In the pursuit of science: The 'subjective' element*

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Several facets of 'subjectivity' in science, from abstract value judgement to mundane human level interactions, are discussed vis-à-vis its traditional 'objective' image.

There is an almost instinctive perception in any unsuspecting mind that the pursuit of science is necessarily an objective exercise, intensely rational in its scope, and singularly free from any 'preconceived' notions. Indeed, the standard scenario of an interplay of theory versus experiment for determining the outcome of any idea in science, major or minor, is a *prima facie* evidence for this basic perception. In the language of Gerald Holton[1], this 'playground' is the so-called *contingency plane*, one in which theory (the calculus of logic and mathematics) is depicted as an ordinate, and experiment (empirical matters of fact and 'data') as the abscissa. Formally, this is called the 'inductive method', one in which a five-step process is involved, according to the physicist Fredrich Dessauer[2]. These five steps are simple, and in conformity with common sense[1,3]: (i) hypothesize a provisional statement; (ii) refine and structure the hypothesis; (iii) draw logical conclusions or 'predictions' which can stand experimental check; (iv) then check the predictions against the 'data' (experiment); (v) if both tally within experimental limits, then a warrant is available for claiming a 'universal validity' for the result.

On the face of it, this looks like an airtight procedure, with hardly any scope for intrusion of any other degree of freedom. However, a little reflection would show that such a picture is a gross oversimplification. For, one may ask, what is the nature or rather the origin of the hypothesis itself on whose basis the theorizing and experimentation were planned and executed? More specifically, what is the source of the original induction[1] or idea? It is here that an 'undefined' ingredient is suspected to have (unobtrusively) entered the fray already! This ingredient is quite distinct from the two-component (theory versus experiment) characterization of the 'induction process' introduced above. It is far more nebulous than the straight-jacket theory–experiment classification, and often 'resists' a quantitative definition. And yet it is a vital component of the structure of modern science, and has all along played a vital underlying role in the shaping of scientific theories since the Kepler–Newton days. Kepler had a twofold philosophy of the Universe (by fiat?), viz. (i) a mathematical harmony on the one hand and (ii) a central theological order on the other (no 'proof' offered here!). Newton too had his five rules[1,3], of which the first four are quite 'rational' (simplicity and economy of description; uniformity of Nature; universality (no exceptions); new hypotheses disallowed against contrary evidence). However, he had a fifth rule, not explicitly stated, concerning the very definition of a hypothesis.

Holton in his book[1] calls it the thematic component, which cannot be 'tested' in the sense of theory vs experiment, yet cannot be wished away[3]. One might then wonder if such a concept goes against the standard philosophy of Karl Popper, viz. the essential characteristic of any science is the 'falsifiability' of its theories. A possible answer is that no obvious contradiction is involved, since the Popperian criterion applies only to the structured part of the hypothesis or 'themata' in the form of a 'theory' which must face confrontation against experiment, towards eventual victory or defeat. On the other hand, the 'themata' is more like the 'spirit' or soul behind its structured form (the theory), and cannot be contained within the four walls of the laboratory. It resides in the human mind: it can reassert itself, or make a comeback in another form, or disappear from the scene altogether, all depending upon the nature of the outcome of its 'present incarnation' (the structured theory) with experiment. The point is that it is basically 'free', much like the proverbial '*die Gedanken*':

'Die Gedanken sind frei; wer kann sie erraten?
Sie fliegen vorbei; es nechliche schatten.
Kein Mann kann sie wiessen; keine Yaeger erschissen,
Es bleibeth dabei; die Gedanken sind frei.'

It is this thematic element that turns out to be a subjective concept inasmuch as it resides in the human mind with all its complex dimensions, which cannot be quantified. It is mostly concerned with a sort of 'value judgement' on the 'quality of ideas' which (by experience?) are deemed relevant for the 'understanding of nature' and those which are not. For example, past experience strongly suggests that certain attributes like 'beauty' and 'naturalness' which cannot be quantified as such (but 'felt' by the mind all the same!) have all along played a vital role in shaping the course of science, but only their sufficiently structured forms, say in the form of some well-defined principles, such as the 'principle of extremum' in the area of physical sciences, have been successfully confronted with experiment. Indeed, it is even possible to classify the thematic element somewhat more systematically into different categories in accordance with their 'performance', i.e. quantitative contact with 'data', in the contingent plane[1]. In particular, three such categories have been suggested[3,4] – principles, postulates and prejudices[4] – in accordance with the nature of successful contact with data (after suitable structuring) in descending order, and the resultant 'degree of confidence' thus generated with respect to their credibility within the scientific community. In this respect principles have undergone so many acid tests through continuous feedback from experiment that their speculative or 'subjective' aspects have by and large been forgotten, and they are now regarded as an integral part of the scientific discipline concerned. Postulates, on the other hand (though highly structured), have not quite made the grade

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The subjective element: value judgement of concepts

At the other end of the spectrum lie certain themata, termed prejudices[3,4], which, despite their strong appeal to intuition, are somewhat nebulous in character: they resist an unambiguous formulation. On the other hand, their sweep seems to be even wider if anything, and confidence in their intrinsic power (despite their nebulosity) has somehow been sustained (often strengthened) due to past experience. Some famous examples in this highly subjective category are well within the theme of this discussion so as to stand some repetition[3,4]:

(i) 'Naturalness' as a selectivity criterion (impossible to define, yet easy to feel in a given context).

(ii) 'Beauty' and 'simplicity' in physical concepts (same problem).

(iii) 'Duality' in physical sciences (can be illustrated with many examples but not more generally definable)[5].

(iv) 'Bootstrap' philosophy (somewhat akin to Mach's principle; once very popular in particle physics; coming up again in the context of modern string theory[6]).

(v) 'Shell structure' *ad infinitum* (so far only three stages have been confirmed, though, according to an unpublished theorem due to Drell, any regularity checked up to three stages is valid generally; see also ref. 7).

These examples illustrate but one aspect of subjectivity which relates to a sort of 'value judgement' in the pursuit of science, and operates mostly in the abstract plane of pure thought process. This 'noble' aspect, which may be more appropriately defined as 'natural subjectivity' (God is not neutral as between good and evil; 'beauty' vs 'ugliness'; simplicity vs complexity; and Nature seems to conform to His choice!), represents a perception which is largely the result of 'collective wisdom' of the human race acquired over a vast millennium in evolving its way of life, and in the process unearthing the secrets of Nature. And in the hands of scientists too, the manner in which this 'noble' form of subjectivity (value judgement) came into being (despite its spontaneous appeal in most individual minds) is the result of a complex feedback process involving interactions at the human level, and hence subject to all factors prone to human failings and weaknesses. They often relate to various techniques of 'persuasive argumentation' (to borrow a phrase from Thomas Kuhn's famous book[8], which concerns much of the discussion to follow), with an obvious connotation of 'survival of the fittest'[8]. This (second) aspect of 'subjectivity' in science (which is not always 'noble' in its working!) is the central theme for the rest of this discussion with the help of illustrative examples.

Subjectivity: the human factor

Since this field is vast in itself, it is first necessary to delineate the scope of this discussion by ruling out certain topics at the outset. Thus, we shall not be concerned with rather esoteric philosophical issues like the nature of the physical laws themselves[9]: Do these laws have an 'objective reality' in the sense of an 'out there' existence, ready to be discovered by a Newton or an Einstein, or must one be satisfied with a Bohr-like effective description, *à la* the Copenhagen interpretation (CI)? Although such questions have always retained their time-honoured philosophical significance, their renewed interest in recent times owes its origin to fresh efforts directed towards bridging the conceptual gap between the CI and a formal understanding of some famous paradoxes through the strengthening of their conceptual foundations, with extensive reviews both technical[10] and non-technical[6] available in the literature. At the moment the upshot of all this highly intellectual exercise seems to be that while the CI, thus strengthened[10], is in a better position to take on its critics, the operative value of the new machinery in terms of a capacity to predict new observable phenomena is quite limited[11]. Nor should we be unduly exercised over a proper definition of science *vis-à-vis* pseudo-science[12], since any attempt to provide a sharp distinction is fraught with serious ambiguities[12], and instead take a pragmatic, matter-of-fact view in this regard. Instead, we shall find it more relevant to our chosen theme of 'human subjectivity' to focus on the attitudes of scientists[8] within their own community in the matter of implementing the time-honoured theory–experiment confrontation in actual practice. To that end we shall begin with recalling, again at the cost of some repetition[3], the salient points of Kuhn's thesis[8] before going on to discuss some specific examples.

Kuhn's thesis: the arbitrary element

The evolution of science is a very complex process wherein its history is not a mere collection or catalogue of facts, theories and methods. Indeed, there exist many *inter se* contradictions in them, and only a directed stream emerges at the end after 'resolving' such contradictions. It is only such 'predigested' material at the hands of the chronicler of science that goes into the textbooks. This resolution is admittedly a democratic process in the usual sense of the term, and it is here that there is a catch! How about those ideas, theories (and even experiments) that did not make the grade? Indeed, it is entirely possible that there could well have been many good points about them, but somehow they got lost out in the race, not always because they got rejected by experiment (or superior experiment), but often because they never got a chance to come up to that stage! Perhaps their theories did not have strong enough godfathers (or willing listeners) for 'marketing' them. One must hasten to add that this is by and large a fair game, but the main point of emphasis (and not infrequent concern) is that in this complex process of evolution there is a fierce element of competition (often bordering on rivalry) in which the 'fittest' of ideas is given the best chance to survive. And the definition of 'fittest' is not always reassuring, even though a minimum criterion is one of maximum internal consistency plus fits to the data. Now suppose there are more than one such candidates, all conforming to these minimum criteria: Which one should be chosen? The question is not necessarily as hypothetical as it sounds, especially if such theories are proposed at some 'branching-off' stage (or cross-roads?) in the process of evolution of the science concerned. In such cases the 'lucky one' often gets chosen in an apparently arbitrary fashion, to the detriment of the others with equal *a priori* claims. True, such a choice involves a broad consensus within the community; yet it implies a preselection[8] at the current state of the art (not waiting for more discriminating criteria at a later date). Such a process, by its very nature, is a highly subjective exercise, and gives science a very different image from that of an 'objective pursuit' of truth.

The essential point here is that the inadequacy of methodology[8] introduces an arbitrary element into the field in the course of its evolution, through a continual competition between a number of distinct views of Nature, each partially compatible with experiment. How did this come about? Somewhere along the line a particular 'belief structure' got espoused by the community[8], to the detriment of the other claimants (the arbitrary element), due to personal or historical coincidences. A sort of spontaneous symmetry breaking occurred in the branching process wherein a particular branch has been followed[6,8]. If the methodology were more perfect, other competing ideas would perhaps have had a better chance. However, taking things as they occurred one must say that the 'chosen belief branch' at a crucial turning point in history would get strengthened with time due to its pursuit by a large and committed band of well-trained workers who have learned to work within well-defined 'conceptual boxes'[8], but somehow were not encouraged to think beyond that particular discipline. This is what constitutes normal science, whose constraints strongly inhibit any novelty that clashes with its basic tenets. This may often be broken by an 'outsider' to the field who probably 'sees' more than such constrained workers (a pointer to the benefits of interdisciplinary research?).

And yet, even within the precincts of normal science, there exists an inherent 'governor mechanism'[3] which eventually comes to the rescue, and prevents an indefinite suppression of novelty. For there appear fresh contradictions – 'anomalies' – when a normal problem fails to respond to standard rules and procedures, leading to a paradigm shift[8] – a new set of commitments or rules[3,8]. Yet the fact remains that normal science is a rather conservative process of accretion of knowledge which does not as a routine aim at novelties of fact or of theory. It likes to remain quite smug in its 'understanding' of Nature in terms of its stated rules and procedures, howsoever imperfect or inadequate they may look to its own eyes. It is only when it (normal science) is 'pushed to the corner', so to say, due to the appearance of new and unsuspected phenomena, that it feels helpless against the onslaught of fresh ideas which have a potential to lead to new theories which may even be radical.

Subjectivity: the community and the establishment

Kuhn's thesis, as summarized above, brings out the nature of subjectivity in science as seen through its actual working. There is a strong 'directional element' in the *personality* that is normal science. Within this scenario, the 'objective' aspect of science is merely confined to laboratory investigations, while the dominant role is played by its 'subjective' partner, which offers a rather 'chauvinistic' view of science as a whole, one in which the survival of the fittest is the order of the day. True, it is the laboratory investigations that provide the basic strength to its practitioners to 'fight for survival'; yet a better measure of their power lies in their debating skill, or in Kuhn's words, in their capacity for 'persuasive argumentation'[8], which is probably the dominant factor that determines their 'standing' in the scientific community, while the objective part (laboratory investigation) remains largely in the background. In this dual scenario, science appears as an organized structure governed by the establishment within the community, much like in a 'democratic socio-political entity', where the final arbiter is the 'assent of the community'[8]. Nevertheless, the establishment has its own authority which 'determines' the direction of science, despite the existence of occasional 'antiestablishment' or dissident groups within the community that are at best 'tolerated' in the sense of being 'allowed' to publish their own ideas in journals which in turn are controlled by the former. However, one cannot be too sure even on such matters, what with the information explosion on 'ideas' leading to a proliferation of 'publication aspirants', who must be kept in 'reasonable check'. In this respect the prospects of publication are better ensured with 'proximity to establishment' than through the freelance efforts of an obscure worker. As further evidence of 'centralization' around the establishment in the high-tech atmosphere of today's science, yesterday's culture of individual (small-scale) scientific enterprise is gradually yielding place to a more organized form of collective (large-scale) science, with thousand members not uncommon in an experimental team. The most recent example of such team work is provided by the spectacular manner in which the CDF and D-ZERO groups have finally 'zeroed in' on the elusive TEE quark[13].

Now while such 'big science' is by and large inevitable as well as beneficial for the planning and execution of a major project in the experimental field, it is not equally obvious that the same is also true in the *theoretical* domain, where too much 'direction' may well prove counterproductive. A topical example is provided by the amount of organized effort by theoreticians in the early eighties (under the aegis of some important centres) in predicting a finite lifetime for proton decay within the limits of experimental measurability. These limits were variously estimated by methods which paid due regard to the parameters of the (high-profile) 'high-energy' dimension of the problem (grand unification theory), without a matching concern for the (mundane) 'low-energy' (hadronic matrix element) part, which required a much greater effort to produce a physically reliable figure, even though the measured value, if any, could not possibly distinguish between the two components. An independent estimate, one in which greater attention had been paid to the hadronic part, produced a value which was more than hundred times larger than most of the above (one-sided) estimates, a limit that had not been violated by the then available data[14], but coming as it did from an obscure source far away from the (theoretical) establishment, this finding was largely ignored in the literature. This result was *not* intended to claim that the GUT theory could still be valid – perhaps not – yet it seemed to be rejected for the wrong reason (Other examples of 'subjective' decisions arrived at prematurely by consensus are given further below.) A related trend in the same context which brings out yet another dimension of subjectivity (by way of wishing to 'conform' to the 'views' of the establishment) was also discernable around the same time, in the anxiety of certain experimental groups to appear to agree with the prediction of 'faster' rates (10^{31} years) prevalent at that time[14].

The analogy with the sociopolitical scenario could indeed be pushed further if science were to be pursued under one (world) organization, in keeping with its supposedly common (international) standard. In its actual working, however, the pursuit of science is heavily circumscribed by the local and regional environments of the scientists themselves, and this often constrains their relative opportunities *vis-à-vis* their more fortunate counterparts residing in more visible centres of scientific power. This circumstance, in turn, brings in further 'asymmetries' in the overall scientific culture, and adds yet another dimension to the 'subjectivity' factor, one governed by the *actual* (not just by analogy!) sociopolitical environments of the workers themselves.

Subjectivity: theory vs experiment

Even without pursuing the sociopolitical analogy any further, the subjective nature of the actual working of science may be brought out with some specific instances of its impact on a global scale. One example was cited just above in the context of proton decay, but it need not (indeed, should not) give the impression that subjectivity is necessarily 'bad' for the growth of science on a global scale, despite the 'local' suffering of individual workers. Yet the examples are quite instructive insofar as they help in exploding the myth about the so-called objectivity in science. In a recent book[15], Collins and Pinch have highlighted some famous and not-so-famous instances of how the subjectivity factor has prevented unwarranted fluctuations in the overall stability of basically 'correct' premises of science, when it is remembered that science is merely the 'art of the possible'[16] and does by no means aim at discovering the absolute 'truth' (which is at best an asymptotic concept not accessible via 'finite efforts'). This is not to deny the *need for time evolution of even the most* fundamental concepts of science (did not relativity and quantum theory eventually replace the classical Euclid–Newton concepts?), but the time scale for such evolutions is the result of a very complex feedback within the scientific community and cannot be determined on the basis of any insufficient efforts, howsoever reliable, from a few individual workers without a thorough debate within the community on their wider implications.

The first example concerns the famous expedition of Eddington in 1919 to verify Einstein's general relativity. Even a cursory perusal of the Collins–Pinch account would convince anyone that the 'raw data' were hardly discriminatory as between the Newton and the Einstein predictions (which differed by half). Yet a conscious value judgement had to be introduced by Eddington (with active support from the Astronomer-Royal) to decide in favour of the Einstein (not Newton) prediction. At the hands of a lesser mortal such a 'doctoring' of data would either be ignored or (if, the worker has a good reputation) provoke violent protests. Indeed, in the present age such 'poor' data would simply be 'unacceptable' in the first place, a recent example being the 'monopole' event of Cabrera[17]. A more substantial evidence of the demands of modern rigour in the processing of data is provided by the 'processing time' taken by the CDF team work before finally deciding to publish their results[13], even though the crucial nature of their findings (for which the entire community had been waiting with bated breath) would have ensured a ready acceptance. A greater boldness (and daring!) had been shown by Carlo Rubbia a decade earlier, when, as leader of the UA2 team, he had decided to publish their findings somewhat in advance of the final results, for which the entire high-energy community had been waiting anxiously.

However, Eddington's expedition was in the Rutherford era when science and its practitioners used to be regarded with far greater awe and reverence than is the case today. And, with his formidable standing within the scientific community, he was able to push through his 'interpretation' successfully, as the circumstantial evidence was overwhelmingly in its favour, though not necessarily on the basis of the actual data obtained from the expedition. The important lesson from this episode is that an experimental 'confirmation' of a theory in the standard orthodox fashion – the bedrock of *'objectivity' in science* – is not always relevant for 'belief in the theory'! In retrospect, Eddington turned out to be right, but for a 'wrong' reason, since it would not have mattered much what the data showed. On the other hand, his deep insights in physics, howsoever buttressed by his high prestige, had their own limitations as were tellingly confirmed by his own student Chandrasekhar's subsequent 'victory' over the Professor's (superior?) judgement on the now famous *Chandrasekhar limit*. And yet the damage that the Professor's initial verdict had caused to his student's career in the interim period is a matter for history[18]. Lesson? Personal authority as an instrument of subjectivity must not be pushed too far, since its credibility is not unlimited! Failure of 'institutional authority' is already illustrated (see above) by the failure of proton decay within the time limits set by the grand unification theory.

Yet subjectivity has been known to 'pay' if combined with a right 'dose' of intuition together with establishment's support, in both of which the actual 'experimental' evidence, especially in the short run, often plays a marginal role. Thus, in the famous 'experimental' debate between Pouchet and Pasteur[15] on the origin of life, Pasteur was against the spontaneous generation theory advocated by Pouchet. Although Pouchet's experiments seemed to support his theory, Pasteur would invariably come up with some 'catch' in the experimental material and/or set-up. He even 'discarded' some of his own experiments which showed a bias for 'spontaneous generation', relying more on his intuitive perceptions than on the actual observations. And he was supported by the Paris-based Academie des Sciences – the establishment – even though the latter's reasons were anything but objective. That Pasteur was upheld by later, more precise, results is an entirely different matter.

A similar incident of more recent origin concerns Fairbank's (an experimental physicist of considerable reputation) 'discovery of the quark'[19], a result which was hotly disputed by Morpurgo, who was so sure of his ground that he even ventured to suggest that it was an artefact of the 'mode of observation'[20]! Finally, at the Rochester Conference of 1960, during the heydays of the V–A theory, Feynmann simply refused to believe that the theory could be wrong, even though some helium-6 data were 'strongly supporting' the scalar-tensor theory. Such examples can be multiplied.

All these examples illustrate the more 'noble' aspects of subjectivity that relate to a 'value judgement' in which deep intuition plays a vital role.

Conclusion

In the foregoing we have tried to depict several facets of subjectivity, from value judgement to human interactions, that are employed in the actual practice of science. While the traditional picture of science is one of 'strictly objective' interplay of theory and experiment within the rules of the game, subjectivity enters in a vital way as a sort of value judgement in the worker's own perceptions of the same. This 'abstract' aspect of subjectivity is an intensely personal perception which remains hidden from view but may be reflected in his choice of problems and the tenor of his writings. The more visible aspect of subjectivity, on the other hand, enters through human interaction, wherein science is a part of the community's larger concerns, and hence is subject to all standard types of socioeconomic and sociopolitical pressures. Sandwiched between these dominantly 'subjective' elements, the 'objective' part of his practice of science is more or less a routine affair, appearing essentially as an end product of his investigation, while the vital decisions are controlled by the former.

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