# Needle in the haystack: The art of scientific packaging

*V. Sitaramam*

Method of science has been relegated to arm chair activities of philosophers based on post facto analyses of 'successful' science, rather than being the preoccupation of the current practitioners. This lack of reflection led to two absurdities: first, science has acquired a pseudo-status as a perfect model for objective knowledge; and the second is that it led to a colonialist view (as in top dogs vs followers, to borrow a phrase from Feyerabend) and practice of science in developing countries, e.g. India. Science (as opposed to knowledge), as it is practised, is Western in its origin as well as practice. It is one imported religion (as a faith and a collection of organized practices) that has not been questioned to the extent that the other faiths have come under sociological scrutiny. Some damage control is necessary if we are to improve upon its practice, even if it is of Western origin, to adapt to our use. It is critical to do so, as trying to keep up with the West led to a sociological equivalent of Zeno's paradox of Achilles and the tortoise.

What with the world rapidly shrinking in resources as well as options, what remains to expand is only the reasoning with reference to the data we obtain from the world. Science has been the major contributor to our insatiable capacity as consumers. With diminishing resources, we need greater and wider options, which cannot be foreclosed for the larger segments of people. The West, either by political pressure or in the garb of science, cannot control our future. The white man's burden is best left to the white man to carry (as the reaction to Club of Rome's report on Limits to Growth indicated). The major detractor is the way we manage science itself based on rather obsolescent notions, which are often self-seeking in a consumption-driven world. An exclusively retrospective analysis, the means for the method and philosophy of science, loses the context of 'operations' of the then practitioners, which has much to do with the social practices among scientists in doing science at that instant of time. Never before has such an analysis become as urgent as it is now.

Individuals in every age spend enormous efforts gaining some knowledge. The very rarity of insights brings home the analogy of the needle in the haystack. Haystack is not simply a paradigm working overtime. The sociology of haystacks is best understood in contemporary science, particularly in biology where we have a clearer picture of our own practices and operations, rather than from a clinical description of historical successes, where the context is lost.

## A problem statement

The idea of haystacks can be formally defined. Simply stated, at time $t_1$, we have empirical content $e_1$ which is explained by theory $T_1$. Time goes on and more evidence is accumulated such that at time $t_2$, the empirical content $(e_1 + e_1)$ we have, forces us to review $T_1$ with two possibilities: we come with an altogether new theory, $T_2$, or we modify and accommodate the new findings in $T_1$ such that we have merely $T_1'$ (a modified $T_1$, that is) and not a new theory, $T_2$.

Much of the method and philosophy of science hangs on this question whether science, as a model of objective knowledge, be distinguished from metaphysics, which represents all other forms that are more subjective. Popper's emphasis on falsifiability was considered the stronger criterion in the sense that levitation, for example, if an empirical fact, would directly refute the theory of gravitation. This was questioned by Kuhn that a paradigm shift is forced rather than sought. Kuhn observed that much of $e$ is normative in that it merely explores the consequences of $T_1$. $T_2$ is the happenstance of the accumulated burden of $e_1$ that forces move to $T_2$ when even $T_1'$ no longer suffices and forms the structure of scientific revolutions. Feyerabend had the apparent last laugh. He argued that Kuhn's normative science and the usual safe breaking are indistinguishable since both scientists and burglars work under a relevant paradigm! Popper, argued Feyerabend, has too naï ve a notion of falsifiability to be of value since some of the most influential paradigms are neither testable nor falsifiable, something that Popper had to concede; but then the criteria of demarcation have vanished.

The debate continues.

Though Kuhn's approach was considered to be a sociological approach, many aspects of sociology of science and its practices have not been identified as of material use to the scientist and the teacher, and hence this attempt.

## The virtue of hindsight or the richness of historicism?

Let us assume that at some time (in the past), a hypothesis is published. As matters stand, the work received due recognition and over the last few years is progressively replaced by another hypothesis. The old and new hypotheses are not necessarily competing but address different aspects of the problem. The comparison therefore eludes us unless we formulate it as the difference in the empirical content in the problem space that has been achieved to form the core of the new theory.

Let the original proposal contain $p$ components that make up the first hypothesis and let $r$ components be the ingredients of the current hypothesis, such that $q$ components are common to both. Let each component, $i$, have a weight $a$ associated with its relevance to a hypothesis such that the worth of an active hypothesis is always *1*, that is,

$$\sum_{i=1}^{p} a_i = 1,$$

Similarly the current hypothesis, with a weight, $b$, of each $j$th component,

$$\sum_{i=1}^{r} b_j = 1,$$

such that for the common components, $q$, with the same weightages, it would necessarily be

$$\sum_{i=1}^{q} a_i = \sum_{j=1}^{q} b_j < 1 \quad \text{for} \quad i, j = 1, 2 \ldots q.$$

That is simply to say the weightage of new concerns $(r - q)$ outstrip those of the earlier $(p - q)$, accounting for the shift in emphasis from $p$ to $r$ over time. However, this is a dilute statement. We should ask more penetrating questions.

There is the residue, $p - q$ in the original hypothesis, that became irrelevant over time. There is the unanticipated, $r - q$, which is novel. The haystack hypothesis suggests

that, assuming that assignments of weightages in $q$ did not change (just for convenience of the argument),

$$\left[\sum_{i=(q+1)}^{p} a_i\right] \ll \left[\sum_{i=(q+1)}^{r} b_i\right].$$

To be consistent with normalization, the weightages must change. In other words, a significant component of $p$ was largely packing material that did not stand the test of time. *The haystack really becomes an interesting sociological device in science if there is reason to believe that, even at time $t_1$, it was designed to be so.* An objective evaluation should not be blind to a value assessment and the spirit of science also embodies values we impose to science and its practice.

## Identifying haystacks

Haystack is not a paradigm. Haystack is the package that gives the paradigm its colour, texture, feel, sophistication... anything except its essence. It comprises both the statements made to be compliant and statements omitted to avoid conflict. Kuhn's notion of a paradigm actually refers to that pack behaviour that makes you belong to a club. Clubs offer package deals. Conforming to any club requires that the threat perception $(r - q)$ is minimized. In other words, the short fall of $(p - q)$ compared to $(r - q)$ is what the club attends to. Any observations need to be couched in a language that is acceptable to the $p$ club, for whom $r$ club is a potential and serious threat in the time frame $t_2 - t_1$.

Some examples are worth briefly exploring to illustrate haystacks.

Historically, the most engaging debate relates to Galileo. Who was right, the Church or the savant? Heliocentricity was a hypothesis since Copernicus and it did not worry the Church as a hypothesis. When Galileo trained his spy glass on heavens rather than on the ships approaching the port, his observations were based on the theory of optics which still awaited Newton. If the Jesuits warned him to leave the teaching of the Church alone since empirical observations have no standing independent of theory, they were right. Galileo also had other motivations, it appears. When we evaluate the content of a theory for its $(p - q)$ content, we should not be influenced by what turned out to be right as much as we wish to be understanding about what was missed. If one did not trust the pictures drawn of craters in the moon, that is not the only haystack. Were the theological arguments called for?

The idea, now, is to reread the original papers and contemporary accounts and identify $p - q$ from $q$. It is for the physicists to identify their own haystacks. It may not be too difficult. Contrary to the newspaper reports, Sam Wilberforce appears to have got the better of Thomas Huxley in the then evolutionary debate, while Darwin was afraid to face the question of the origin of man, wrote Stephen Gould. Biochemists have changed in thinking from the allosteric model of Monod and colleagues to, say, Koshland's induced fit model. We can ask questions, e.g. the preoccupation of Monod *et al*. with symmetry consideration is elegant or required for his own hypothesis? Here hindsight would not only help, but would also point out that alternative explanations could be visualized as indeed they were. I will restrict myself to examples wherein I have firsthand experience.

Haystacks, if clearly identified, are self-destruct and therefore serve a useful purpose. In the theory of centrifugation and isolation of subcellular organelles, which occupies half the Nobel lecture of deDuve, a major concern was the sucrose space. Permeation of water soluble sucrose across the oily biological membrane was thought to be forbidden and therefore a subcompartment in mitochondria was postulated. Since mitochondria were soon shown to have two physically separated compartments, the kinetic, though spurious, argument of assigning the outer space to the sucrose space gained ground. The ubiquity of sucrose entry forced the hand such that even single-walled organelles began to have claim to sucrose space, wherever that would be. Since sucrose was shown to enter during centrifugation, the mechanism of sucrose permeation and the irrelevance of sucrose space came to be known only later. While the entire sucrose space hypothesis turned out to be non-existent, why was it included in the first place? If an explanation were not given, most measurements of transport across organelles including membrane potentials and active transport would be patently false. Secondly, active transport across a permeable membrane leading to gradients is still hard to conceive, while there is nothing actually to forbid it. The entire packaging given by deDuve for sucrose space sought only consistency between the physical theory and the observations and proved or disproved nothing. A very clear definition of the purpose of packaging, indeed. In fact, the virtue of his handling was that the haystack could be precisely identified and dismantled by direct experiments.

Perhaps an immediate contrast lies in Peter Mitchell's chemiosmotic hypothesis. The little gray book, which he privately published, could be understood for its essence by most biochemists only after David Nicholls wrote a monograph on the hypothesis. What remains central to the hypothesis is the creation of a gradient of bulk phase protons separated by a barrier to store energy mediated by primary pumps (the energy generators) and the energy harvested by secondary pumps (the energy dissipators). What was written however, was considerably more complex. Further, general acceptance by the community, a conscious decision for consensus if ever there was one, came after 1982, despite a huge debate at the 2nd European Bioenergetics Conference at Lyon. The focus of explanation in oxidative phosphorylation is not about ATP synthesis! It is to account for the Lardy and Johnson experiment wherein respiration (the energy generator, the cause) ceased when ADP was exhausted (i.e. phosphorylation, the energy dissipator, the effect, ceased). A case of tail wagging the dog! What is the nature of this respiratory control, what interactions underlie this and what is the nature of coupling, since distant action is forbidden? Proton translocation was one among the hypotheses as the mediator, while the caveats were never spelt out by Mitchell. Even the basic Grothus mechanism (protons, unlike other monovalent cations, go skinnydipping and move without the hydration shell by jumping along the lattice defects of water, given a chain of water) of water wires across the bilayer to account for the observed rates of proton translocation remains unsolved despite huge efforts in molecular dynamics.

## Some more haystacks

You cannot call something a haystack unless the larger component in the proposition is found wanting. Usually these moves are motivated. The most famous (or infamous) example is the issue of star wars. It was clear from the beginning that the idea of star wars is not tenable technically. But then that was not the purpose. The resulting effort contributed no less to the unsustainable soviet economy and the rest was history. It is strategy triumphing over technicalities. Weapons of mass destruction are a sociological or technical haystack, as it turns out

to be. These were not historical errors but political strategies.

In genome projects we clearly see strategies based on haystacks. Promises of progress, supported by journals, ensured governmental funds while promises towards pockets ensured corporate funding. Even before the ink dried in the announcements across the Atlantic of the success of human genome initiative, the same journals started writing whether it was worth the effort and how much could be hype. Those who wrote about the prospect of an atom bomb were also among the first to blow the whistle on how bad it could be. There were clear social dividends in switching gears.

Is it worth knowing the human genome in its entirety? Yes. But, once there is no more future in mere sequencing, many labs in the third world have been either persuaded or contracted to do the sequencing work. When genomics failed (failure should be judged in terms of the hopes and projections), proteomics took over. If we know every single protein and in every single cell under every single condition… may be we will cure cancer. Two patients differ in the drug response. Perhaps a combination of proteomics and genomics… functional genomics, would help identify the idiotypes in drug response… pharmacogenomics.

Combinatorial chemistry, mass throughput screening have all become possible due to automation and miniaturization which permitted analyses that could not have been dreamt of a few decades ago. These are techniques that permit mass screening wherein the idea still has to be correct.

One recent major development is systems biology, started in a number of major schools in the world. There have been many attempts at computational biologies of different kinds, from simple metabolic modelling to more ambitious metabolic control theory and inrreversible thermodynamic approaches. Most have them did not deliver much. Now virtual cell and systems biology have resurfaced as current trends in computational biology with the hope to wed much of the information now available in genomics and proteomics.

There are many reasons why a purely computational approach is doomed. Take for instance the growth of an *E. coli*. It was known that the growth rate constant would not change as a function of free glucose as expected of a Michaelis Menton curve (a rectangular hyperbola). It is now clear that glucose transport stimulates respiration similar to respiratory control with ADP in mitochondria and that this respiration induces porosity in the bacterial membrane leading to a leak! How will a virtual cell indicate the reality that was not been even visualized in the first place? If the virtual cell becomes a tool to measure deviations in the *real* behaviour as a probe to understand these mechanisms, then only it would serve a useful, though very limited purpose.

There is a downside to it all. Discovery of land led colonization. Discovery in science is hoped to lead to commercialization. When the push came to a shove, actually the throughput of drug discovery has fallen to some 25% of what it was. One has to appreciate the foresight involved in WTO such that, had any of these fructified, the discovery would have become the property of a select few. It was not to be. If the haystack argued for endless natural resources, it served the immediate commercial ends and long term global disaster.

## Functions of a haystack

Any hypothesis must be placed in a context of its 'contemporary' science and this context relevance is provided by the haystack. Some useful roles of haystack component in a hypothesis are:

- Context creation helps the sense of belonging and enhanced peer acceptance.
- Citation enhances due to haystacks because of enhanced acceptability.
- Grants become easier because committees find it easier to accept acceptable ideas than gauche, novel ideas.
- Haystacks are devoid of content and therefore likely to be devoid of mistakes. Applications of catastrophe theory, metabolic control theory have been some areas of intense effort that brought in accolades as well as remarks about Emperor's new clothes. Haystacks are thus necessary for sustaining the 'publish or perish' paradigm.
- Science policy in any country is largely a compilation of haystacks; for countries like India where there is very limited GDP to spend, this awareness is critical. The science policy tends to be supported by a series of brain-storming sessions which function by pre-written agendas and conclusions. *Existing problems cannot be solved by existing ideas because they would not have existed as problems in the first place.* Aping Western solutions has been our major mistake perhaps after the first two 5-year plans (which were indigenous). Our science policy is dictated by the Western mores. The extent of damage can be understood only if we are aware that even technology is dictated here by scientists and not technologists! Policy statements are necessarily politically savvy. Policy issues become critical when they begin to define what should or should not be investigated. Policy is an area where sociology and science collide head-on. It should be so. It cannot be decided by the West.
- There is another curious anomaly of even deeper significance of what is admissible as science. For instance, determinism in classical physics is acceptable and we speak of deterministic laws and their applications. Determinism in sociology had very unacceptable consequences when it was implemented in the garb of eugenics and IQ testing. Therefore if we take a view that sociological 'laws' cannot be formulated or found, we close the doors on certain inquiry. While classical physics started on certain well-understood primitives, in quantitative econometrics, the subject matter has well-developed mathematical structures, however contrived, and yet no clear emergence of primitives. One would imagine that consumer economics would form some common basis (e.g. man spends because man needs) to identify sociological primitives. General econometrics is still dissociated from poverty economics as if the poor obey different rules, a matter of some importance to Indian policy. One sees that many areas, say in biology, ecology, etc. are increasingly coming under deep quantitative scrutiny and social sciences will be no exception. Sooner or later the question will have to be faced, however sustained is the prejudice against formulation of the relevant sociological laws.

## The many *avatars* of a haystack

I could be rightly accused of using one word to describe many phenomena. My plea is that $(p - q)$ needs attention and a serious look at it led to many ways of describing it. Since it did not survive, we would call it a haystack, not in a pejorative sense, but in the sense it be not of substantial importance *even at the time of formulation*.

It could perhaps be less clear to a physicist, as opposed to a biologist, what the issue of a haystack is, as paradigm shifts rather than social processes behind them have been the object of inquiry in physics, barring a few examples like Galileo.

Central to each published manuscript, there is an idea, and there is an incremental improvement of that idea either in logic or in evidence or in both. Even for an idea that is considered new or exciting, its presentation involves so much of the circumstance or plausibility that makes it acceptable to the community, i.e. the haystack. The community systematizes the haystacks in terms of the state of the art reports that, while offering useful service of stating what is being done, also indicate what is foreseeable. *When what is allowed to be foreseen is ordained by the club members, we encounter hegemony or control of science.* Different kinds of haystacks serve all these different purposes.

## Control of science and haystacks

Control in science occurs at two levels: one is with publication and the other with grants. Publicity and Nobel prizes help. Recognition is secondary to doing science and therefore science is primarily controlled at the level of doing it.

The problem with science management is the discrimination that is associated with it. Non-citation of work from developing countries, the general mistrust of citation indices, the fact than Review Journals (such as Trends) have a higher citation that research articles, scientific groups that are hard to enter, allocation of resources largely to 'mission' mode research to institutes that do basic research . . . the list is large. One major outcome of the haystack hypothesis could be that we identify the need to modify our publication procedures. Perhaps e-journals suitably tailored could replace our conventional style of publications. Similarly, evaluation of grant proposals should probably become extinct. Even in the current scenario, if we scrap our granting procedures and allot money based on peer-reviewed publications, and be liberal for those at the entry point, a lot of politics would subside.

With this background, we can see the odds of doing science in the country. We still have people extolling virtues of foreign trained or educated persons. Collaborations and a trip abroad are a must to be visible. Citation is good if your article is in a high-impact journal and suspect if it is not. What you published itself does not matter. Since what is published abroad usually is what is current there, it is safer to pursue haystacks than developing your own problems in India.

The terrible disadvantage that a teacher faces can be understood, given the notion of a haystack. The syllabus, arrived at by a Board of Studies, which is compendium of yesterday's haystacks, is to be delivered using textbooks, which were written the day before, such that it enhances creativity in a student, today! Offered together with that unparalleled ruminating pastime called the examination system, we have an oxymoron of sorts, . . . creative teaching! Enough has been said.

The real problem is that education and educational psychology themselves suffer from an overdose of foreign haystacks. Lack of reflection is certainly made clear when we find actual discussion in this journal and elsewhere on the qualities required of a vice chancellor or a JRF to improve universities!

## Concluding remarks

Haystacks are necessary for practitioners of science. They do not ensure its growth. Nearly all planned activity is based on the necessary rather than sufficient conditions. Any novel idea requires you to do relatively simple things. Getting somewhere is faster since you do not have to do too many things. But the idea does not get accepted readily. It is tough to publish. It has high risk. On the other hand, one has to do many things in an established area to be able to come up with something. Complex ideas and experiments have to be managed. But it is easy to publish because the situation is comprehensible to the community. Rewards follow visibility.

The West emphasizes and dominates science and its clubs are rigidly protected by their own needs. It is necessary that we disengage ourselves from aping the western practices of controlling science due to our own internal schisms and ape their result-orientedness instead! To do that, we must do away with haystacks, internal and external, wherever possible.

People make conscious choice of following a paradigmatic route or an independent route early in the game. There are no accidents. Society prefers the paradigmatic route. There is no choice. Progress lies in the difference.

ACKNOWLEDGEMENTS. I am grateful to Profs N. J. Rao, V. V. S. Sarma, A. D. Gangal, N. V. Joshi and late Mr V. Krishnabrahmam and many others for offering divergent views on the matter.

*V. Sitaramam is in the Department of Biotechnology, University of Pune, Pune 411 007, India.*
*e-mail: sitaram@unipune.ernet.in*