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PASSION AND CONTROVERSY IN SCIENCE

by

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My title may sound shocking, for the first thing you know about science is that it is dispassionate. Science is said to describe facts which are there whether you like them or not; facts derived from the experience of things that can be seen and touched. Scientific knowledge is therefore regarded as matter-of-fact, impersonal, objective, in sharp contrast to our moral convictions, religious beliefs and other persuasions that are charged with emotion.

The father of empiricism, John Locke, has said: Whatever is not capable of demonstration by experience "is not, unless it be self-evident, capable to produce knowledge, how well-grounded and great soever the assurance of faith may be wherewith it is received; but faith it is still, and not knowledge; persuasion and not certainty." [1] How can then passion ever participate in producing knowledge? How can ~~it~~ indeed do aught but adulterate objective demonstration by an admixture of unwarranted persuasion?

Yet there is on record the overwhelming elation felt by scientists at the moment of discovery, elation of a kind which only a scientist can feel and science alone can evoke in him. Kepler testified to it in his "Harmonices Mundi", published in 1619, in which he announced the discovery of his "Third Law", connecting planetary periods and distances. He writes:

> " - nothing holds me; I will indulge my sacred fury; I will taunt mankind with the candid confession that I have stolen the golden vases of the Egyptians, in order to build of them a tabernacle to my God, far indeed from the bounds of Egypt. If you forgive me, I shall rejoice; if you are angry, I shall bear it; the die is cast, the book is written, whether to be read now or by posterity I care not: it may wait a hundred years for its reader, if God himself has waited six thousand years for a man to contemplate His work."

We have the image of Archimedes jumping out of his bath to rush through the streets of Syracuse shouting "Eureka!". And we have it on record how Louis Pasteur rushed out likewise from his laboratory when he first separated laevo and dextro rotatory tartrates, and fervently embraced the laboratory assistant in the corridor.

The outbreak of such emotions in the course of discovery is well known, but they are not thought to affect the outcome of discovery. Science is regarded as objectively established in spite of its passionate origins. But in my title I mentioned passions <u>in</u> science, and I do actually want to show that scientific passions are no mere psychological by-play but have a logical function which contributes an indispensable element to science. They respond to an essential quality in a scientific statement and may accordingly be said to be right or wrong depending on whether we do acknowledge or deny the presence in it of that quality.

What is this quality? Passions charge objects with emotions, making them repulsive or attractive; positive passions affirm that something is precious. The excitement of the scientist making a discovery is an <u>intellectual</u> passion, telling that something is intellectually precious and, more particularly, that it is <u>precious to science</u>. And this affirmation forms part of science. The words of Kepler which I quoted were not a statement of fact, but neither were they merely a report of Kepler's personal feelings. They asserted as a valid affirmation of science something else than a fact: namely the scientific interest of certain facts, the facts just discovered by Kepler. They affirmed indeed that these facts are of immense scientific interest and will be so regarded so long as knowledge lasts. Nor was Kepler deceived in this majestic sentiment. The passing centuries have paid their cumulative tribute to his vision, and so, I believe, will the centuries yet to come.

The function which I attribute here to scientific passion is that of distinguishing between demonstrable facts which are of scientific interest, and those which are not. And hence also of serving as a guide in the assessment of what is of higher and what

1. John Locke, "Third Letter on Toleration".

of lesser interest; what is great in science, and what relatively slight. I want to show that this appreciation depends ultimately on a sense of intellectual beauty; that it is an emotional response which can never be dispassionately defined, any more than we can dispassionately define the beauty of a work of art or the excellence of a noble action.

It is true that the scientific value of an observation usually increases with its generality. But unique facts may also possess profound scientific significance. Take Tycho Brahe's discovery of a nova, a new fixed star, in 1572, or the synthesis of urea by Wöhler in 1828. The great scientific value of these discoveries lay in the fact that they fundamentally changed our conception about the nature of things. The birth of Tycho's new star proved that the empyrean was subject to change. Its occurrence invaded the last area of the universe still reserved to the rule of divine principles and annexed it to the realm of accident and necessity, a fit subject of empirical science. By this discovery Tycho anticipated Newton's demonstration that the same laws governed the stars in their courses and the swing of a clock's pendulum. Similarly Wöhler by his synthesis of urea broke through the barrier between the inanimate and the living. He proved that no 'life-force' was required for the production of organic matter, and thus opened the path to biochemistry.

Discoveries like those of Tycho or Wöhler are valued for the breadth of their implications, even though they establish no new general laws. They offer something more vague and also more profound: namely, a truer understanding of a large domain of experience. Generality is indeed but an aspect of profundity in science, and profundity itself, as we shall see, but an expression of the feeling that we are making a new, more extensive, contact with reality.

The following case of scientific glory blighted by the later progress of science will illustrate this point. In 1914 T. W. Richards, then Professor of Chemistry in Harvard, was awarded the Nobel Prize for his determination of some atomic weights with an accuracy far surpassing that which had been achieved before. His results have never been contested. But in 1932 Frederick Soddy could write of them that they appear now "of as little interest and significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty." It had been realised that the value of atomic weights resulted from the accidental proportion in which the constituent isotopes happen to be present in the element as found in nature. A magnitude that had seemed to characterise a deep seated feature of the universe, had turned out to have no such bearing. Though factually correct it had proved deceptive, because - contrary to expectation - it did not correspond to anything substantial in nature. When the exact atomic weight of an element ceased to be of interest to science, what had seemed important had turned out to be trivial.

Though not definable in precise terms, scientific value can as a rule be reliably assessed. Its appraisal is required and depended upon every day in the process of advancing and disseminating science. Referees consulted by journals have to judge whether the scientific interest of a contribution would justify the expenses of its publication. Others have to decide whether the award of a research grant is worth while. Scientists must be able to recognise what is manifestly trivial, just as what is manifestly false. When the distinguished German physicist Friedrich Kohlrausch (1840-1910) declared in a discussion about the aims of natural science, that he would be pleased to determine accurately the speed of water running through the gutter, he was talking utter nonsense. He made no false statement of fact, but he completely misjudged the nature of scientific value; for the accuracy of an observation does not in itself make it valuable to science.

The foolish promise made by Kohlrausch was of course not his true intention. He was merely expounding a false theory of science more consistently than is usual; relying on it, no doubt, that the errors of philosophy are only ridiculous and its extravagances do not influence our lives.[1] But in doing so he demonstrated involuntarily that such absurd conclusions can be avoided - without inconsistency - only by abandoning altogether the ideal of a strictly objective science.

To this revision of our scientific ideal we may find a clue in Kepler's title: "Harmonics of the World". It suggests that we should acknowledge our capacity for recognising scientific value by our sense of harmony - by our emotional response to the intellectual beauty of science. And since no part of science can be said to be beautiful unless it is also believed to be true, we must claim for this emotional

1. Hume, A Treatise on Human Nature, Part IV, Section VII.

response also that it makes contact with reality. A discovery is beautiful if it reveals a new vision of reality. This is the news to which the scientist's heart goes out: this the object of his scientific passion.

Discovery reveals new knowledge, but the new vision which accompanies it is not knowledge. It is less than knowledge, for it is a guess; but it is more than knowledge, for it is a foreknowledge of things yet unknown and at present perhaps yet inconceivable. Our vision of the general nature of things is our guide for the interpretation of all future experience.

Such guidance is indispensable. Theories of the scientific method which try to explain the establishment of scientific truth by any purely objective procedure are doomed to failure. Any process of enquiry unguided by intellectual passions would inevitably spread out into a desert of trivialities. Our vision of reality to which our sense of scientific beauty responds must suggest to us the kind of questions that it should be reasonable and interesting to explore. They should recommend the kind of conceptions and empirical relations that are intrinsically plausible and which should therefore be upheld - even when some evidence seems to contradict them; and tell us also, on the other hand, what empirical connections to reject as specious, even though there is evidence for them, and even though we may as yet be unable to account for this evidence on any other assumptions.

In fact, without a scale of interest and plausibility based on a vision of reality nothing can be discovered that is of value to science; and only our sense of scientific beauty, responding to the evidence of our senses, can evoke this vision. Such is the selective function of scientific passion.

This function is continuous with another which comes out clearly in the part of Kepler's text which precedes the words I quoted before. He writes there:

> " What I prophesied two-and-twenty years ago, as soon as I discovered the five solids among the heavenly orbits - what I firmly believed long before I had seen Ptolemy's Harmonics - what I had promised my friends in the title of this fifth book, which I named before I was sure of my discovery - what sixteen years ago I urged to be sought - that for which I have devoted the best part of my life to astronomical contemplations, for which I joined Tycho Brahe . . . at last I have brought it to light, and recognised its truth beyond all my hopes . . . . So now since eighteen months ago the dawn, three months ago the proper light of day, and indeed a very few days ago the pure Sun itself of the most marvellous contemplation has shone forth - nothing holds me; I will indulge my sacred fury . . . "

It is clear from this that intellectual passions not merely affirm the existence of harmonies which foreshadow an indeterminate range of future discoveries, but that they can also evoke intimations of specific discoveries and sustain their persistent pursuit through years of labour. The appreciation of scientific value merges here into the capacity for discovering it; even as the artist's sensibility merges into his creative powers. Such is the heuristic function of scientific passion.

But the words of Kepler show us also that this truth-bearing passion is far from infallible. Kepler rejoices in his discovery of the five solids among the heavenly orbits; he thought that the solar distances of the six planets known to him corresponded to the sizes of the successive Platonic bodies as measured by the radii of inscribed and circumscribed spheres. This is nonsense, and we would regard it as nonsense today, however accurately it corresponded to the facts: merely and simply because we do not believe any longer that the fundamental harmonies of the universe are disclosed in numbers and geometrical figures. But though this view of reality led Kepler astray in this case, it was close enough to the truth to guide him aright to the discovery of his three laws of planetary motion. Therefore Kepler remains a great scientist to us, in spite of his erroneous reference to the Platonic bodies. It is only when he talks of such things as the mind residing in the sun which listens to the planets, and puts down in musical notation the several tunes of the planets, that we no longer regard him as a scientist, but as a mystic. We draw here a distinction between two kinds of error, namely between scientific guesses which have turned out to be mistaken, and unscientific guesses which are not only false, but incompetent.

Scientists - that is creative scientists - spend their lives in trying to guess right. They are sustained and guided therein by their heuristic passion. We call their work creative because it changes the world as we see it, by deepening our understanding of it. The change is irrevocable. A problem that I have once solved can no longer puzzle me; I cannot guess what I already know. Having made a discovery, I shall never see the world again as before. My eyes have become different; I have made myself into a person seeing and thinking differently. I have crossed a gap, the heuristic gap which lies between problem and discovery.

To the extent to which discovery changes our interpretative framework, it is logically impossible to arrive at it by the continued application of our previous interpretative framework. In other words, discovery is creative also in the sense that it is not to be achieved by the diligent application of any previously known and specifiable procedure. Its production requires originality. The application of existing rules can produce valuable surveys, but they can as little advance the principles of science as a poem can be written according to rule. We have to cross the logical gap between a problem and its solution, therefore, by relying on the unspecifiable impulse of our heuristic passion, and must undergo as we do so a change of our intellectual personality. Like all ventures in which we comprehensively dispose of ourselves, such an intentional change of our personality requires a passionate motive to accomplish it. Originality must be passionate.

But this passionate quest seeks no personal possession. Intellectual passions are not like appetites; they do not reach out to grab, but set out to enrich the world. Yet such a move is also an attack. It raises a claim and makes a tremendous demand on other men; for it asks that its gift - its gift to humanity - be accepted by all. In order to be satisfied, our intellectual passions must find response. This universal intent creates a tension. We suffer when a vision of reality to which we have committed ourselves, is contemptuously ignored by others. For a general unbelief threatens to evoke a similar response in us which would imperil our own convictions. Our vision must conquer or die.

Like the heuristic passion from which it flows, the persuasive passion too finds itself facing a logical gap. To the extent to which a discoverer has committed himself to a new vision of reality, he has separated himself from others who still think on the old lines. His persuasive passion spurs him now to cross this gap by converting everybody to his way of seeing things, even as his heuristic passion had spurred him to cross the heuristic gap which separated him from discovery.

We can now see the great difficulty that may arise in the attempt to persuade others to accept a new idea in science. To the extent to which it represents a new way of reasoning, we cannot convince others of it by formal argument, for so long as we argue within their framework, we can never induce them to abandon it. Demonstration must be supplemented therefore by forms of persuasion which can induce a conversion. The refusal to enter on the opponent's way of arguing must be justified by making it appear altogether unreasonable.

Such comprehensive rejection cannot fail to discredit the opponent. He will be made to appear as thoroughly deluded, which in the heat of the battle will easily come to imply that he was a fool, a crank or a fraud. And once we are out to establish such charges we shall readily go on to expose our opponent as a metaphysician, a Jesuit, a Jew or a Bolshevik as the case may be or - speaking from the other side of the Iron Curtain - as an 'objectivist', an 'idealist' and a 'cosmopolitan'. In a clash of intellectual passions each side must inevitably attack the opponent's person.

I shall recall here three controversies to illustrate this. They occurred in the Nineteenth Century and their outcome has had an effective part in developing our present sense of scientific value.

The first of these conflicts, though brief, was momentous. The quixotic attack of the young Hegel on the empirical method of science and his swift defeat at the hands of the scientists was one of the great formative experiences of modern science. In the year 1800 a band of six German astronomers led by Bode set out to search a new planet to fill a gap between Mars and Jupiter in the numerical series of planetary distances discovered by Titius and known as Bode's Law. The series is obtained by writing down the number 4, followed by the series $3+4$, $2.3+4$, $2^2.3+4$, $2^3.3+4$ . . .

etc. this gives for the first seven places: 4, 7, 10, 16, 28, 52, 100, which can be shown to correspond pretty well to the relative distances of the six planets known in 1800, provided you leave out the fifth number. Setting the distance of Mercury arbitrarily at 4 you have the table:

Bode's Law.

	observed	predicted
Mercury	(4)	(4)
Venus	7	7
Earth	10	10
Mars	15	16
. . . ?		28
Jupiter	52	52
Saturn	95	100

The young Hegel poured scorn on an enquiry following up a regularity of numbers which, being meaningless, could only be accidental. On the grounds that nature, shaped by immanent reason, must be governed by a rational sequence of numbers, he postulated that the relative spacing of the planets must conform to the Pythagorean series 1, 2, 3, 4, 9, 16, 27. This would predict a large gap between the 4th and 5th planet, i.e. Mars and Jupiter. The quest for a planet to fill this gap was therefore nonsensical.[1]

However, on January 1st, 1801, Bode's party of astronomers discovered the small planet Ceres in the region in question. Since then over 500 small planets were found in that neighbourhood,[2] and it may be that these are fragments of a full sized planet that took this place before.

Hegel was discomfited and the astronomers triumphed uproariously. This was all to the good, for it confirmed a juster sense of scientific value. But we should realise that it had little else to support it. Whether Bode's Law has any rational foundation or has been fulfilled so far by mere coincidence (as Hegel had said) is still open to question today: opinions have changed on the subject repeatedly during the last 15 years.[3] So Hegel may have been right in rejecting the astronomers' grounds for their search for a new planet. Nor was Hegel's series refuted by the discovery of Ceres in the gap between Mars and Jupiter. For on examining the figures one finds - rather surprisingly - that Hegel's series, like Bode's, agrees very much better with the relative distances of the planets if you leave out its fifth member; it is easy to confirm this by aid of the table illustrating Bode's law. Hegel was presumably too angry with the astronomers to notice that his own theory fully endorsed their enterprise, and nobody seems to have looked at the figures since then.

Yet I agree that the astronomers were right and Hegel was wrong. Why so? Because the astronomers' guess lay within a conceivably scientific system and so it was a kind of guess to which astronomers as scientists are entitled. It was a <u>competent</u> guess, and - if Bode's Law has any truth in it - even a true guess; while Hegel's inference was altogether unscientific, <u>incompetent</u>. Fortunately Hegel guessed wrong and the astronomers, though their guess was perhaps unjustified, did hit the mark. But even if Hegel's guess had proved right and the astronomers' wrong, we would still reject Hegel's vision of reality and cleave to that of the astronomers.

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1. Hegel, Dissertatio philosophica de Orbitis Planetarum (1801) <u>Werke</u>, Berlin, 1834 vol. IXVI, p.28

2. H. H. Turner, <u>Astronomical Discovery</u>, 1904, p.23.

3. An attempt to interpret Bode's law rationally by deriving it from a theory of the planetary system was made by C. F. von Weissäcker in 1943 (Zs. für Astrophysik Vol. 22, 1944, p.319). But from a later paper it appears that the problem is still in flux (see C. F. von Weizsäcker Festschrift der Akademie der Wissenschaften in Göttingen 1951, p.120).

certainly
revulsion against Hegel's <u>Naturphilosophie</u> was violent and lasting. Empiricism henceforth ruled unchallenged. But unfortunately the empirical method of enquiry - with its associated conceptions of scientific value and of the nature of reality - is far from unambiguous, and consequently, conflicting interpretations of it had ever again to fight each other from either side of a logical gap.

In his Doctoral Thesis, presented in 1875 to the university of Utrecht, J. H. van t'Hoff had put forward the theory that compounds containing an asymmetric carbon atom are optically active. In 1877 there appeared a German translation of this work with a commendatory introduction by Wislicenus a distinguished German chemist and an authority on optical activity. This publication evoked a furious attack from Kolbe, another leading German chemist, who had recently published an article "Signs of the Times", [1] in which he castigated the decline of rigorous scientific training among German chemists; a decline which, he said, had led to a renewed sprouting of

> "the weeds of a seemingly learned and brilliant but actually trivial and empty Philosophy of Nature, which, after having been replaced some 50 years ago by the exact sciences, is now once more dug up by pseudo-scientists from the lumber room of human fallacies, and like a trollop, newly attired in elegant dress and make up, is smuggled into respectable company, to which she does not belong."

Now, in a second paper [2] he gave as a further example of this aberration an account of van t'Hoff's work which "he would have ignored like many other efforts of its kind" but for "the incomprehensible fact" of its warm recommendation by so distinguished a chemist as Wislicensus. So Kolbe wrote:

> "A certain Dr. J. H. van t'Hoff, employed by the Veterinary Academy at Utrecht, appears to have no taste for exact chemical research. He found it more convenient to mount Pegasus (borrowed no doubt from the Veterinary Academy) and to proclaim in his <u>La Chimie dans l'espace</u> how on his daring flight to the chemical Parnassus the atoms appeared to him disposed in world space."

Kolbe's comment on the introduction given by Wislicenus to van t'Hoff's theory reveals the principles of his criticism in further detail. Wislicenus had written of "this real and important step in the advancement of the theory of carbon compounds, a step which was organic and internally necessary." Kolbe asks: What is "the theory of carbon compounds? What is meant by saying that "this step was organic and necessary"? And he goes on: "Wislicenus has here expelled himself from the ranks of exact scientists and has joined instead the nature philosophers of ominous memory who are separated only by a slender 'medium' from the spiritists."

Scientific opinion eventually repudiated Kolbe's attack on van t'Hoff and Wislicenus, but his suspicion of speculative chemistry ("paper chemistry") continued to be shared by most of the leading chemical journals, which refuse up to this day contributions containing no new experimental results. In spite of the fact that chemistry is largely based on the speculations by Dalton, [3] Kekule and van t'Hoff, which were initially unaccompanied by any experimental observations, chemists still remain suspicious of this kind of work. Since they do not sufficiently trust themselves to distinguish true theoretical discoveries from empty speculations, they feel compelled to act on a presumption which may one day cause the rejection of a theoretical paper of supreme importance in favour of comparatively trivial experimental studies. So difficult is it even for the expert in his own field to distinguish scientific merit from incompetent chatter by applying the criteria of empiricism.

Nor does this apply only to purely theoretical discoveries. The great controversy on the nature of alcoholic fermentation which, starting in 1839, went on for almost forty years, showed that the verification of an experimental observation may run into precisely the same difficulties. From 1835 to 1837 no less than four independent observers (Caignard de la Tour, Schwann, Kützing and Turpin) had reported that yeast produced during fermentation was not a chemical

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1. Kolbe, Journ. für praktische Chemie, <u>14</u>, 268 (1877).
2. Kolbe "Zeichen der Zeit II" Journ. für prakt. Chem. <u>15</u>, 473, (1877). The above summary of the first paper is quoted from the second paper.
3. For the case of Dalton, see H. E. Roscoe and A. Harden, <u>A New View of the Origin of Dalton's Atomic Theory</u>, London, 1896, p.50.

precipitate, but consisted of living cellular organisms which multiplied by budding, and they had concluded that fermentation was a living function of yeast cells. [1] But this went against the overmastering intellectual passion of contemporary scientists. In 1828, Wöhler had synthetised urea from inorganic materials and had triumphantly disproved thereby the existence of powers hitherto ascribed exclusively to living beings. Liebig had followed suit by laying the foundations of a chemical approach to all living matter, and Berzelius had recognised that platinum could speed up reactions occurring in its presence, in the same way in which fermentation was caused by yeast. All three great masters poured scorn on the claims which they regarded as a fantastic resurgence of the kind of "vitalism" they had banned for ever. [2] Wöhler and Liebig published an elaborate skit making fun of these absurd speculations.

In 1857 Pasteur entered the list on the side of the "vitalists". His investigations on yeast and putrefaction involved him at the same time in another fierce controversy of longer standing, the question of 'spontaneous generation'. In this, too, he was on the side considered at the time reactionary (and which is still so considered in the Soviet Union), which denied that living beings could be produced experimentally from dead matter. [3]

The reason why both these controversies dragged on indefinitely is revealed by a remark of Pasteur's concerning his own arguments for regarding fermentation as a function of the living cells of yeast: "If anyone should say that my conclusions go beyond the established facts (he wrote) I would agree, in the sense that I have taken my stand unreservedly in an order of ideas which, strictly speaking, cannot be irrefutably demonstrated." [4] This order of ideas was therefore separated by a logical gap from that entertained by Liebig, Wöhler and many other great men of his time. The schism was eventually bridged by a conceptual reform induced by Buchner's discovery in 1897 of zymase in the liquor squeezed out of yeast cells. The agent of fermentation was proved a dead catalyst of the kind imagined by Liebig and Berzelius, but it also proved a vital organ of yeast cells, as Pasteur and his precursors since Caignard de la Tour had affirmed; the new conception of intra-cellular enzymes combined these two aspects. [5]

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1. R. J. Dubos, Louis Pasteur, 1950, p.120-121.

2. Wöhler and Liebig, Annalen der Pharmacie, Vol.29, 1839, p.100.

3. See for example the violent attacks on Pasteur by Pisarev published in 1865. (A. Coquart Dmitri Pisarev, Paris 1946 p.336 ff). Experiments acknowledged today in the Soviet Union as proofs of the Spontaneous generation of cellular organisms were carried out by Lepeshinskaia (see Th. Dobshansky, Proceedings of the Hamburg Congress on Science and Freedom, London. 1955, p.219).

4. R. J. Dubos op. cit. p.128
J. B. Conant Pasteur's and Tyndall's Study of Spontaneous Generation, Harvard Univ. Press 1953 suggests (p.15) that the most convincing evidence for the impossibility of spontaneous generation is to be found "in the whole fabric of the results of the study of pure [bacterial] cultures in the last sixty or seventy years". The author implies here that all the experiments made to decide this question from the inception of Spalanzani's studies in 1768 up to 1880-1890 could be interpreted in terms of either opposing systems of thought.

5. The way an apposite new conception can reconcile two alternative systems of interpretation which hitherto completely excluded each other, is illustrated by Braid's conception of 'hypnosis', which acknowledged the reality of the very features of Mesmerism which were hitherto taken to prove its fraudulence, while rejecting the evidence for 'animal magnetism' which had been regarded as its claim to scientific solidity, but was in fact illusory.

The great scientific controversies which I have just recalled were conducted in passionate accents, as was inevitable between contestants who shared no common framework within which a more impersonal procedure could be followed. Kolbe could not argue against van t'Hoff. He quoted with ironical glee van t'Hoff's description of the disposition of atoms in spirals, which to him was sufficient evidence that the new theory was a tissue of fancies. From his own point of view he was right in refusing to enter into any detailed argument on these lines, since he denied that one could argue rationally in terms of such wild ideas. The ironical travesty by which Wöhler and Liebig replied to the papers of Caignard de la Tour, Schwann and others, claiming that fermentation is a function of living yeast cells, sprang from the same view that an argument believed to be wholly specious cannot be seriously discussed point by point. A Western scientist challenged to answer Lysenko's biological theories would similarly refuse to discuss them on the Marxist-Leninist grounds on which they are put forward; while Lysenko would refuse to consider the statistical evidence for Mendelism on the grounds that "in science there is no place for change".[1]

There are serious questions still open today concerning the nature of things. At least, I believe them to be open, though the great majority of scientists are, as usual, convinced that the view which they hold is right and meet any challenge to it with scorn. A notorious example is offered by extra sensory perception. The evidence for it is ignored today by science in the hope that it will one day find some trivial explanation. In this they may be right, but I would respect those too who think they may be wrong. And no profitable discussion is possible between the two.

Another example. Neurologists today accept almost without exception the assumption that all conscious mental processes can be interpreted as epiphenomena of a chain of material events occurring in the nervous system. Some writers, like Professor Kapp, have tried to show that this is logically untenable, and I myself believe he is right. But to my knowledge only one neurologist, namely Professor J. C. Eccles, has gone so far as to amend the neurological model of the brain by introducing an influence by which the will intervenes to determine the choice between possible alternative decisions.[2] This suggestion is scornfully ignored by all other neurologists, and indeed, it is difficult to argue profitably about it from their point of view.

A similar schism is present today between the ruling school of genitics, which explains evolution as a result of a haphazard sequence of mutations, and writers like Graham Cannon in England, Dalcq in Belgium, Vandel and others in France, who consider this explanation quite inadequate and support the assumption of a harmonious adaptive power controlling the most important innovations in the origin of phyla.

Some people may listen to these illustrations with impatience, for they believe that science provides a procedure for deciding any such issues by systematic and dispassionate empirical investigations. However, if that were clearly the case, there would be no reason to be angry with me. My argument would have no persuasive force, and could be ignored without anger.

At any rate, let me make quite clear what I have urged here. I have said that intellectual passions have an affirmative content; they affirm the scientific value of ~~facts~~ certain facts, as against any lack of such interest in others. This <u>selective</u> function - in the absence of which science could not be defined at all - is closely linked to another function of the same passions in which their cognitive content is supplemented by a conative component. This is their <u>heuristic</u> function. The heuristic impulse links our appreciation of scientific value to a vision of reality, which serves as a guide to enquiry. Heuristic passion is also the mainspring of originality - the force which impels us to abandon an accepted framework of interpretation, and commit ourselves by the crossing of a logical gap, to the use of a new framework.

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1. Pravada, August 10, 1948. Quoted by Sidney Hook "Marx and the Marxists". Anvil Publication, New York, 1955, p.235.

2. Eccles, "<u>The Neurophysiological Basis of Mind</u>", Oxford 1953, p.271-286

Finally, heuristic passion will often turn (and have to turn) into <u>persuasive</u> passion, the mainspring of all fundamental controversy. I am not advocating the outbreak of such passions. I do not like to see a scientist trying to bring an opponent into intellectual contempt, and to silence him in order to gain attention for himself; but I acknowledge that such means of controversy may be tragically inevitable.

I certainly, affirm that passion, and controversy moved by passion, must continue in science. And I would add to this my belief, that if this amendment to our conception of science be accepted, it would have to be followed by others which would lead to a fairly comprehensive revision of our philosophy of science.