Classics of Moral and Political Theory**, 4th Edition, Hackett Publishing Co., Indianapolis — Cambridge 2005, pp. 1010–1068 (first published in 1859).

between idea and action". [27] Or as Einstein once put it: "The external conditions which are set for [the scientist] by the facts of experience do not permit him to let himself be too much restricted, in the construction of his conceptual world, by the adherence to an epistemological system. He therefore must appear to the systematic epistemologist as a type of unscrupulous opportunist…". [28]

The situation is, then, as follows. According to the rationalist, alias methodologist, alias systematic epistemologist, certain events in the history of science constitute progress. But, Feyerabend points out, for those events to come about some scientists have to be opportunistic enough to adopt "whatever procedure seems to fit the occasion". [29] This means that even the best of methodological rules *must* be violated from time to time. This inherent limitation of all rules implies that nothing can be excluded once and for all. To a methodologist this amounts to an admission that *anything goes*. Therefore, *from the methodologist's point of view*, anarchy will occasionally be essential to science.

Now, what about the rules of logic? Must science obey them? Let's see. An inference is valid if and only if its corresponding conditional is a logical truth. To say that a sentence is a logical truth is to say that it comes out true no matter what the actual truth values (true and false) of its components are. Take, for example, the sentence "I am Martian, or I am not". This sentence is true if I am Martian and true if I am not, since both possibilities are covered. There are no more possibilities; thus, the sentence will be true no matter what.

Valid inferences are then required to have logical truths as corresponding conditionals. The problem is that the conditionals of logic are very peculiar. Indeed, they are not equivalent to those of the sciences or of real life. A conditional of logic, called a "material conditional", has an antecedent (the sentence that follows the "if") and a consequent (the sentence that follows the "then"), just as real conditionals do. It also shares another property with real conditionals: whenever the antecedent is true and the consequent false, the entire conditional is considered false (e.g., "If Feyerabend was born in Austria, he is Chinese").

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[27] Feyerabend, **Against Method…**, p. 17.

[28] Albert Einstein, "Remarks Concerning the Essays Brought Together in this Co-Operative Volume", trans. Paul Arthur Schilpp, in: Paul Arthur Schilpp (ed.), **Albert Einstein: Philosopher-Scientist**, *The Library of Living Philosophers*, Vol. VII, MJF Books, New York 1951, p. 684 [665–688].

[29] Feyerabend, **Against Method…**, p. 10.

Here the similarity ends: material conditionals are true under all other conditions. For example, in the logic of logicians, the following is true: "If the moon is made of cheese, the human population problem is due to an overproduction of storks". The reason is simply that the antecedent is false, and all material conditionals with false antecedents are automatically true. There need be no connection at all between antecedent and consequent. By contrast, in the conditionals of science we often find a causal connection between antecedent and consequent (e.g., "if the mass of that star is one hundred times that of the Sun, it will eventually become a black hole").

The consequence of this line of thought is that material conditionals do not apply in science. And one of the main reasons is precisely that they can be made true simply by having a false antecedent. But, and here we come to the crucial point: a presumably valid inference must have a corresponding conditional that is always true. The determination of such truth must be made, for the logician, in terms of his definition of the material conditional. That is, that determination will give a value of true in all cases in which the antecedent is false. Of course, these cases will include antecedents with contradictions in them — precisely the conditionals that correspond to the most puzzling "valid" inferences. But these cases are inadmissible in science! And since the notion of logical validity is tied to that of the material conditional, such a notion of validity is also inadmissible in science! In other words, the logic of logicians does not apply to science (at least not fully) — and neither, by the way, does it do so to real life.

Nevertheless, as illustrated in this essay, the historical and psychological facets of science do make a great difference; indeed, the tides of historical investigation have washed away the castles that philosophy built on the sands of logic.

Science as Part of Nature

It is high time to acknowledge that human beings are part of nature and that, when analyzing their conceptions of themselves and the world, we should take seriously the lessons that Darwin began to teach us in 1859 with the publication of his **Origin of Species**. [30] In this vein, it pays to recall that science is a product of intelligence, that intelligence is an instrument of adaptation and itself the result of

[30] Charles Darwin, **On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life**, John Murray, London 1859.

evolution. Now, what distinguishes intelligence from other chemical and neural mechanisms of interaction with the world is that intelligence transcends the ability to respond to the immediate demands of the environment, as Piaget made clear. [31] It is this freedom of response that permits intelligence to form sweeping views of the world and the means to criticize them. Science is, of course, a communal enterprise that involves a division of labor and is carried out in a social milieu. It is, thus — to speak perhaps metaphorically, but not inaccurately — a social application of intelligence to the understanding of our world, a world of which we ourselves are part.

The view I will argue for is not like the analogical evolutionary epistemology of Popper, Campbell, Toulmin, and Hull. Nor is it like Quine's naturalized epistemology. In Chapter 8, I go on to sketch a biological view of scientific rationality: a view more in line with the spirit, and occasionally the letter, of Lorenz's thought. [32]

I do not wish to say that science is *like* nature, but rather that it is part of nature. Let me return to my account of the origin, the genesis of science. I claimed earlier that science is a social expression of intelligence in dealing with the world. A characteristic aspect of intelligence, Piaget tells us, is that it allows organisms to transcend the immediate demands of the environment so that they may behave to their greater advantage at a more convenient time and place. This indirect action of intelligence permits us, for example, to evaluate alternative courses of action on the basis of prior experience, and to rehearse future actions in the imagination. Piaget found intelligence to be a powerful instrument of adaptation. His insight is buttressed by an analysis of the neural basis of intelligence. As the complexity of the central nervous system increases, so does the flexibility of its response. Information from the senses can now be rerouted, delayed, and stored; it can be compared with information from other sense modalities, as well as with previous information, and with expectation. As the complexity of the central nervous system increases, so does the number of modes of indirect action. Intelligence, of course, has many facets, but there is one, in particular, that suggests how adaptability may be increased: I am referring to curiosity.

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[31] See Jean Piaget, **The Psychology of Intelligence**, Adams & Co., Littlefield 1972.

[32] See Munévar, **A Theory of Wonder…**, pp. 119–132.

Curiosity is best seen as a form of play (with the environment), and as such it arises in situations that do not demand immediate attention to the environment. Animals play, or exhibit curiosity, not to satisfy hunger or sex drives directly, but because play (and curiosity as a form of it) provides a motivation of its own: it is enjoyable. In trying to satisfy its curiosity, an animal rehearses a wide range of skills, and of combinations of skills, that will later enable it to deal more effectively with the environment. For those skills — cognitive skills, in this case — will permit the animal to either know its environment better or devise strategies by which to accomplish that goal. (By "knowing the environment better" I mean developing means of dealing with the environment that lead to a more successful response). Through curiosity, others can adapt to environments for which their species have not been "designed", and still others, who preserve much of their playful character throughout their lives, can adapt to changing environments.

My suggestion is that we can find the origins of science at the juncture where human curiosity about the world becomes social. Just as we came to hunt in groups — an exercise of another form of social intelligence — now we try to satisfy our curiosity in groups. There are two main reasons why this should be so. The first is simply that to explore our environment in great depth often requires the cooperation of others. An experiment in gravitational physics may have to be carried out beyond the Earth's atmosphere and will involve fields as diverse as rocketry, metallurgy, superconductors, chemistry and orbital dynamics, as well as the general theory of relativity. Even in the same field some projects are much too large to be taken up by a single investigator. At a certain level of sophistication, division of labor becomes of the essence.

The second reason is that the very attempt to satisfy one's curiosity in a specific way may well require the prior existence of an institution devoted to such a goal. One cannot just decide to study the interactions between hadrons and leptons unless one has available to one the possibility of entry into a society committed to a program of research in elementary particle physics; likewise, one cannot just decide to become a milkman in a continent where placental mammals do not exist.

Once it becomes social, the attempt to satisfy our curiosity about the world gains extraordinary power, and so do the skills that result from it. Now, if this general account is correct, we should expect that such a social enterprise (science) would allow us to:

(a) have a more thorough interaction with the environment,

(b) increase the number of environments to which we can adapt, and

(c) adapt to a changing environment.

What increases adaptation (or the potential for adaptation) in one type of organism depends on the sorts of interactions organisms of that type can have with the environment. An opposable thumb may be of great value to animals that exhibit a certain skeletal structure and development of their central nervous system, e.g., humanoids, in many but not all environments. But to other types of animals, say horses or cockroaches, an opposable thumb would be disadvantageous or useless in most typical environments.

Nor should one think that every product of science, or every scientific skill or technique, must be clearly adaptive. Surely, the model of science as arising out of curiosity does not entail such a conclusion. After all, not every skill that an animal develops in its play with the environment will later prove to be of the greatest usefulness. Some of them will be of no use at all, and others will be put to use indirectly. If science is play, as I suggest, we are likely to devise all sorts of games in exploring our world, and some of those games are bound to be very abstract and intricate. A few of those may greatly facilitate the development of some skills that will prove useful in our dealing with one or several environments (e.g., by providing for conceptual, mathematical, or instrumental elaborations of our theories). As it is, Kuhn already pointed out that much scientific work goes into the articulation of the main views we hold.

With this account of the genesis and nature of science in mind, let me now turn to the problem of rationality. Given that science is a communal enterprise with a division of labor, the question of the rationality of science should be asked of science as a whole. This point goes directly against the typical manner in which philosophers have approached the question of rationality: they look at whether this or that great scientist, or research group, adhered to this or that set of methodological rules. But it seems to me that to approach the question in this manner is to commit a reasoning mistake. In trying to determine whether a football (soccer) team is good, we cannot merely look at whether its players are individually good. We wish to know instead the social and structural relations that the team exhibits during its games: whether, in short, it can play as a team. When a player creates space into which another can move to receive the ball and score, the social unit is working well. Even brilliant individual action often depends on good positioning by teammates that keeps the defense guessing about what the next move is going to be. In any event, to ascribe the properties of the individual members to the whole team would be a mistake, and it seems to me that the same is true in science.

I propose that the question "What would it take for science to be rational?" be thought of as equivalent to the question "How should science be structured so as to perform its function?" My evolutionary account forces us to pose the question in this way, and it also suggests how to answer it. To determine how science should be structured or organized so as to perform its function is to determine what it would take for science to enable us to adapt to new environments or to a changing environment, and so on.

We may then easily realize that scientific views are often designed to make sense of a particular environment: that of our experience. But success in one environment, or in one context, does not guarantee success in others. If the environment or context is likely to change, it pays to have a strategy for generating alternative points of view. That is, an organizational requirement of science is that it should allow dissension and the generation of alternatives. Moreover, this requirement of intellectual freedom must be accompanied by another: that scientific views be given a chance to develop. They must begin like all ideas: as small and almost certainly vague. Yet, if we see some promise in them, we should not abandon them just because they are in conflict with the evidence. We may do so, but we should not have to. Otherwise, ideas would never blossom into glorious scientific achievements. Let us recall a key reason why counterevidence need not be always decisive: observation and experiment always have to be interpreted, but the interpretation that makes them into counterevidence may depend on theories that the very development of the new ideas would expose as inadequate.

These two requirements of intellectual freedom — that science must be organized so as to permit, and perhaps encourage, the generation and development of new ideas — must be met by science as a whole, though not necessarily by individual scientists. Some scientists will generate new approaches, others will develop them in a very stubborn fashion, and still others will reject all but the accepted views of the time. Some scientists will be open-minded, and some will not. That does not matter, as long as there is enough room in science for all kinds. If there is, if science does employ a strategy for accepting and developing new ideas, then science will be in a better position to adapt flexibly to new challenges. It will thus permit us to deal with new or changing environments. If so, it will perform its function, the function suggested by my biological account. And, on a very straightforward means-ends analysis of rationality, we ought to conclude that science would then be a rational enterprise.

It should be noted that this means-ends analysis also provides a recipe for solving the contemporary problem of scientific rationality. Two demands were placed upon the epistemologist. The first was that science should proceed in such a manner that its practitioners generate opportune methods and procedures. When viewed from the perspective of my social conception of rationality, science offers precisely a general strategy to improve the chances of accomplishing the desired goal. The second demand was that the first should be met without tying science to the dangers of being ruled by a stagnant elite. The two requirements of freedom under the social conception will reduce such dangers.

Indeed, what I have called the two requirements of intellectual freedom overlap to a great extent with Feyerabend's principles of proliferation and tenacity. [33] What looks like anarchy under a conception that equates rationality with adherence to methodological standards now looks like the very sort of organizational structure that science ought to have. With the shift to a social conception, we also shift from looking for rationality in the choice of theory to finding rationality in the ability to reach certain goals. As happens in science itself, the solving of a problem takes place within a transformation of outlook in the field.

I do not mean to say, incidentally, that irrationality at the level of the individual becomes rationality at the social level. My point is rather that the concept of scientific rationality no longer should be applied to individual scientists. Social properties are social properties. Nevertheless, there are many other ways in which the question of individual rationality may still come up. For example, once a certain view of the world is found promising by a scientist or group of scientists, procedures are devised for developing it further and testing it. Many goals and subgoals must then be reached, and some means may be more effective in reaching those goals. Once more, a means-ends analysis of rationality would be employed.

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[33] See Munévar, **A Theory of Wonder…**, pp. 87–104.

Realism vs. Relativism

Scientific realism is the view that the world has a certain structure, and that it is the function of science to try to uncover it. One important version of scientific realism holds that this view is supported by the success of science because, as Richard Boyd says, truth is the only reasonable explanation for that success. [34] In this he follows Hilary Putnam, who argued that realism "is the only philosophy that doesn't make the success of science a miracle". [35] Such a position has been elaborated from then until the present by a variety of authors, with comments from J. Brown, P. Lipton, S. Psillos, E.C. Barnes, T. D. Lyons, J. Busch, G. Frost-Arnold, and F. Dellsén. [36]

By contrast, *evolutionary relativism* holds that an organism's view of the world depends on its mind, that mind depends on biology, that biology supports a logically impeccable form of relativism, and that success explains truth, not the other way around. [37] This approach is consistent with the history of science and with the science most relevant to understanding the pursuit of knowledge.

Behind the first intuition is the feeling that if realism is not right, pursuing science makes little sense. After all, the business of science is presumably to investi-

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[34] See Richard Boyd, "On the Current Status of Scientific Realism", in: Richard Boyd, Philip Gasper, and John D. Trout (eds.), **The Philosophy of Science**, MIT Press, Cambridge 1992, pp. 195–222.

[35] Hilary Putnam, **Mathematics, Matter and Method**, Cambridge University Press, Cambridge 1975, p. 79.

[36] See James Robert Brown, "The Miracle of Science", *Philosophical Quarterly* 1982, Vol. 32, No. 128, pp. 232–244, https://doi.org/10.2307/2219325; Peter Lipton, "Truth, Existence, and the Best Explanation", in: Anthony A. Derksen (ed.), **The Scientific Realism of Rom Harré**, *Studies In General Philosophy of Science*, Tilburg University Press, Tilburg 1994, pp. 89–111; Stathis Psillos, **Scientific Realism: How Science Tracks Truth**, *Philosophical Issues in Science Routledge*, Routledge, London — New York 1999; Eric C. Barnes, "The Miraculous Choice Argument for Realism", *Philosophical Studies* 2002, Vol. 111, No. 2, pp. 97–120, https://doi.org/10.1023/A:1021204812809; Timothy D. Lyons, "Explaining the Success of a Scientific Theory", *Philosophy of Science* 2003, Vol. 70, No. 5, pp. 891–901, https://doi.org/10.1086/377375; Jacob Busch, "No New Miracles, Same Old Tricks", *Theoria* 2008, Vol. 74, No. 2, pp. 102–114, https://doi.org/10.1111/j.1755-2567.2008.00011.x; Greg Frost-Arnold, "The No-Miracles Argument for Realism: Inference to an Unacceptable Explanation", *Philosophy of Science* 2010, Vol. 77, No. 1, pp. 35–58, https://doi.org/10.1086/650207; Finnur Dellsén, "Explanatory Rivals and the Ultimate Argument", *Theoria* 2016, Vol. 82, No. 3, pp. 217–237, https://doi.org/10.1111/theo.12084.

[37] See Gonzalo Munévar, **Evolution and the Naked Truth: A Darwinian Approach to Philosophy**, *Avebury Series in Philosophy*, Ashgate Publishing Ltd, Aldershot 1998.

gate what is *out there*. If talking about what is out there is pointless (e.g., because realism is false or nonsense), then science has no particular significance. Popper, for instance, talks about realism as a metaphysical presupposition of doing science. [38] Of course, trying to show that realism is true has been such a messy affair that many philosophers, particularly in the twenty twentieth century, rose to great levels of sophistication in how they sought to wash their hands of the issue. Nevertheless, realism seems to come as standard equipment where most philosophers are concerned. As we have seen, according to Boyd, only realism can explain why scientific success is not a mystery. [39]

Absolute Truth vs. Success

Popper and Boyd notwithstanding, when we look into the history of science, we find a disturbing separation between "absolute truth" and success. Greek astronomy postulated a universe with two basic spheres: the Earth in the center, and the sphere of stars on the outer edge. This model has been an excellent guide to navigation. Only in the last century did modern science surpass it (with the help of electronic inventions, such as satellites that indicate position, etc.). That is, during more than two thousand years a completely false point of view has had great success in an area of major importance to the survival and welfare of human beings.

Boyd's claim that only realism can explain the success of science thus seems less than compelling. To make matters worse for him, the most successful scientific field of the last century is quantum physics, and quantum physics in its orthodox interpretation is decidedly anti-realist. At least, that is what Niels Bohr, the foremost thinker in the field, explicitly claimed: "[…] an independent reality in the ordinary physical sense can neither be ascribed to the phenomena nor to the agencies of observation". [40] By "phenomena", Bohr does not mean the sense data of philosophers but the subatomic objects being *measured*. Phenomena are always the result of specific interactions with specific measuring equipment, but we

[38] See Karl R. Popper, **Objective Knowledge: An Evolutionary Approach**, Oxford University Press, Oxford — New York 1972, p. 203.

[39] See Boyd, "On the Current Status…", pp. 195–222.

[40] Niels Bohr, "The Quantum Postulate and the Recent Development of Atomic Theory", *Nature* 1928, No. 121, p. 580 [580–590], https://doi.org/10.1038/121580a0.

should not therefore conclude that they are two separate things, one of which gives us information about the other, as Bohr insists on the "*impossibility of any sharp separation between the behavior of atomic objects and the interaction with the measuring instruments which serve to define the conditions under which the phenomena appear*". [41]

This interactionist view has many unpleasant philosophical consequences. One of them results from the fact that some measuring arrangements exclude others. In some, an electron will behave as a wave, in others as a particle, but never as both. It all depends on what kind of experimental arrangement we employ, and we thus end up with *complementary* descriptions, as happens in the Two-Slit Experiment. *Real* things, however, supposedly cannot behave this way. If we are realists, we want to know *the* way the electron really is. These complementary descriptions, moreover, cover the gamut of Heisenberg's uncertainty relations. One experimental arrangement allows us to measure the momentum of a particle, but this brings about some uncertainty as to its position, and so on. Given all of these considerations, it seems unjustified to ascribe an independent reality to those subatomic objects. Furthermore, insisting on their independent reality requires that we do away with complementary arrangements (and, therefore, descriptions) inconsistent with that reality. Yet we then rule out discovering important aspects of the subatomic "realm". As Bohr puts it: "In fact, it is only mutually exclusive of two experimental procedures, permitting the unambiguous definition of complementary physical quantities, which provides room for new physical laws, the coexistence of what might at first sight appear irreconcilable with the basic principles of science". [42]

Brains and Knowledge

Some may think that evolution supports scientific realism, for even at the level of perception it seems clear that (approximately) true or veridical perceptions give an organism a greater chance to survive. Organisms with false perceptions in-

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[41] Niles Bohr, **Atomic Physics and Human Knowledge**, John Wiley and Sons, New York 1958, pp. 39–40, my italics.

[42] Niels Bohr, "Can Quantum Mechanical Description be Complete?", in: Stephen Toulmin (ed.), **Physical Reality: Philosophical Essays on Twentieth-Century Physics**, Harper & Row, New York 1970, p. 139 [122–143].

stead are likely doomed. Moreover, by "truth" they mean correspondence to the way things really are. Nevertheless, considerations from evolutionary biology, neuropsychology, and other scientific fields make implausible the claim that veridical ideas or perceptions are required to explain evolutionary success. Going further back in the history of science, Galileo's comments on perceptual realism should no longer come as a surprise:

> […] tastes, odors, colors, etc., so far as their objective evidence is concerned, are nothing but mere names for something which resides exclusively in our sensitive body[…] so that if the perceiving creatures were removed, all of these qualities would be annihilated and abolished from existence. [43]

What may surprise some is that Galileo's attitude, albeit with an evolutionary turn, is quite common today among scientists whose work is obliged to take perception into account. As the neuroscientist V.S. Johnston tells us, we must abandon the common-sense view of reality, because:

> […] although the external environment is teeming with electromagnetic radiation and air pressure waves, without consciousness it is both totally black and utterly silent. Conscious experiences, such as our sensations and feelings, are nothing more than evolved illusions generated within biological brains. [44]

Consider an example: the color spectrum is linear, while our experience of the spectrum is not. Perceptually, red and green are "opposing" colors, but the wavelength difference between them is barely 1/150,000,000,000 m. Why do we perceive such an extraordinarily small difference, then? Evolution gives us the reason: green "corresponds" to a band of frequencies reflected in normal white light by chlorophyll molecules, whose detection would have given an evolutionary advantage to our remote ancestors. Perception of other colors such as red and blue helps fix the detection of chlorophyll at dawn and dusk and in cloudy days. In a different place, where vital resources depend on different chemical compounds, evolution may bring about a different perceptual parceling of the color spectrum. This means that the "normal" color experiences of creatures on Earth and on Carnap II (a yet-to-be-discovered planet in Andromeda) may be quite different, even

[43] Galileo, "The Assayer", in: Matthews (ed.), **The Scientific Background to Modern Philosophy…**, pp. 56–57 [56–61](first published in 1623).

[44] Victor S. Johnston, **Why We Feel: The Science of Human Emotions**, Perseus Books, Cambridge 1999, p. viii.

at the hypothetical level of the "sense data" so beloved by the logical positivists.

When looking at Figure 1. we clearly see a three-dimensional image. But we know it is a two-dimensional drawing. When I project it on a screen, I run my hand up and down and sideways over it. My hand confirms what my intellect affirms: it is not three-dimensional. But try as we might, we see it as three-dimensional. That is, we see it as it is not. Of course, there exist reasons why our visual mechanism works this way, and one of them is that, as we will see below, there are evolutionary advantages, at least on occasion, for perceiving falsely.

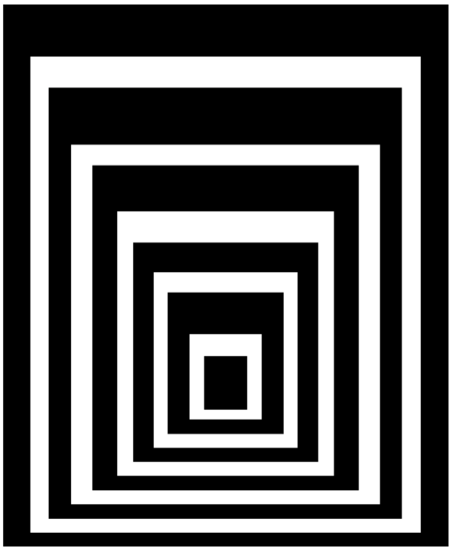

Figure 1. Drawing courtesy of Ruoyu Huang.

The extraordinary complexity of the brain mechanisms that produce our perceptions contrasts starkly with the ready-to-be-used character of the perceptions they produce. Instead of the realist impulse, it seems more sensible to suppose that the brain constructs perceptions that will allow us to interact promptly and successfully (at least much of the time) with our environment. Thus, when we look at a scene full of snow (or dots, or letters), but there is a different feature in our blind spot, we do not see the feature: instead, our brain fills the blind spot with more of the same — snow, or dots, or letters. [45] This construction by the brain is more than a bet on what is most likely to be in front of us. Indeed, the

[45] See Susan Blackmore, **Consciousness**, Oxford University Press, New York 2002.

brain takes the direct response of its own neurons to sensory input and transforms it into a meaningful perception that is in important respects different from that response. As Edelman and Tononi point out:

> […] the activity of many neurons in sensory […] pathways can be correlated with rapidly varying details of a sensory input […] but do not seem to map to conscious experience. For example, patterns of neural activity in the retina and other early visual structures *are in constant flux* and correspond more or less faithfully to spatial and temporal details of the rapidly changing visual input. However, *a conscious visual scene is considerably more stable*, and it deals with properties of objects that are *invariant* under changes in position or illumination, properties that are easily recognized and manipulated. [46]

Daniel Hoffman's remarks on illusions show how far the brain may go in constructing perception. In **Figure 1.**, for example, we know that the perception is radically different from the sensory input. Perceptions, he explains with a very apt metaphor, are like the icons that appear on user-friendly computer screens. The actual software and hardware those icons "stand for" are complicated and beyond the knowledge of most computer users. The colorful, easy-to-identify icons constructed by the programs are not like anything in particular — they are not like the programs, certainly — but they are convenient "symbols" for them. The relation is arbitrary, since the symbols could have been quite different. Similarly, the relation between our perceptions and those "real" objects they are supposed to be about (and that, on some accounts, produce them) is arbitrary. They could have been quite different also. Let us consider the case of color.

As we have seen, even creatures somewhat similar to terrestrial land mammals may chromatically divide the world differently from the way we do, and might thus experience different colors when looking at our "green" grass and "red" apples. But we need not travel to other planetary systems. Right here in the Earth's ocean we find shrimp with as many as eleven primary colors — instead of our three primary colors (red, green and blue), which combine to produce the others, according to Young and Helmholtz's trichromatic theory of color vision. The biological basis for this theory is provided by the relative activation of our three types of cone. [47] Some women actually have four types of cone, and are thus likely to perform more color discriminations than men. The shrimp mentioned

[46] Gerald Edelman and Giulio Tononi, **A Universe Of Consciousness: How Matter Becomes Imagination**, Basic Books, New York 2000, p. 141, my italics.

earlier may share our planet, but their "world" is quite different from ours, and so are their environmental challenges and opportunities. It should not be surprising, then, that their color experience should be different as well. Moreover, there are also the better-known cases of birds, snakes, and insects that *see* portions of the electromagnetic spectrum that remain dark to us (ultraviolet or infrared). Being able to do so is clearly advantageous to them. These considerations suggest that the "reason" for perception is pragmatic, and that there is nothing intrinsic about "greenness" or "redness" (as regards corresponding to essential "features" of the world).

The present evolutionary line of thought is strengthened by the discovery that perception works through exaggeration, particularly when small differences of degree are perceived as drastic differences in kind. This process of radical, false contrast is the same as that employed by space science in its observation of other worlds (and of our own planet, for that matter). I have in mind so-called "false color", in which arbitrary bands of electromagnetic frequencies can be assigned colors arbitrarily to help us determine at a glance patterns of global temperatures. We can also photograph contiguous regions made of materials of slightly different hues of brown and show them instead in clear-cut patterns of, say, purple and gold.

The mammalian brain, in particular, has evolved an appropriate structure. As Paul Churchland explains it, the lateral geniculate nucleus (LGN), for example, "projects a massive cable of ascending axons forward to your visual cortex. Curiously, the neurons in your visual cortex project ten times as many descending axons to make synaptic connections within the LGN". [48] As he says, this pattern is "widespread throughout the brain". "Higher" centers in the cortex can, thus, by means of these descending axons, affect "lower" LGN neurons' response to stimuli with previous information, concerns, etc. What this means is that previous states of the brain partially determine your present perception, often by tilting the arrangement of the otherwise confusing patterns of light hitting your retina in favor of one interpretation found meaningful on the basis of previous experience.

[47] See Christof Koch, **The Quest for Consciousness: A Neurobiological Approach**, Roberts & Company Publishers, Englewood 2004.

[48] Paul M. Churchland, **The Engine of Reason, the Seat of the Soul: A Philosophical Journey into the Brain**, *Bradford Books,* MIT Press, London — Cambridge 1996, p. 99.

Perception is also often influenced by evolutionary considerations, many of which do their work in subcortical areas. Francis Crick suggested that the thalamic reticular nucleus plays a big role in the processing or filtering of potential perceptual consciousness — a suggestion recently vindicated. M. Halassa and his team individuated a complicated neural circuit that also includes the basal ganglia. [49] Most of the activity of this circuit concerning perception consists in a massive inhibition of incoming signals. In addition, the signals that are favored involve movement, size, and bright coloring, as well as other properties that would be relevant to being able to survive or adapt to the environment. For example, Tadin and other autors found inhibition of the detection of large objects in favor of the perception of the motion of small objects. [50]

This construction of perception through brain structure makes use of many neuronal networks, including those that involve emotions. Emotions can, of course, interfere with useful perception, but they may also provide the key to resolving perceptual ambiguities. The reason they can do so is that they provide us with what Edelman and Tononi call "value systems". In neuronal terms, out of several possible interpretations emotions will tilt the perceptual system towards those that are most significant for, say, our survival (so a tiger's face will suddenly stick out of the jungle's canopy). Not unlike perception itself, emotions also work by exaggeration, by tending to react very strongly to subtle differences in the situations in which we find ourselves. [51] Once again, exaggeration and contrast, rather than "true" representation, are the keys to success.

Emotions motivate us to action. They do so by exaggeration, by tending to react very strongly to subtle differences in the situations in which we find ourselves. In this, the brain adapts us to the world as it does with perception. Nevertheless, wise people advise us to let reason prevail over emotion. The reasonable conclusion is that emotion plays a role in effective reasoning, at least when it comes to

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[49] See Ralf D. Wimmer, L. Ian Schmitt, Thomas J. Davidson, Miho Nakajima, Karl Deisseroth, and Michael M. Halassa, "Thalamic Control of Sensory Selection in Divided Attention", *Nature* 2015, Vol. 526, No. 7575, pp. 705–709, https://doi.org/10.1038/nature15398.

[50] See Duje Tadin, Woon Ju Park, Kevin C. Dieter, Michael D. Melnick, Joseph S. Lappin and Randolph Blake, "Spatial Suppression Promotes Rapid Figure-Ground Segmentation of Moving Objects", *Nature Communications* 2019, Vol. 10, article number: 2732, https://doi.org/10.1038/s41467-019-10653-8.

[51] See Johnston, **Why We Feel…**, p. 87.

practical reasoning.

On our planet many animals have senses that are quite different from ours. The luna moth, for example, sees in the ultraviolet. To human eyes, the male and female moths look pretty much alike. But the moths themselves detect some very vivid patterns that distinguish them. Other animals perceive the world through even more drastically different senses: vipers detect heat, bats navigate with sonar, and the main interactions with the world of some fish are based on electric fields.

Electroreceptors are particularly interesting. They work by analyzing the distortions to the electric waves or pulses the fish sends out when they return. The fish needs a sophisticated system to distinguish its own returning signals from those emitted by other fish, particularly members of its own species, as the latter could be rivals or possible mates. Entire social and courtship rituals depend on the proper manipulation of the fields (e.g., turning them down in the presence of a friendly fish). The world appears very different to them than it does to us. We depend, for instance, on the perception of surfaces that reflect light, which to the fish may not be an important consideration (those surfaces may be transparent to the electroreceptor), while the changes in mood that are given away by changing electric fields may well be.

An even more important lesson is that such different senses are likely to require different brain structures. As intelligence develops, it follows the paths opened up to it by the structures with which the animal interprets the world, including its social world, and which the animal uses to cope with that world. This is not a merely theoretical point. We can find such differences in structure in the brains of ordinary fish and fish that perceive using electric fields. Of particular interest is the clear segregation between relay cells and pacemaker cells in the brains of the latter. We encounter there a radically different type of brain. An intelligent creature whose main sensory modality is electric rather than visual will have patterns of thought completely foreign to us.

What this line of reasoning establishes, as the book explains, is not that *all* (perceptual and conceptual) frames of reference are equally good, but rather that, no matter how good a frame is, others may be equally good, in the sense of the level of interaction with the world that they permit the relevant species to enjoy. Given that, it would be arbitrary to say of any one frame that it gives us *the way* *the world really is*. If it turns out that there is only one "good" frame (e.g., the human one), we still cannot say that its view of the world is *the* correct way of viewing the world, for it is a mere accident that natural history did not bring about different but equally good frames (or ones that would have been better than ours). The difficulty is not due to lack of information, for there is no new information that, added to any one view, could turn it into the uniquely proper representation of the world. And when every possible view fails to represent "the world" correctly, we seem to face a dilemma: either the world is unrepresentable, or else the expression "the world" is a mere convenience — there is no truth of the matter there, as is discussed in the book.

The views of "the world" produced by different frames may thus be *complementary* in a sense akin to Bohr's. It is possible, then, to produce information in one frame that is not logically, conceptually, theoretically, or mathematically equivalent to any produced in another, even if it is presumably about the same aspect of "reality". (They are "equivalent" only when that word is understood as a synonym of "analog", which is not the relevant sense here). As Bohr pointed out, the wave and the particle descriptions are in no relevant sense *equivalent*. An analogous situation (as in quantum physics) may thus obtain between descriptions of the "world" produced in mutually exclusive frames of reference.

It is worth emphasizing that there is nothing common to waves and particles for us to discover that would provide a more complete description of the subatomic realm. So, there is no information we lack: failure to arrive at "reality" is a consequence of complementarity, not of ignorance on our part. My suggestion is, not surprisingly, that since analogous conditions obtain in the case of the different conceptual biological frames, we should arrive at the same sort of conclusion: our talk of reality is misplaced — there is no truth of the matter "out there".

Not all brains are equal. Some are better at certain tasks than others, depending, of course, on what structures they have and how those structures function alone and in concert with other brain structures. How do those structures come about? In a variety of ways. While still in the mother's womb, for example, neurons grow in the new brain, led by their growth cones, which are attracted to certain chemicals and move in the direction of the strongest signal. A slight difference in genes can alter the balance of those chemicals in the new brain and thus the structures that result. For example, visual area V1 in the occipital cortex is emphasized considerably in an animal for whom quick and sharp appraisals of three-dimensional structures are crucial aspects of their environments (e.g., the flying fox), but not so developed in the mouse. There is some overlap of structure in these cases, but we can imagine easily that as perceptual structures dominate the brain to a large extent, they affect the overall function of the brain. It also happens that, in the development of the brain, to emphasize a structure (or function) means to make a choice at the expense of others. Furthermore, as natural selection slowly shapes the evolution of the brain, a change in emphasis, which leads to a change in structures, provides a different context, a different niche. And given a different evolutionary context, other new structures are more likely to arise, creating even larger differences. For example, when an organism acquires the ability to react to small amounts of chemicals in the atmosphere or oceans (smell), several new structures will be favored over others (such as neurons to carry the information, connections to combine it with the sense of taste, others to synchronize it with an internal sense of position, and still others to allow the organism to act on the perception of those chemicals quickly).

Doing things in one way, then, starts to exclude doing them in another. At appropriate phylogenetic distances, e.g., between humans and electric fish, the brains have several incompatible structures and functions. At even larger evolutionary distances, one type of intelligence will be mutually exclusive of certain others. Their resulting approaches in dealing with the world will thus also be mutually exclusive. (We even experience that within our own species: interpreting a situation in terms of waves excludes interpreting it in terms of particles). Nevertheless, they both may give equally fruitful information about the world. They are thus "complementary" in a sense akin to Bohr's.

Neither uncertainty relations, nor the notion of complementarity, reflect ignorance on our part. These are precisely the conditions described earlier, conditions that led to the conclusion that, since "real" objects could not behave this way, we should not speak of "reality" in the subatomic world. My suggestion is, not surprisingly, that since analogous conditions obtain in the case of different conceptual frames, we should arrive at the same sort of conclusion: our talk of reality is misplaced — there is no truth of the matter "out there".

This is the view at which I arrived many years ago as a result of my failed attempt to develop an interactionist epistemology along the lines of Popper's scientific realism. It was Paul Feyerabend who first drew my attention to the similarity between my evolutionary relativism and Bohr's epistemological position regarding quantum mechanics — a position that I had not fully appreciated until then. I do not mean to suggest, though, that Bohr's epistemology is on the whole similar to mine. The views of his that I favor were clearly confined by him to the description of the behavior of atomic objects, and he might very well have looked unsympathetically upon a generalization of those views to the entire field of empirical knowledge. He did try to extrapolate the concept of complementarity to a few other areas of experience, without much acceptance anywhere. The significant difference, it seems to me, is that the principle of complementarity made eminent sense where the classical notion of reality was found wanting by the quantum phenomena. In the philosophical view that I propose, the classical notion of reality is found wanting even for macroscopic phenomena. This finding requires a certain amount of reflection, and in that reflection an analog of Bohr's principle of complementarity helps us understand the possibility of equally worthy yet non-equivalent frames.

The reasoning I have employed is also analogous to that used by Einstein concerning some important conceptual consequences of his Special Theory of Relativity. In evolutionary relativism we can show that (a) our perceptions and conceptualizations of the world are relative to a biological frame of reference (or, rather, to a biologico-social frame of reference), and (b) that there is no preferred frame. Likewise, in the Special Theory of Relativity, (a) mass, length, and time are relativized to an inertial frame of reference, and (b) there is no preferred inertial frame of reference. From the fulfillment of these two conditions, (a) and (b), we conclude that mass, length, and time are relative properties and so cannot have absolute values. I trust I have used an analogous mode of reasoning to establish the relativism of perception, intelligence and science.

This suggests that the notion of performance can be fruitfully tied to the notion of understanding — particularly scientific understanding, which in turn suggests a biological theory of relative truth. I will introduce that theory by means of an illustration.

Let me suppose that when I perceive a mango, I see it as golden-red, taste it as juicy and delicious, and find it beautiful enough to make it the subject of a still-life painting. Successful perceptions of the mango lead me to suppose that they best serve me for dealing with that portion of the world (the mango). Imagine, now, that beings of a very different kind have perceptions of the mango different from mine, though just as successful as mine. Upon coming to know of these beings' perceptions, should I stop trusting my perception of the mango? Should I replace it by the perceptions those beings have? The answer in both cases is "no". For I have already said that this is the best way I can perceive that section of the world. Thus, learning of the other beings' perceptions would lead me to conclude that I do not perceive "the way the mango really is", simply because doing so would require me to claim my frame of reference as the preferred one, and that would be arbitrary. Nonetheless, this conclusion does not obligate me to change my perceptions of the mango.

In this example, my perceptions best exploit the resources of my genotype (or rather, of the genotype of beings like me) in dealing with a typical environment. Whenever the resulting performance is as satisfactory as in the case of the ideal perceptions of the mango, we tend to think that the world must be just as we perceive it. We then feel entitled to speak of true representations. Our talk of truth is warranted by the successful interaction with the world, given our frame of reference. A "little green man", though, can also have successful interactions with the mango, but since his frame of reference is drastically different from ours, his perceptions will also be different. Nevertheless, that success will entitle him just as much to speak of truth.

Of course, in our conceptualizations of the world — e.g., in our scientific views — we seldom, if ever, reach the level of sufficiency or satisfaction given in the case of the mango. But when we do approach it, we speak of truth. Human science is ultimately a variety of human behavior, and human behavior is part of the human phenotype. And yet, at least in the case of humans, we should really speak of phenotypes, since the plasticity of human behavior is such that there could be many expressions of the genotype even in the same environment. It seems to me also that some phenotypic expressions better exploit the resources of our genotype in a given environment. Likewise, some scientific viewpoints (with their complex machinery of practices, experimental procedures, and so on) permit us to exploit better the resources of our genotype in a given environment (e.g., in dealing with the dynamics of bodies). In other words, some viewpoints enable us to realize more of our potential for performance. In this biological context, a viewpoint is said to be relatively true when it approaches the limits of the resources of the genotype. When a theory allows us to deal with the world in a great variety of ways, when thinking that the world accords with the theory leads to continuing success, when this capacity for performance clearly surpasses that of its competitors, then we come to think that the world must be so. And in a limited domain we may not be able to conceptualize the world any better. We conceptualize the world as powerfully as in the earlier example we perceive the mango. It is then that we may speak of truth.

This account explains why it may be worthwhile to make distinctions between what is true and what is untrue. On my account, we would say that a viewpoint is true because the interaction (with the world) that results is (or seems to be) of very high quality, and greatly superior to its alternatives. This is not to say that we have finally arrived at the way things really are, but merely that our "picturing" of the world approaches the level of quality exemplified earlier by our perception of the mango. This "picturing", however, just like that perception, is relative to a frame of reference, and thus the kind of truth involved is a relative one.

The relative (or seemingly absolute) truth of a viewpoint depends on its success, not the other way around. Certain views hold us in a strong grip because they permit a strong, successful interaction with the world. I suggest that it is that grip under those conditions of successful interaction that seems to have a special character — one that is what philosophers have sought to explain by invoking correspondence theories of truth.

Few views are so successful that they are accepted on the basis of a clearly superior track record. They are accepted because, in a few instances, the success achieved is felt to be so striking that many members of the discipline find that way of doing things extremely promising. That is, they are accepted on the basis of a promise of performance, rather than overall performance. After a group takes up a way of thinking about the world and elaborates it to the point that its performance begins to approach the limit of the potential of the genotype in the relevant environments, its truth seems "evident" to all those concerned. There are also cases in which that limit is not approached, but the scientists committed to the point of view are unable to think about the world in any other way, and so keep on feeling that the truth must lie somewhere along the path they have undertaken.

There are, in addition, cases in which a point of view, if developed, would have better exploited the resources of the genotype — and so, years later, we feel that an opportunity has been missed. Moreover, I suppose there are cases in which the superiority of a point of view goes unrecognized. All the sensible things that philosophers wanted to convey with the old correspondence-based notions can be conveyed with this relativistic and evolutionary one.

It is in this sense that I accept the truths of evolutionary theory, neuroscience, and other scientific views I am sympathetic to as being relative. To alien beings elsewhere, engaged in completely different modes of interaction with the universe, evolutionary thinking, say, of any kind resembling ours might not make sense within the bounds of their conceptual equipment. But to beings like us it does. Or so I believe. I would say similar things about the truth of my philosophical position, if called upon to do so, and I would adduce as evidence precisely the evolutionary and other scientific arguments I have provided so far.

Such evolutionary relativism has some features in common with pragmatism's conception of ideal truth as approaching a limit. But they differ in that on the evolutionary account that limit may well be a horizon that recedes.

Treating science as human behavior, i.e., as part of the human phenotype, allows us to put into perspective the question of its rationality. [52] Organisms as simple as bacteria may change their behavior radically as their environments change — from, say, being impoverished to being rich in nutrients (from preying on their competitors to avoiding them instead). The second phenotype does not "follow" from the first in a logical or rational way. The organisms simply undergo a radical change of posture toward the environment. Similarly, our scientific views at a given point in time need not be continuous with those that replace them, although in some cases there might be quite a bit of continuity. In any event, looking at the history of science from a naturalistic perspective gives us some strong hints of a new conception of rationality.

Once again, this view does not depend on a mere analogy with evolution, as others have proposed. After all, what does follow, from even a close analogy to evolution? Surely not that science is rational. Being like the evolution of life, *which is not itself rational*, cannot suffice. Thus, the very approach seems wrongheaded. To make matters worse, all such proposed analogies have broken down upon close inspection.

I much admire Feyerabend's keen insight and skillful use of the history of science to help us understand how to approach the pursuit of science. Nevertheless, it has struck me that his own contributions could be enhanced by looking at sci-

[52] See Munévar, **A Theory of Wonder…**, pp. 119–132.

ence through the lens of neuroscience as construed in the context of evolutionary biology.

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