

CURRENT SCIENCE

P.B. No. 8001, C.V. Raman Avenue, Bangalore - 560 080, India. Telephone : (80) 3342310, Fax : 91-80-3346094, E-mail : currsci@ias.ernet.in

Editors
Prof. P. Balaram
Prof. S. Ramaseshan

Ref. No. OP173

1 February 2000

Dr N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref : OP173
The dilemma of influenza

by Fred Hoyle and Chandra Wickramasinghe

Dear Dr Joshi :

The enclosed article

The dilemma of influenza

by Fred Hoyle and Chandra Wickramasinghe

has been submitted for publication in the Opinion section of Current Science. We would appreciate if you commented on its suitability for publication. Please send your report as early as possible, preferably within ten days.

Yours sincerely,


for Editors

Opinion articles present views on issues related to science and scientific activity.

THE DILEMMA OF INFLUENZA

By

Fred Hoyle and Chandra Wickramasinghe

01217

25 10

There is a great deal of evidence to show that catching influenza is due far more to where we are than to the people we have been in contact with over the last few days. For instance studies have found that spouses of sufferers are no more at risk from the disease than members of the population at large. Variations of immunity cannot explain such facts particularly in the case of new and virulent strains of the virus. Where person to person contact is greatest within the context of families detailed statistics have consistently shown that nothing significant seems to happen. This experience is repeated year after year with every influenza season, the present one being no exception.

A study in schools that we made during the bad winter attack of 1978/79 showed unexpectedly large differences in the incidence of disease between pupils who boarded in different houses at the same school. Showing that it was the particular location of the house that pupils were domiciled that mattered, not whether contacts at mealtimes and in school classes happened to be incubating the virus. There was no evidence at all that one pupil caught influenza from another. It was the place where a boy or girl spent his or her time that mattered above all else. (For details of this analysis we refer the reader to our book *Diseases from Space* J.M. Dent & Sons Lond., 1979)

Another point of significance is that the spread of influenza takes no account of modern modes of travel. The spread is still the same as it was before the advent of modern air travel. The spread over the Earth still takes months, which would be difficult to explain on the basis of spread through contact with an incubation period of only a few days. It is still the same as it was a century ago.

The lethal wave of influenza in 1918/19, said to have killed more than the murderous assaults of the first World War, was first detected on the *same day* in Boston and Bombay. Yet in spreading within the United States it took three weeks to go from Boston to New York.

And of the influenza epidemic of 1948 an Italian doctor (Professor Magrassi) reported of the then remote island Sardinia:

"We were able to verify the appearance of influenza in shepherds who were living for a long time alone, in solitary open country far from any inhabited centre. This occurred at just the same time as influenza appeared in the nearest inhabited centres."

In January 1919 Governor Riggs of Alaska reported to a committee of the U.S. Senate that influenza had spread all over an area with the size of Europe and with only a

small thinly spread population of about fifty thousand. This was despite conditions for human travel being worse than anybody could remember, "The territory has to be reached by dog team. You have the short days, the hard, cold weather, and you only make 20 to 30 miles a day. The conditions are such as have never happened before in the history of the territory....."

Influenza is known to come to us in winter, with January and February usually being the worst months. Why? Because in temperate latitudes it is in the winter months that air from the Earth's stratosphere comes down to ground level, and it is because exceptionally cold air from the stratosphere came down on Alaska in the winter of 1918/1919 that conditions for travel there were the worst in living memory.

Air brought down from the stratosphere carrying either the virus itself or a trigger for it reaches ground-level patchily. Occasionally it can arrive at the same time at widely separated places like Boston and Bombay, not requiring any humans to go from one place to the other. The patches of virus appear to have a very fine scale like smoke caught up in swirls of turbulent air. Even to the extent of hitting one school house and missing another as we found in our 1978 study. An idea such as this may have been seen as wildly outrageous in 1978, but now it should be less so with the modern trend to accept that life could be distributed on a vast cosmic scale.

The best defence against influenza is to stay put where you are if you can! Provided you have the luck to avoid a virulent patch you should be safe. But if you move uneasily from place to place sooner or later you will move into an infective patch, and within a couple of days you are likely to succumb. Doctors administering to patients in a bad patch will be run off their feet, while those in a place that is not hit might wonder what all the fuss is about.

We believe the advice to stay put in a safe place is better than a so-called influenza jab. All this can do at best is to give you protection against last year's variety of influenza. It won't give you protection against a new variety, and all the "bad" forms of influenza are new.

Our expectation is that sooner or later a really bad situation, possibly similar to that in 1918/19, will arise. This seems inevitable so long as the Government's advisors continue to prefer medical dogma to taking a closer look at the facts. Which suggest to the point of certainty that what we get from the high atmosphere, be it a virus or a genetic trigger, is significantly more dangerous than anything we may catch from other people.

To take things a step further, the curve of Figure 1 is a plot of sunspot numbers throughout the past century. Sunspot numbers give a measure of high-energy activity at the sun's surface, the peak numbers corresponding with frequent solar flares and the emissions of charged particles that reach the Earth. Such activity on the sun is known to result in geomagnetic storms, ionospheric disturbances that interfere with radio communications, and most spectacularly the production of bright auroral displays, the latter being caused by the streaming of charged particles from the sun moving along magnetic field lines that connect the sun and the Earth.

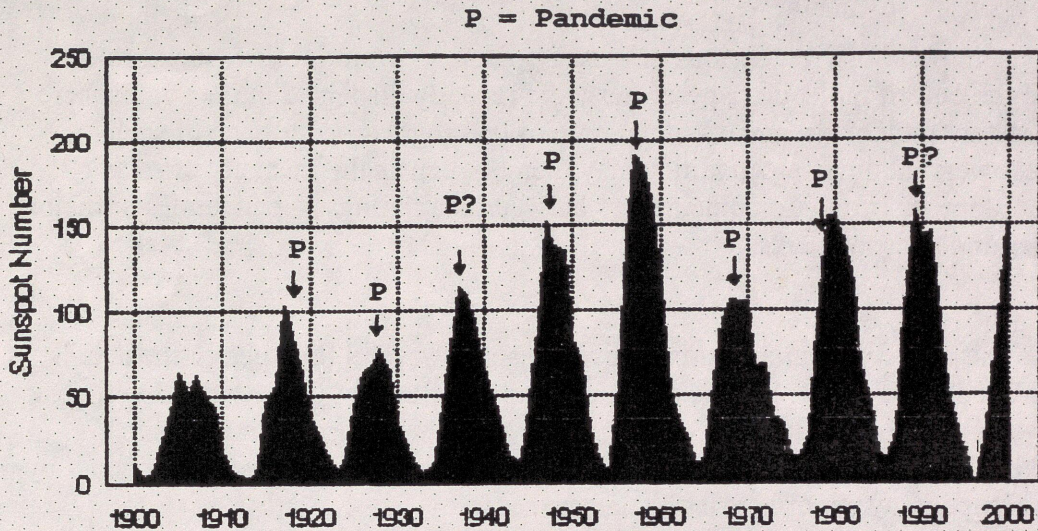


Figure 1: Mean sunspot numbers compared with timings of major world-wide pandemics (P) of influenza. Note in particular 1918: "Spanish Flu" (H1N1) caused 500,000 deaths in the USA; 1957-58 "Asian Flu" (H2N2) caused 70,000 deaths in the USA; "Hong Kong flu" (H3N2) caused 34,000 deaths in the USA; 1977 "Red flu" (H1N1) caused 30,000 deaths in the USA.

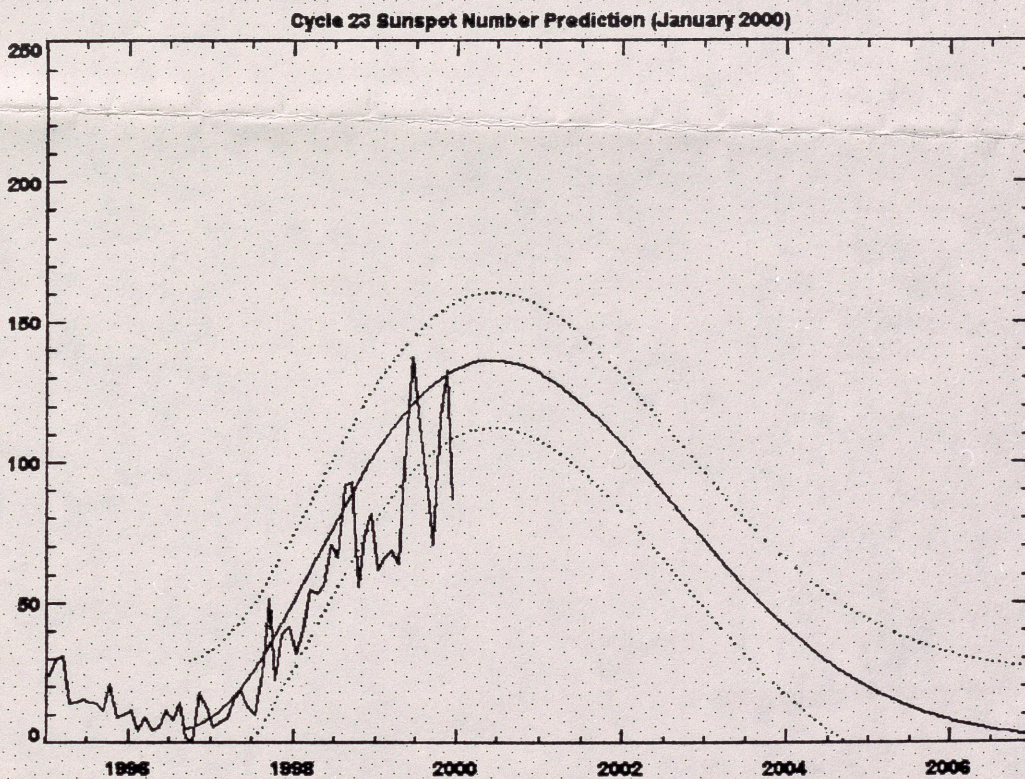


Figure 2: Sunspot cycle No. 23, including theoretical prediction showing peak in the middle of 2000.

Peaks of solar activity will undoubtedly assist in the descent of charged molecular aggregates (including viruses) from the stratosphere to ground level. Thus according

to our present point of view serious influenza epidemics would follow such peaks, provided the culprit molecular aggregates were recently dispersed in the stratosphere from cometary meteor streams. With a more or less regular occurrence of such meteor showers the limiting condition may then be seen as the intensity of solar activity, leading naturally to coincidences between the timings of pandemics or major epidemics and sunspot peaks.

From the end of last September solar activity reached several high peaks, the details of which are shown in Fig. 2. The expectation is that Sunspot Cycle No. 23 will peak sometime in the middle of the present year. In view of the correspondences in Fig. 1 it would be unwise to remain sanguine that we will miss a major pandemic, even if by luck we escape its worst effects in the present season.

CURRENT SCIENCE

P.B. No. 8001, C.V. Raman Avenue, Bangalore - 560 080, India. Telephone : (80) 3342310, Fax : 91-80-3346094, E-mail : currsci@ias.ernet.in

Editors
Prof. P. Balaram
Prof. S. Ramaseshan

31 August 2000

Dr N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
Bangalore 560 012

Dear Dr Joshi,

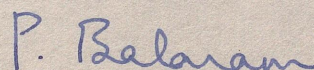
I enclose the Minutes of the *Current Science* Editorial Board Meeting held in Bangalore on 13 July 2000. The meeting was very fruitful and several important inputs were received from the members who attended.

I would be particularly grateful if you could send in specific suggestions or criticisms regarding the journal at any time. Your help in soliciting good articles for *Current Science* and your suggestions for prospective special sections/supplements, along with names of potential guest editors, would be most welcome.

Prof. Ramaseshan and I deeply appreciate your help in running the journal and look forward to your continued support.

With regards and best wishes,

Yours sincerely,



P. Balaram

CURRENT SCIENCE

P.B. No. 8001, C.V. Raman Avenue, Bangalore - 560 080, India. Telephone : (80) 3342310, Fax : 91-80-3346094, E-mail : currsci@ias.ernet.in

Editors
Prof. P. Balaram
Prof. S. Ramaseshan

Ref. No. GA836

17 July 2001

Prof. N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref :GA836
Mapping agricultural research in India: A profile based on CAB
Abstracts 1998

by S. Arunachalam and K. Umarani

Dear Prof. Joshi :


The enclosed article

Mapping agricultural research in India: A profile based on CAB
Abstracts 1998

by S. Arunachalam and K. Umarani

has been submitted for publication in the General Article
section of Current Science. We would appreciate if you
commented on its suitability for publication. Please send
your report as early as possible, preferably within
ten days.

Yours sincerely,



for Editors

General articles discuss current trends in research in a field that
will be of interest to readers outside the field; interdisciplinary
topics; science policy and administration; or some aspect of the
application of science and technology to human needs or the impact of
science and technology on society/ecosystem/life.

CURRENT SCIENCE

P.B. No. 8001, C.V. Raman Avenue, Bangalore - 560 080, India, Telephone : 91-80-3612310, Fax : 91-80-3616094
email : currsci@ias.ernet.in, website : www.ias.ac.in/currsci

Editors
Prof. P. Balam
Prof. S. Ramaseshan

Ref. No. K311

22 June 2002

Prof. N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref :K311
A scientific method for the computation of target scores in
interrupted limited over cricket matches

by V. Jayadevan

Dear Prof. Joshi :

The enclosed paper has been submitted to Current Science. We would appreciate if you, or a competent colleague of yours, reviewed the paper and let us know if it merits publication.

Since Current Science is an interdisciplinary journal, papers reporting novel ideas or results of broad general interest will be given priority.

We will be obliged if you could return the paper with your comments as soon as possible, preferably within two weeks. The comments may include queries that seek replies from authors or indicate corrections/additions/deletions etc.

Yours sincerely,

(S. Ramaseshan)
for Editors

A SCIENTIFIC METHOD FOR THE COMPUTATION OF TARGET SCORES IN INTERRUPTED CRICKET MATCHES.

Introduction

Making use of the advancements in science and technology, many of the important decisions in a cricket match are now made with great precision, with the help of television replays. Decisions on run-outs, stumping, boundaries (four/six), and even catches are now taken only after thoroughly analysing it on television screen. When these items, decided with such great precision are just a few events in the course of the game, it is extremely difficult to believe that there were no scientific methods till recently, to work out target scores in matches interrupted in between. The importance of target scores over any of these individual events, specified above, need not be emphasized. The Duckworth/Lewis system, following now, is perhaps the first scientific approach made in this direction. The earlier methods were hardly anywhere near satisfying even the minimum criteria of acceptability. In this article, the author presents a method developed by him to solve the above problem of fixing target scores in interrupted cricket matches in a most satisfying manner.

beg followed now

D/L
journal
criteria

Background

The maximum score overs concept tried in 1991-92 World cup is often criticised as the worst of all the rain rules tried so far. But this in fact was a wonderful concept but failed terribly due to unscientific execution. In 1992, after many teams including India tasting the bitter flavour of this method, the author had developed a concept to improve it by *over grouping* technique. Though it was sent to the then president of the BCCI, by that time the ICC had decided to change the method as such, and hence there was no further follow up. ~~But~~ the main problem with the *over grouping* technique was it requires a computer program to do it if say more than 10 overs are to be deducted.

It was very accidentally that the author came across working on this once again. In 1998, to explain the programming techniques to one of his colleagues, Mr. Rethnakaran, this was taken as a problem. The programme is found working quite well and found giving very good results in some of the actual as well as imaginary cases tried. During a telephonic talk with his cousin and the secretary of the Century cricket club Mr. Sanjeev Menon, the author told about this work and asked him about the details of the present method then in use. It was the parabolic method (the D/L method was not introduced at the international level then) also known as the "Norm method" which was in practice. The advantages as well as the drawbacks of the system were vivid in this norm method. Though the programme developed became of no use, the author got the idea of clubbing the concepts in the "norm method" and "maximum score overs method" to develop a more scientific system. Thus the first version of the method evolved. Though this was sent to both the BCCI and ICC, by then the present method D/L system was introduced and hence no response received. As the new method (D/L) was giving fair results in some of the early matches, the author naturally thought that it could be a better method and again there was a temporary stoppage for about six months. It was after March 1999, just prior to the world cup that the author came

On making enquiries with appropriate persons, the author came to know that

An ~~examined~~ Even a casual examination of the norm method was adequate to vividly bring out the drawbacks and the author to evolve a better method.

to know through an article of Mr. Chetan Shah (Hindu Daily) that the D/L system also has so many drawbacks. Again the work to bring the method to a proper shape started. Tried to contact many of the authorities of the BCCI, the media people and so on. But there was hardly any response. It was in 2000 March the author met Mr. Ajitkumar a BCCI panel umpire during the India South Africa Match at Kochi. Mr. Ajitkumar showed genuine interest in this. He had with him the official handbook of the D/L method through which the author could study the D/L system thoroughly. This helped him in making the presentation style of the method much more impressive. More over this gave added confidence to compare the two methods with more authority.

[In July 5th 2000 got an e-mail from Mr. Sunil Gavaskar inviting to make a presentation on 11th July in Pune in a BCCI technical committee conference. Impressed by some of the results of the method, the committee asked the author to make a presentation in the umpire's seminar in September 2001 at Jamshedpur. As suggested by many in that seminar, a computer programme was also developed subsequently to effect quick calculations.

One of the major breakthroughs was the interaction the author had with Dr. Sreenivas Bhogle, NAL, Bangalore. The discussions with Dr. Bhogle and the interactions with Dr. Duckworth through him really helped in improving the method further. The problem of inconsistency the method had during multiple interruptions is rectified and the method became more elegant. The BCCI meeting held on 7th April 2001 decided to forward this proposal to the ICC. But for some unknown reasons, the ICC committee meeting held in the last week of May 2001 did not take up this proposal and decided to continue with the D/L system.]

Theory

Let us come to the theoretical background on which the method is based on. The two curves shown in figure-1 constitute the body and soul of the method. Curve-B, called the normal score curve is the statistical curve. This represents the general scoring pattern of a team in a limited over cricket match. A number of closely fought matches are analysed to develop this curve. Different stages of the match are designated as different milestones (5, 15, 25, 30, 35, 40, 45 are important milestones in a 50 over match) and the general score percentages at these milestones are found out observing the data. A regression analysis is now carried out using a standard spread sheet software, and the using the coefficients an equation relating the cumulative % overs and cumulative % runs is developed. It is found that a cubical polynomial equation is the most suited one to represent the scoring pattern of a team. This is because the rate of progress in the score is not uniform in the normal curve. After the first 30% overs there is a decrease in the rate of progress and after 60% overs the rate of increase becomes sharper. The lowest-order-polynomial consistent with this pattern is the cubic one, and hence this is the simplest possible function to describe the normal score curve. As the cubical polynomial equation itself is found giving a sufficiently smooth curve, it was felt that there is no need to go for a higher degree polynomial. The data used for the regression analysis, the equation etc. are furnished in table-1 of, appendix-1.

The normal fall of wickets corresponding to the normal score percentages is also worked out from the data and furnished in table-2 of appendix-1.

The appendix-1 is self explanatory and for getting a better physical feeling of the situation, in table-1, the scores at each milestones in a 250 runs scored match is given. Similarly for the same purpose, in table-2, the actual overs and corresponding normal wickets in a fifty over match is indicated in brackets.

Now, the data in table-1 is rearranged in such a way that the maximum scored overs come first and the minimum scored overs fall last. This is done as per column-6 of table-1 which gives the ratio % runs / %overs. The modified table is shown as table-3. Again using the spreadsheet programme, the regression analysis is carried out for the rearranged data and the equation for the target curve (Curve-A) is developed.

These two curves are now capable of defining 90% of the situations in any limited over match. It should be noted that the scores corresponding to the normal score curve corresponds to the normal wickets expected at that stage and the score corresponds to the target curve corresponds to 9 wickets (as the team-2 can win the match by achieving this score even by losing 9 wickets).

It is observed that there is a certain amount of chance in any limited over match that the batting team loose wickets in quite an abnormal way thus tend to get all out before the scheduled quota of overs. Under this circumstance, it is necessary to stipulate, minimum % runs to be achieved corresponding to each % of fall of wicket. With the assumption that a team normally have seven capable batsmen and four bowlers, table-4 giving the minimum score% to be achieved against each wicket % is developed.

These equations and assumptions have formulated the basic ingredients required to analyse any match situation. Now what remains is to devise a systematic procedure to tackle the different problems in course of a match. The theoretical aspects related to this are explained while dealing with the different category of interruptions in the "application part". Generally in curtailed matches, the field restrictions, the number of overs per each bowler etc. are also reduced proportionately. As in this method, the score percentages are taken corresponding to the % of overs played/available, these changes are also taken care of to an appreciable level.

The target table

It is needless to explain in detail, the practical difficulties one may have, in taking values from these curves to solve the problems related to curtailment of overs. Especially when the wickets fallen at the time of interruptions differ from the ones corresponding to the normal curve and where the minimum score% criterion corresponding to the fall of wickets need to be looked into, the job becomes tedious. A user-friendly table, the target table shown in appendix-II takes care of these problems. It can be seen from the table that the column "target score%" is the one corresponding to the target score % curve in fig-1, and there are 10 columns corresponding to normal score percentages. These give the modified normal score percentages corresponding to different percentages of fall of wickets. The minimum score % criterion is also integrated into it. (An extended version of the table, which gives values corresponding to 0.1% of overs, is also available).

Application of the proposed method for different cases of interruptions:

Here, the whole problem of fixing target score is broadly categorized under three cases:

- 1 Case-A: The interruption is after team-1 have completed their innings and before team-2 begin.
- 2 Case-B: The interruption comes after team-2 have batted through some overs in their innings.
- 3 Case-C: The interruption is during the batting of team-1 itself.

Any problem related to fixing target scores can be included in one of the three categories or can be treated as a combination of two or all of these cases.

It will be helpful to keep the following point in mind while working out the problems.

- For overs played out before the interruption, always look at the normal score columns corresponding to the wickets fallen.
- For the overs remaining after the interruption, always look at the target score column. This is independent of the wickets fallen.

The Step by step procedure for Case-A:

- 1 Find out the percentage of overs team-2 get
- 2 Find out the corresponding target score % from the target table
- 3 Multiply the score made by team-1 with the value obtained in #2

Problem-1: Team-1, scores 264 runs in 50 overs. Before team-2 starts batting, an interruption occurs, and the match is reduced to a '42 over' one. Target score for team-2?

Solution:

'% overs' to be played by team-2 = $42/50 \times 100 = 84\%$.

>From target table, corresponding to 84% of overs, target % = 90.3

Hence, the target score = $90.3 \times 264 = \underline{239 \text{ Runs}}$.

Step by step procedure for Case-B:

- 1 Find out the percentage of overs played up to the interruption.
- 2 Find out the normal percentage of runs corresponding to #1 and the wickets fallen.
- 3 Find out the par score (say PAR-1) as, normal score % multiplied by the score of team-1.
- 4 Find out the percentage remaining overs with respect to the total overs remaining.
- 5 Find out the corresponding target percentage.
- 6 Multiply the target percentage of #5 with "the Total score of team-1 minus PAR-1" to get the target score in the remaining overs.

$RN = Pa$
 $R(1-N) = T$
 $RN + TR(1-N)$
 $TR + RN - TRN$
 $TR + RN(1-T)$
 $\frac{50-x-y}{50-x}$

$N \cdot T = Pa$
 $(1-N)T$

7 Add PAR-1 with the target obtained in #6 to get the Net Target.

Problem 2: LOI# 1442: Australia Vs. West Indies. Australia 252 in 50 overs; WI, after 29 overs, 138/1. 10 overs are lost. Target for WI in 40?

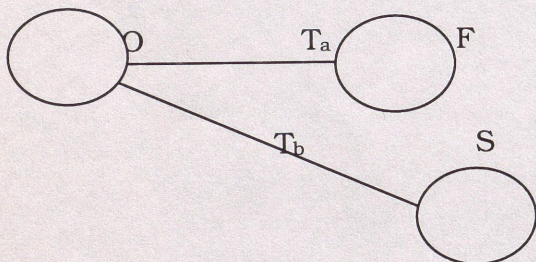
Solution:

% Overs played by WI at the time of interruption = 58
 Normal score corresponding to that = 48.4%
 $PAR-1 = 48.4 \cdot 252 = 121.97 \rightarrow (1)$
 % of remaining overs w.r.t. the total remaining overs = $11/21 \cdot 100 = 58.4$
 Corresponding target % = 65.6
 Target score for the remaining overs = $65.6\% \cdot (252 - 121.97) = 85.30 \rightarrow (2)$

Net target in 40 overs = $121.97 + 85.30 = 207.27 = \underline{208 \text{ runs.}}$

In case of a secondary interruption, the procedure will be to find out PAR-2 at the secondary interruption and add the target for the remaining overs with that.

- 8 $PAR-2 = PAR-1$ plus $(N2-N1)/(100-N1)$ multiplied by "target in the remaining overs" calculated in #6. Here, $N1$ = the normal score percentage with respect to the new base at the first interruption and $N2$ = the normal score percentage with respect to the second interruption with respect to the new base. (New base means the over corresponding to the Net target calculated in #7)
- 9 Target % in the remaining overs is calculated by multiplying the remaining runs (i.e. Net target as per #7 minus $PAR-2$) with T_c . Where T_c is the ratio of the target % as shown below.



$T_c = T_b / T_a$

(Instead of taking the target % from F to S one has to take it from the original point as shown above. Since the percentages of overs are used in this method, taking directly from F to S may arise a problem of inconsistency in results due to the change in base.)

10 Add $PAR-2$ with the target obtained as per #9 to get the 'Net target-2'.

Problem 3: In this particular match (problem-2) there was no further interruption. But let us now assume that there was another interruption after 35 overs when WI were at say 172/3. Now two more overs are lost.

Now the procedure will be to find our $PAR-2$ at 35 overs and add target for the 3 overs remaining after that.

$PAR-2 = 121.97 + (N2-N1)/(100-N1) \cdot 85.30$

N1= The normal score% for 29/40*100=72.5% of overs with 1 wicket lost.
Here it is= 62.3% (from the target table)

N2= The normal score % for 35/40*100=87.5 % of overs with 3 wickets lost.
Here it is= 80.6% (from the target table)

Hence PAR-2 = $121.97 + (80.6 - 62.3) / (100 - 62.3) * 85.3 = 163.40 \rightarrow (1)$

Target score for the remaining 3 overs = $T_c * (207.27 - 163.40) = T_c * 43.87$
 $T_c =$ (Target % for 15 overs to 3 overs) divided by (Target % for 15 overs to 5 overs)
i.e. Target for 20% divided by target for 33.33%
i.e. equal to $29.8 / 46.16 = 0.646$

Hence the target for the remaining overs = $0.646 * 43.87 = 28.34 \rightarrow (2)$

Target score for WI in 38 overs would be (1)+(2) $163.40 + 28.34 = 191.74 = \underline{192 \text{ runs}}$

Step by step procedure Case-C:

- 1 Find out the percentage of overs played up to the interruption.
- 2 Find out the normal percentage of runs corresponding to #1 and the wickets fallen.
- 3 Find out the percentage of remaining overs with respect to the total overs remaining.
- 4 Find out the corresponding target percentage.
- 5 Multiply the target % obtained in #4 with the remaining score%, i.e. 100 minus the normal score calculated in #2.
- 6 Add the percentages obtained in #2 and #5 to get the effective normal score of team-1 in total % overs played.
- 7 Find out the target % for the total % overs played.
- 8 Target % in #7 divided by the Effective normal score % in #6 will give the Multiplication factor (MF). It is proposed to keep the lower limit of this multiplication factor as 1.
- 9 Multiply the score made by team-1 with MF to get the target of team-2.

Problem-4: (Single interruption) LOI #1485 Sri Lanka Vs. Australia. Australia were 110/3 in 23.1 overs when the interruption took place. Seven overs were lost. Australia make 206 in 43. Target for Sri Lanka in 43.

Solution:

% of overs played at the interruption=46.32

Normal % with 3 wickets lost =42.8%

Remaining over %= $19.84 / 26.84 * 100 = 73.9$.

Corresponding target % = 83.2%

Effective normal score of Aus in 43 overs = $42.8 + (100 - 42.8) * 83.2\% = 90.39\%$

Target score% for 43 overs (86%)= 91.6%

Multiplication Factor (MF)= $91.6 / 90.39 = 1.0134$

Target for Sri Lanka in 43 overs= $1.0134 * 206 = 208.76 = \underline{209 \text{ runs}}$.

In the case of a secondary interruption the procedure will be:

10. Find out PAR-2 as PAR-1 Plus
 $(N2 - N1) / (100 - N1) * (ENS \text{ minus } PAR-1)$

11. Target % for the remaining overs as $T_c \cdot (\text{ENS minus PAR-2})$. Where $T_c = T_b / T_a$.
12. Add the results of #10 and #11 to get the new effective normal score.
13. Find out the target % for the total overs played.
14. Multiplication factor MF = Result of #13 divided by result of #12. (The lower limit is kept as 1 for game related reasons)

Problem-5: (Multiple interruptions) New Zealand were 86/4 in 26 overs when the first interruption took place and 1 over was lost. When they were at 114/5 in 32,4 overs their innings terminated. Target for South Africa in 32 overs?

Solution:

Normal score % in 26 overs (52%)

with 4 wickets lost (PAR-1) = 53.2%

Target score in the remaining 95.8% (23/24*100) overs =98%

Effective normal score in 49 overs (ENS)

$$= 53.2 + (100 - 53.2) \cdot 0.98 = 99.06$$

$$\text{PAR-2} = \text{PAR-1} + (\text{N2} - \text{N1}) / (100 - \text{N1}) \cdot (\text{ENS} - \text{PAR1})$$

Where:

$$\text{PAR-1} = 53.2$$

$$\text{N2} = \text{Normal score \% corresponding to } 32.67/49 \cdot 100 \text{ percent of overs} = 69\%$$

$$\text{N1} = \text{Normal score \% corresponding to } 26/49 \cdot 100 \text{ percent of overs} = 53.8\%$$

$$\text{ENS} = 99.06$$

$$\text{PAR-2} = 53.2 + (69 - 53.8) / (100 - 53.8) \cdot (99.06 - 53.2) = 68.3\%$$

Since team-1 did not bat any more after the second interruption Target for the remaining overs =0.

Hence the effective normal score of team-1 in 32,4 overs =68.3%

The target score in 32,4 overs (32.67/50*100 % overs) =76.7%

$$\text{MF} = 76.7 / 68.3 = 1.1230$$

$$\text{Target for team-2 in 32,4 overs} = 114 \cdot 1 / 1230 = 128.02$$

Target in 32 overs (target corresponding to 32/32.66*100 =97.98 overs which is 99.3%) = 99.3*128.02=127.12= 128 runs.

The computer programme.

To compute target scores using the procedure furnished here, the use of a computer is not quite essential. But it is always desirable. With the kind of availability of computers and Laptops these days, one must always be trying make use of them if they give any advantage. Hence, the author has also developed an interactive computer programme with the help of which all these computations can be done very easily and within seconds. The source programme is written in FORTRAN 77 and it works in the DOS mode.

Comparison with the existing system

1 Theoretical aspects:

The D/L method and the proposed method are developed based on two different concepts. But the objectives are the same and hence the theoretical

aspects can be compared without much trouble.

Please refer to figure-2. The curve-1 is similar to the following:

- 1 The curve used in the Norm method
- 2 A specimen curve of the D/L system.
- 3 The target curve of the proposed System.

If the calculations were done only based on the corresponding curves of the three methods, the results obtained from them also must be quite similar (except for the variations due to the data used for the statistical analysis). Of course for the 'Norm method' there is no second curve and hence it can't distinguish between the stages of interruption.

The D/L curves are not actually single ones. Each curve is a family curve. Or in other words each curve has one associated curve with it. In figure the curve-3 represents a sample 'associated curve'. When the first curve represents the resources remaining, the second one (curve-3) stands for the resources utilized. The Curve-2 of the figure represents the Normal score curve of the proposed system. This is the counterpart to the associated curve in the D/L system.

It can be seen from the figure that there is a significant change in the behavior of the two curves 2 & 3. This itself is the basic reason for the differences in results of the two methods in some cases. In the D/L system, the increase rate is exponential right from the beginning whereas in the proposed system such increased is observed only after 60-70% overs. In the initial part it behaves quite differently. Since the curve is developed based on data at different stages of the match, it is so happened that this curve lies closer to the actual match situation than the D/L curve. This is the basic reason why the results obtained from the proposed system become more acceptable.

There is no arbitrary constant in this method like the G50 the D/L system has. The scaling up is done purely based on the performance of team-1 in the more than 25 overs they play. If a model cannot fix a target based on "the performance of a team in more than 25 overs", it should be treated as the weakness of that model. The history should be used only up to the stage of arriving at a suitable model. It should not again be pulled into (as it is done in the D/L system), while the model is applied to the prevailing situation. A good model should give sensible results from the following facts: team-1 have played 'x1' overs, they have lost 'y1' wickets and they have scored 'z1' runs; what are the equivalent x2,y2 and z2 for team-2. The model presented here just does that.

2 The practical aspects:

This method was shown to many a category of experts in the field of cricket including players, umpires, statisticians et al. Every one has the unique opinion that the method always give sensible results. Whereas it is widely talked about the D/L system that it favours the team that is batting when the interruption occurs. Also when the interruption is in between the batting of the two teams, the target set for team-2 is quite high.

A table giving 10 situations where the author feels that his results are better

than the D/L results is furnished in Appendix-III. The author feels that these examples are good enough to understand the superiority of the proposed method over the present system being followed by the ICC.

Conclusions.

The author feels that the method proposed here is a very powerful mathematical tool for solving practically any problem related to fixing targets in an interrupted limited over cricket match. Also it gives results, which satisfy not only both the teams but also the millions of spectators and cricket lovers.

Acknowledgement: The author here by expresses his sincere gratitude to Dr. Sreenivas Bhogle, NAL, Bangalore, for his valuable suggestions, which helped him in making some significant improvements in the method.



CENTRE FOR ECOLOGICAL SCIENCES
INDIAN INSTITUTE OF SCIENCE
BANGALORE 560 012, INDIA

Telephone: 91-080-3600985/3092635/3092506
Telefax: 91-080-3601428
E Mail: nvjoshi@ces.iisc.ernet.in

Prof. N.V. Joshi

Date : 9 August 2002

To,
The Editor, Current Science

Ref: The m/s K311 "A scientific cricket matches" by V. Jayadevan

Dear Sir

Thank you for your letter of 22 June 2002 and the enclosed manuscript.

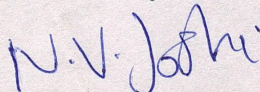
As subsequently discussed with you, I have been e-mail correspondence with the author over the last several weeks. In response to the various points raised in the discussion, the manuscript was extensively revised by the author. The manuscript can now be accepted for publication in Current Science.

The author has come up with an ingenious method of resetting targets in interrupted limited over cricket matches. The approach makes use of several mathematical and statistical techniques, and the article would be of considerable interest to a large number of readers of Current Science.

Please find enclosed a copy of the printout of the revised version of the article. I will also e-mail the corresponding file (msword) soon.

The article is revised to the extent that even the title has been changed. I have retained the reference number as K311. The article can be considered for the category General Article.

Thank you and with best regards


N.V. Joshi

Encl:

1. Copy of original article.
2. Copy of the article with some changes marked by Prof. Balaram (in green ink)
3. Copy of the final version of the article.

CURRENT SCIENCE

Editors

Prof. P. Balaram

Prof. S. Ramaseshan

Ref. No. GA959

22 August 2002

Prof. N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref :GA959
Methodological considerations in measurement of dominance in
primates

by Mewa Singh et. al.

Dear Prof. Joshi :

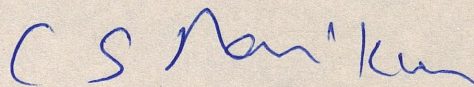
The enclosed article

Methodological considerations in measurement of dominance in
primates

by Mewa Singh et. al.

has been submitted for publication in the General Article
section of Current Science. We would appreciate if you
commented on its suitability for publication. Please send
your report as early as possible, preferably within
ten days.

Yours sincerely,



for Editors

General articles discuss current trends in research in a field that
will be of interest to readers outside the field; interdisciplinary
topics; science policy and administration; or some aspect of the
application of science and technology to human needs or the impact of
science and technology on society/ecosystem/life.

With Compliments

Prof N V Joshi

Can you see this and
Garfield's comment. We
could ask for a condensed
version. Can we get the
address of Cole/Cole.

P.B.

18/1/03

CURRENT SCIENCE

C.V. Raman Avenue, P.B. No. 8001,

Sadashivanagar, Bangalore - 560 080, INDIA.

(Telephone : 334 2310 Fax : 334 6094 e-mail : currsci@ias.ernet.in)

C M 981

"It is essential that they (Jonathan and Stephen Cole) be asked to referee this paper since they are the ones being criticized. Give Dr. Balaram their e-mail addresses.

It is ridiculous for this author to cite about eight papers written in Hungarian to which I or most others do not have access. Further, the authors should cite the titles of all the articles they cite, including the one by me.

You can tell them that I probably am in basic agreement with a lot of what he says, but this paper is excessively wordy as is a lot of his writing.

If they are willing to wait, and I don't see why there is such a rush for such a paper, then I would be glad to look at it more carefully once I have the additional information I need.

If what he says is true then it should be more condensed and then published once the Coles have a chance to respond. This was a highly cited and important paper. (I think he is referring to "The Ortega hypothesis. Science 178:368-75, 1972")

E.
Carbelle

Q A 981
28 802

**The False "Ortega Hypothesis":
Falsification, Indexed Influence and Reception**

*Endre Száva-Kováts**

The well-known anti-elitist "Ortega Hypothesis" published 30 years ago in Science has been constructed and named by two science sociologists, and refuted by themselves on the basis of a quantitative author "citation analysis" investigation carried out in the physics literature. Textual evidence presented here proves that the construction of this so-called "Ortega Hypothesis" having an anti-elitist meaning was based on falsification of the quoted text and misinterpretation of the doctrine of the eminent elitist Spanish philosopher. This anti-elitist, and hence anti-ORTEGA, false hypothesis spread very widely in the scientific literature as the "Ortega Hypothesis". The literatures of numerous disciplines have so far accepted this falsely constructed hypothesis: the fact of the falsity of the "Ortega Hypothesis" has not even arisen in the debates about it and under its name. Analysis of the literature case of the false "Ortega Hypothesis" also threw light on the current depressing state of referencing practice in the scientific literature.

The first and authorized English translation of "La Rebelión de las Masas" of the Spanish philosopher José ORTEGA Y GASSET, entitled "The Revolt of the Masses" appeared seventy years ago, in 1932 (1). In the 20th century this book was the best known explanation and popular synthesis of the doctrine of philosophical elitism rooted in the 19th century, and had a world-wide influence on intellectuals in the following decades. By the mid-20th century the revolt of the masses and the elitist doctrine of ORTEGA became a part of the higher curriculum of philosophy and history, and a part of encyclopedical knowledge – with the exception of the term "elitist", since even much later the most respected English-language encyclopedias (2,3) did not contain the term "elite". However, they stress the significance of "The Revolt" and briefly sum up its message, using the

* Retired Director-General, Library of the Hungarian Parliament.

Present address: Magyar István u. 2., Budapest, Hungary, H-1114

circumscriptions “minorities of cultivated and intellectually independent men” (2, vol. 4, p. 1015), or the “creative minority” (3, vol. 21, p. 11). European encyclopedias, which contain the term “elite”, have allusions to ORTEGA under that heading and list his works in the literature on elitist theories (see, e.g., 4, vol. 5, p. 465; 5, vol. 7, p. 709).

The elitist doctrine of ORTEGA

In “The Revolt” ORTEGA describes and analyses the phenomenon of how the common masses of people that emerged in the 19th century, the masses of mass-man quality, revolted against the rule of society by select minorities, the elite (6), and the consequences of this historical fact. The common masses only enjoy the benefits of civilisation, but they do not create and do not appreciate them because “the common man, finding himself in a world so excellent, (...) believes that it has been produced by nature, and never thinks of the personal efforts of highly-endowed individuals which the creation of this new world presupposed” (1, p. 63). According to the elitist ORTEGA the mass-man masses do not even maintain civilisation: “it is illusory to imagine that the mass-man of today, (...) will be able to control, by himself, the process of civilisation. I say process, and not progress.” (1, p. 73) According to the elitist ORTEGA “the mass-man in revolt” is a threat to the culture created and maintained by the personal achievements of the outstanding minority. As he sees it: “If that human type continues to be master in Europe, thirty years will suffice to send our continent back to barbarism.” (1, p. 57)

The view of *science* – “the root of our civilisation” – held by ORTEGA, the elitist philosopher of culture, is also naturally strongly elitist, in fact to the reader of today it appears to be excessively so. ORTEGA looks to the outstanding minority not only for the progress of science but also for maintaining the scientific results achieved. He approvingly cites the opinion of H. Weyl (7), “one of the greatest of present-day physicists, the companion and continuer of the work of Einstein”: “if ten or twelve specified individuals were to die suddenly, it is almost certain that the marvels of physics to-day would be lost for ever to humanity.” (1, p. 57)

The view of science held by ORTEGA, the historian and philosopher of culture, is thus elitist in the extreme, but not blindly so. He sees the complex and contradictory nature of reality, he sees and shows that, in contrast with the past, the men of science are no longer

identical with the elite. ORTEGA writes: "the actual scientific man is the prototype of the mass-man. Not by chance, but because science itself – the root of our civilisation – automatically converts him into mass-man, makes him a primitive, a modern barbarian." (I, pp. 120–121) ORTEGA sums up the explanatory cause of this in the title of the relevant chapter XII of his book: "The Barbarism of »Specialisation«". Its result is "a type of scientist unparalleled in history. He is one who, (...) is only acquainted with one science, and even of that one only knows the small corner in which he is an active investigator. He even proclaims it as a virtue that he takes no cognisance of what lies outside the narrow territory specially cultivated by himself, and gives the name of »dilettantism« to any curiosity for the general scheme of knowledge." (I, p. 122)

ORTEGA, the not blindly elitist philosopher and historian, sees the efficiency of the mechanical work of the intellectually barbarian specialist in his narrow section of science; in a single passage of his entire book (I, pp. 122–123) he expresses recognition of it in only one area of disarticulated science: in the field of "experimental science". The reason: "A fair amount of the things that have to be done in physics or in biology is mechanical work of the mind which can be done by anyone, or almost anyone." (I, p. 123) The masses of mass-man are capable of this mechanical mass-work of the mind in the experimental sciences, and perform it successfully.

However, even if mass-work is needed in certain areas of science and must be done, it is not the driving force of scientific progress. For this reason the elitist ORTEGA considers that the real progress of science cannot be expected from these intellectually barbarian specialists, whom he also calls "partially qualified men", and elsewhere uses the expression "learned ignoramus". Such progress requires the exceptional personal capabilities of the outstanding individual, and such individuals need something more and different from the mass of new tiny facts: "(...) Einstein needed to saturate himself with Kant and Mach before he could reach his own keen synthesis. Kant and Mach – the names are mere symbols of the enormous mass of philosophic and psychological thought which has influenced Einstein – have served to *liberate* the mind of the latter and leave the way open for his innovation." (I, p. 125) In short: the real progress of science requires geniuses

and geniuses need each other, not diligent learned intellectual barbarians and the mass of their tiny results.

ORTEGA's unequivocally but not blindly one-sided, elitist view of science was shaped and set down in the 1920s and applies to the state of affairs in Little Science after the beginning of post-classical new physics. It is not the aim of this present article to compare the present state of Big Science with ORTEGA's view at that time; its subject and task arises from its title. However, to carry out this task it must first be noted as a fact that ORTEGA's view of science was an integral part of his view of history and culture and like this wider view was unquestionably elitist. For four decades all the authors of publications of the relevant scientific and higher educational literature agreed on this fact.

Construction of the false "Ortega Hypothesis"

Thirty years ago, in 1972, forty years after publication of "The Revolt" of ORTEGA, a long article was published in *Science* entitled: "The Ortega Hypothesis: Citation analysis suggests that only a few scientists contribute to scientific progress" (8). At the beginning of their article the two science sociologist authors, J.R. Cole and S. Cole, characterize the progress of science as follows:

"In the past, historians and philosophers of science have attributed much of the growth of science to the work of the average scientist, who, it is suggested, has paved the way with his »small« discoveries for the men of genius – the great discoverers. This hypothesis is asserted in many sources, but perhaps no more clearly than in the words of Jose Ortega y Gasset (4):

For it is necessary to insist upon this extraordinary but undeniable fact: experimental science has progressed thanks in great part to the work of men astoundingly mediocre, and even less than mediocre. That is to say, modern science, the root and symbol of our actual civilisation, finds a place for the intellectually commonplace man and allows him to work therein with success. In this way the majority of scientists help the general advance of science while shut up in the narrow cell of their laboratory, like the bee in the cell of its hive, or the turnspit of its wheel." (8, p. 368, in the quoted text of ORTEGA: "in its wheel". E.Sz-K.)

Let it be noted not only for the sake of philological precision that the page numbers in the above reference act ["(4)"] of the COLEs citing the text of ORTEGA ("pp. 84–85") are erroneous; the correct figures are: pp. 122–123. Before continuing with the quotation of the

COLEs' text, it should be observed at once that not only in these three sentences quoted by the COLEs, but nowhere and never in the entire book containing these three sentences did ORTEGA "suggest" that the average scientists "paved the way" for the men of genius. It has already been shown that ORTEGA's view was precisely the opposite.

Directly after this, the COLEs interpret the three quoted sentences of ORTEGA in their own words as follows:

"Ortega seems to be suggesting that average scientists, working on relatively unambitious projects, make minor contributions, but that, *without* these minor discoveries by a mass of scientists, the breakthroughs of the truly inspired scientist *would not be possible*. Thus the work of the great scientist is built upon a pyramid of small discoveries made by average scientists." (8, p. 369, my italics. E.Sz-K.)

It can be seen that ORTEGA did not say this in the three sentences quoted and did not say what the COLEs claim he did in their interpretation. The COLEs did not interpret, they *falsified* the meaning of the three quoted sentences of ORTEGA.

A few lines down on the same page the COLEs repeat their own false interpretation as the conclusion:

"In short, it is proposed that the work of the average scientist *is indispensable* if science is to advance." (8, p. 369, my italics. E.Sz-K.)

In the three sentences quoted by the COLEs ORTEGA did not "propose" what the COLEs state in their »interpretation«. In their conclusion they further intensified the falsity of their interpretation, i.e., the falsification of the doctrine of ORTEGA.

This falsification is shown textually in Table 1.

My first thesis: the statements in the three quoted sentences of ORTEGA and the »interpretations« of the COLEs do not apply to the same thing, do not have the same meaning or apply to some »interpretations« of a non-existent statement. The two science sociologists did not interpret the view of the elitist philosopher ORTEGA but instead constructed a "hypothesis" that is anti-elitist in meaning and consequently anti-ORTEGA in meaning, and they unjustifiably called it the "Ortega Hypothesis".

So far no one has noticed that a passage of "The Revolt" sets out ORTEGA's own, true "ORTEGA Hypothesis" that has nothing to do with the false "Ortega Hypothesis" of the

COLEs. ORTEGA writes: "The European who is beginning to predominate – *so runs my hypothesis* – must then be, in relation to the complex civilisation into which he has been born, a primitive man, a barbarian appearing on the stage through the trap-door, a »vertical invader«." (1, p. 95, my italics. E.Sz-K.)

However, it was not enough for the construction of the false "Ortega Hypothesis" to take three sentences from ORTEGA's book out of context and misinterpret them. The two science sociologists did not do just that or that much. Behind the three sentences taken from ORTEGA, quoted and presented as continuous text by the COLEs, lies a philological crime. The COLEs took the 3rd, 4th and 10th sentences from paragraph 10, Chapter XII, merged them and then »quoted« and presented them as continuous text. They gave no indication that there were five sentences in ORTEGA's original text between the 4th and 10th sentences, that is, between their 2nd and 3rd sentences, which they simply left out. They left out these five sentences without indicating the fact and in doing so, misled readers.

It is hardly by chance that these five sentences were left out without indication, nor is it by chance that the three sentences taken out of context were merged and presented in this way. If we restore ORTEGA's original text and read it continuously – even only from its 3rd sentence, the one with which the COLEs begin their quotation, it can be seen immediately that this full text clearly *contradicts* the false "Ortega Hypothesis", not to mention the false interpretation. This full text of ORTEGA is given in Table 2, side by side with the "quotation" of the COLEs. The beginning of the original text, the first two sentences of the paragraph left out by the COLEs, containing ORTEGA's question raising the problem, are given in a footnote (9)

My second thesis: The COLEs constructed their false "Ortega Hypothesis" by treating ORTEGA's original text in a philologically unacceptable way, misleading readers.

Discussion

ORTEGA's original, unabridged text gives an unambiguous answer, not open to misinterpretation, to the question he posed (to the reader). ORTEGA's question: how is it possible that, in contrast with the past, even the intellectually commonplace man can also do successful work in a segment of modern science? ORTEGA's answer is a clear and

unequivocal, but not one-sided explanation which gives both the turnspits of science and the scientific men of genius their rightful place in the accumulation of knowledge.

The basic problem and the question to be decided in the COLEs' article is whether the progress of science is the result of the work of all scientists or depends only on the work of a narrow top stratum of scientists, the "elite". To represent the former view the COLEs constructed the false "Ortega Hypothesis" in the manner shown above. They then set out to examine the correctness of the false hypothesis and decide the question with the results of a quantitative author "citation analysis" carried out in the physics literature. This, however, requires an "elite" notion of a quantitative nature, something that is *ab ovo* anti-ORTEGA in essence. The COLEs constructed such a notion, and modestly defined it in a footnote:

"We use the term »elite« here and throughout in a statistical sense to refer to the small group of eminent scientists who publish the most, are most frequently cited, and occupy the most prestigious positions." (8, p. 374)

None of the criteria of this quantitative-statistical notion of "elite" correspond to ORTEGA's qualitative view and its concept of elite. The mass producers of publications in the age of Big Science and the method of counting formal citations to evaluate scientists have nothing in common with ORTEGA; on the other hand, this statistical notion of elite of the COLEs makes it possible to apply the method of quantitative "citation analysis" which is still strongly questioned in its meaning and value. This method is not inappropriate for the COLEs' notion and concept of elite; consequently, within the limitations of the immanent imperfections of the method, its use can lead to numerical results that are not meaningless but which can be interpreted with valid meaning only if the limitations are kept in sight. There is no space in this article, nor is it the purpose to criticize the quantitative "citation analysis" investigation of the "elite" carried out by the COLEs. It does not examine whether the COLEs correctly applied the method of quantitative citation analysis when in their opinion they refuted the "Ortega Hypothesis". What has to be said here is that with the use of this seriously problematic method and the numerical results obtained (and their interpretation which cannot be criticized here), they refuted the false hypothesis they had themselves constructed and which in reality had nothing to do with the true doctrine of ORTEGA. It must be said that for an examination of the correctness of the

true »ORTEGA Doctrine«, the use of the formal-quantitative “citation analysis” method is a pseudo-scientific procedure. It must be said that to falsify the text of ORTEGA and so construct a false “Ortega Hypothesis”, and to examine its validity with the method of quantitative “citation analysis” is not a scientific procedure. The COLEs declared the end result of an unscientific investigation as the answer to their own question, given as the subtitle of their article: “Only a few scientists contribute to scientific progress.”

It can be seen that the question raised and answered by ORTEGA was different from the question raised, examined and answered by the COLEs. It can be seen that, in refuting their own false “Ortega Hypothesis”, in reality they answered their own question – formally – with the real view of the elitist ORTEGA. Because in the problem and question of the COLEs, ORTEGA clearly and unequivocally stands for his concept of the “elite”; as he puts it: “And the worst is that with these turnspits of science not even the real progress of science itself is assured.” (*I*, p. 125) But by “elite” ORTEGA and the COLEs mean two different things, and it is not ORTEGA’s “turnspits” who figure in the COLEs’ investigation.

My third thesis: In the problem of the progress of science there is an identity of form between the »COLEs Doctrine« proven by a quantitative author citation analysis confutation of the false “Ortega Hypothesis” constructed in a non-scientific procedure by the COLEs, and the true »ORTEGA Doctrine«. But identity of form is not identity of meaning; and the quantitative citation analysis confutation of the false “Ortega Hypothesis” does not at the same time provide valid proof of the correctness of the true »ORTEGA Doctrine«.

Finally, mention must also be made of the final conclusion of the COLEs’ article. As pioneering proponents of the method of quantitative “citation analysis”, the authors equated the citedness of a publication with the extent of its use in scientific research, and its non-citedness with its non-use. Since they read and found that the majority of articles obtain very little or no citation, they reached the logical inference that these articles are not used. And if they are not used, they are (obviously) superfluous. If they are superfluous, they are unnecessary. Following this train of thought, the COLEs made what they considered the appropriate suggestion for science policy:

“We suggest that it may not be necessary to have 80 percent of the scientific community occupied in producing 15 or 20 percent of the work that is used in significant scientific discoveries, if perhaps only half their number could produce the same work.” (8, p. 373)

According to the COLEs:

“The data we have reported lead to the tentative conclusion that *reducing the number of scientists might not slow down the rate of scientific progress.*” (8, p. 374, my italics. E.Sz-K.)

The “reduction” in the number of scientists as both the conclusion of their article and the suggestion for science policy can be read once again in the article of the two science sociologists (10).

The train of thought is formally logical, but its point of departure is faulty; the suggestion for science policy was no doubt made in good faith, but it is in reality anti-science in nature, and in the Big Science age absurd in practice. However, what is more important here, is that it is also anti-ORTEGA in meaning: as we have seen ORTEGA did not attach very high value to the turnspits of science, but because he was not blindly elitist, he did not regard them, their work and its results as superfluous or unnecessary in experimental science. This difference confirms that the “identity of form” mentioned above was, in fact, only formal: only the words are identical, not the meaning.

The creation of the false “Ortega Hypothesis”: the falsification of the text and misinterpretation of the doctrine of ORTEGA Y GASSET, the pseudo-scientific nature of the COLEs’ investigation and publication, the identity of form of the final result of the COLEs’ investigation and of the true view of ORTEGA, and finally the absurdity of the COLEs’ suggestion for science policy were shown in more detail by the present author as the most important findings (among others) of his “Ortega Hypothesis” research in an article published in 1981 (11).

The two »literature cases« of the false “Ortega Hypothesis”

Three “letter” comments from well known physicists arrived in response to the COLEs’ article and were published two years later in *Science* (12–14). The position of the three physicists who did not study the physics literature from outside but, as publishing

researchers »produced« this literature, was unequivocally and entirely negative: they rejected the results of the COLEs' investigation, and their interpretation. They especially criticized the method of quantitative "citation analysis" used by the COLEs, applying solid arguments based on facts. According to Goudsmit "their article proves merely that citation statistics give a distorted picture of the way in which physics advances. Every physicist knows that in his research he uses a multitude of contributions made by others, (...) Only a few of these are cited;" (12, p. 28). He also pointed out, among others, the (formal) uncitedness of scientific achievements mentioned in the text »eponymally« (e.g.: "Jacquinot advantage"). Among the many critical arguments raised by McGervey against the results of the COLEs' investigation and their interpretation, there is space here for only two. Firstly that "They do not mention that some fields of research are more popular than others. A person working in acoustics will receive fewer citations than a worker in high energy physics." (13, p. 30) Secondly, McGervey was the only one to point out that "Although Ortega specifically referred to »experimental science«, Cole and Cole make no distinction between experimental and theoretical work," (13, p. 30–31), but he raised this objection not because of the falsification – which he did not notice – but in connection with the interpretation of the results of the COLEs. In their rejoinder (15), the COLEs did not reply to the critical arguments of the physicists based on facts, or did not reply substantively or validly; e.g., they gave no answer at all to McGervey's second critical observation quoted above. The rejoinder was not a real and correct answer, either logically or in substance, and the two sociologists did not accept the criticism on a single point. As regards the disputed problems and phenomena, the debate remained undecided and suspended. From the viewpoint of the present article it must be pointed out that

- in this debate which followed the publication of the false "Ortega Hypothesis", the fact of the falsification of the text and doctrine of ORTEGA did not arise, apart from the unanswered valid critical observation by the physicist McGervey. All this was shown in greater detail by the present author in his 1981 article (11).

In the view of the discipline of »literature science«, held by the present author, the basic article of the COLEs and the subsequent debate (8, 12–15) are closely intertwined intellectually and form a single distinctive »literature case«, in the narrower sense of the

term. However, the expression also has a wider meaning: the primary »case« and its influence on the literature, i.e. its reception in the literature, taken together. In order to be able to judge this literature case in the broader sense of the false "Ortega Hypothesis", the indexed influence and reception of the primary case needs to be presented briefly.

The first investigation

The indexed influence of the primary "Ortega Hypothesis" literature case on the scientific literature, i.e. the indexed reception of the false "Ortega Hypothesis" of the COLEs in the scientific literature, was investigated by the present author, at first up to 1980. Not including the secondary communications, in the three series of the Citation Indexes 69 receptive articles were found which referred to the COLEs' article and/or one or more items of the subsequent debate (16). The immediate and wider context of the 88 reference acts to be found in the 69 articles citing the primary case was analyzed, and the meaning of the citing references was established. All the 88 citing reference acts were evaluated from the viewpoints of 9 categories, and were placed in an *ad hoc* classification system containing 135 elements; a system of symbols was also elaborated and applied in the course of the investigation, and the results of the analysis of each citing reference act were summed up and expressed by a formula in a 13-digit numerical code. Finally, with the aid of the numerical code and 12 elaborated analytical tables it was possible to make a detailed analysis and quantified judging criticism of the nature and meaning of the 69 citing articles and their 88 citing reference acts. The findings of the investigation were published in 1982 (17). Among these, only the following can be mentioned here:

- the formal-indexed influence of the primary literature case of the false "Ortega Hypothesis" (especially that of the basic article of the COLEs) was strong, and was found particularly in the literature of sociology and of information science, i.e., the receptiveness was the strongest there;
- apart from these fields, reception of the false "Ortega Hypothesis" was also found in numerous topically peripheral disciplines remote from each other and from the history and sociology of science; e.g.: in the literature of analytical chemistry (18), alcohol research (19), animal behaviour (20), bioscience (21), economics (22), education (23), immunology (24), jurisprudence (25), and so on;

- the deeper analysis showed that the discipline of the journal publishing a receptive article did not determine the discipline of the literature material processed, e.g. a receptive article on the findings of an investigation carried out in the literature of toxicity (26) and of dairy research (27) appeared in the *J.Am.Soc.Inf.Sci*; and also that the subject of the receptive articles appearing in the widely differing disciplinary journals was in most cases the problems of the "citation analysis" method;
- only one content element of the primary literature case remained without influence, the final suggestion for science policy made in the COLEs' basic article: out of the 88 citing reference acts 86 made no mention of it, 1 treated it with reservations and 1 rejected it;
- the reception was generally positive in the case of the other content elements: not including 15 meaningless or neutral reference acts, of the remaining 73 reference acts adopting a position only 14 unequivocally rejected the COLEs' basic article or one of its content elements, or accepted one of the critical arguments of the criticizing physicists;
- of the 14 reference acts that were rejecting in substance, not a single one rejected the falsity of the "Ortega Hypothesis" – the fact of the falsification of ORTEGA's text and doctrine did not even arise until 1980 in the scientific literature receiving the false "Ortega Hypothesis" of the COLEs; the falsity of this hypothesis remained entirely unobserved except perhaps for the single doubtful case of one reference act by Garfield (28).

The final result of the first investigation by the present author: in the first years of publication (up to 1980) the indexed influence of the false "Ortega Hypothesis" on the scientific literature was strong and very extensive. Failing to recognize the falsity, this literature accepted the false "Hypothesis" without reservation.

The second investigation

The indexed influence of the "Ortega Hypothesis" literature case in the narrower sense, i.e., the indexed reception of this false "Hypothesis" remained substantial even after 1980 and continued to spread through a wide range of disciplines: since then 95 indexed citations were found for the basic article of the COLEs alone. A survey carried out by the present author, less detailed and thorough than the previous analysis, among others found the following:

- the indexed reception of the basic article of the COLEs was now most intensive not in sociology but in the literature of scientometrics: out of the 95 citing articles 22 were published in the journal *Scientometrics*; in this journal in 1987, a short but severely critical article of MacRoberts and MacRoberts (29) led to a new debate comprising 13 comments (30), nominally under the common main title of "Ortega Hypothesis" given by the editors, but in reality mainly about the problems of the citation analysis method, although only 5 of these formally cited the COLEs' basic article;
- the indexed influence spread further: citation of the basic article of the false "Ortega Hypothesis" appeared in new fields of topically remote sciences, disciplines and branches; e.g. in the literature of criminology (31), drug metabolism (32), forestry (33), geography (34), pathology (35), psychology (36), statistics (37);
- there has been a strengthening of the tendency for the participants to conduct the debate on the problems of the citation analysis method in journals of different fields, in cases citing the basic article of the COLEs; the most consistent and outstanding critics of the method, MacRoberts and MacRoberts, have published in the following journals, sometimes more than once: *Soc.Stud.Sci.* (38), *Nature* (39), *Trend Biochem.Sci.* (40), *J.Am.Soc.Inf.Sci.* (41), *Scientometrics* (42);
- although from the viewpoint of content, the influence and reception shifted largely to the method of citation analysis and its problems, and the COLEs' basic article is regarded as "the most outstanding application" of this method, "which is a refutation of the Ortega Hypothesis" (43, p. 166), other, less pregnant content elements of the article still had an influence too and were received, e.g., the "halo effect" (37), or the "peer review" (44);
- the falsifying misinterpretation of the original doctrine of ORTEGA continued and progressed: "prolificacy" appeared as a criterion within the "Ortega Hypothesis" (45), an idea which is rooted only in the COLEs' definition of the "elite";
- there was an increase in the number of reference acts involving a group of publications, where the COLEs' basic article is only one item in the group; e.g., in a group of 10 items (46, p. 800), in a group of 15 items (37, pp. 314–315);
- both bibliographically standard secondary citations and secondary citations of a hidden or disguised nature appeared; e.g., an article was found in which both the falsified text of

phenomenon is very high, and its scattering is very limited.” (60, p. 54) Taking into account the two real but non-indexed kinds of literature citedness – NIEC and ICR – it was stated that the formal citations listed in the Citation Indexes comprise only a fragment of the real reference stock of the physics articles, the formal-direct fragment which the referencing author, citing it intentionally in this way, placed in the (indexed) showcase of present Big Science. These recent results place the method of quantitative citation analysis, the meaning, value and validity of its numerical results, in an entirely different light, and so also the investigation of the COLEs in which this method was applied.

In 1995, two librarians, Hoerman and Nowicke, published a remarkable article in *Lib. Quar.* (61). Continuing the work of Broadus (62), MacRoberts and MacRoberts (63), Sweetland (64) and others in a new field, they examined “an aspect of citing behaviour, secondary and tertiary citing, as evidenced by a pattern of citation errors and transformations in meaning related to the Ortega Hypothesis”, as defined by the COLEs (61, p. 415). Their investigation also brought other findings of interest, identical with the main findings of the present author’s first “Ortega Hypothesis” article (11), or reinforced them. In this way Hoerman and Nowicke (a) “discovered not only that the pagination in the Cole and Cole reference was incorrect, but that the text of Ortega y Gasset excerpted by Cole and Cole was presented in an altered form” (61, p. 420); (b) they pointed out and demonstrated the philologically incorrect procedure of the COLEs in quoting the text of ORTEGA: “between the second and the third sentences of the quote, text was omitted without inserting an ellipsis” (61, p. 424); (c) they noticed and regarded it as ironical “that, in the process of transforming the meaning of José Ortega y Gasset and then disputing that transformed meaning, Cole and the many authors who, like him, reject the Ortega Hypothesis, may be said to agree with Ortega y Gasset.” (61, pp. 417–418) (65); (d) among the documents in their set (the composition of which was not given in detail) “were twenty-six that used the exact term »Ortega Hypothesis.« None of these documents correct the directed misregistration or evolved meaning; only fourteen of these articles cite Cole (...), or Cole and Cole (...).” (61, p. 427) (65); (e) finally, they recognized the special case of Garfield [see note (28) of this article] and this is one of their conclusions: “With the

possible exception of Garfield (...), no one in the bibliometric literature has questioned the directed misregistration of the meaning of Ortega y Gasset." (61, p. 428)

Conclusion

The well-known "Ortega Hypothesis" is a false hypothesis, constructed by two science sociologists through the falsification of the text and doctrine of the eminent elitist Spanish philosopher José ORTEGA Y GASSET. In the scientific literature this anti-elitist, i.e. anti-ORTEGA in meaning, false hypothesis spread widely as the "Ortega Hypothesis". The scientific literature, so far failing to recognise the falsity, has accepted this false hypothesis. To date, in the disputes about and in the name of this hypothesis – but mainly on quantitative "citation analysis" – the fact of the falsity of the "Ortega Hypothesis" has not arisen. The earlier (1981) and more recent (1995) communications of librarians demonstrating the falsifications, published in the literature of library and information science, have so far had no effect. Analysis of the literature case of the false "Ortega Hypothesis" threw a sharp light on the current depressing state of referencing practice in the scientific literature and on the inadequate nature of its capacity and readiness to process and evaluate the cited literature.

Table 1 The falsification of ORTEGA's doctrine

The text of ORTEGA*	The text of the COLEs*
“... the intellectually commonplace man ...”	“... the average scientist ...”
“... experimental science ...”	“... if science is to advance.”
“... finds a place for the intellectually commonplace man and allows him to work ... with success.”	“... the work of the great scientist is built upon a pyramid of small discoveries made by average scientists.”
—	“... without these minor contributions ... the breakthroughs of the truly inspired scientist would not be possible. ”
—	“... the work of the average scientist is indispensable if science is to advance.”

* My boldface (E.Sz-K.)

References and notes

1. J. Ortega y Gasset, *The Revolt of the Masses*, Norton, New York, 1932.
2. *The New Encyclopaedia Britannica*, Micropaedia, 15th ed., Encyclopaedia Britannica, Chicago, etc., 1988.
3. *The Encyclopedia Americana*, Americana, New York, etc., 1963.
4. *Brockhaus Enzyklopädie*, 17th ed., Brockhaus, Wiesbaden, 1996–1974.
5. *Meyers Enzyklopädisches Lexikon*, 9th ed., Bibl.Inst., Mannheim, etc., 1971–1979.
6. It may appear ironical that the elitist ORTEGA never used the word “elite”. He could not have used it, because at the time of “La Rebelión” this word did not yet exist in the Spanish language. His expressions: “minorías selectas”, “minorías excelentes” (etc.) coincide with the meaning of “elite” given in more recent Spanish dictionaries; see, e.g. *Vox Diccionario General Ilustrado de la Lengua Española*, Bibliograf, Barcelona, 1980, p. 669.
7. The text of the English translation is faulty: here it gives “Wely” in place of “Weyl”.
8. J.R. Cole, S. Cole, *Science*, 1972, **178**, 368–375.
9. “What happens is that, enclosed within the narrow limits of his visual field, he does actually succeed in discovering new facts and advancing the progress of the science which he hardly knows, and incidentally the encyclopedia of thought of which he is conscientiously ignorant. How has such a thing been possible, how is it still possible?” (1, p. 122)
10. “Thus the basic question emerges, whether the same rate of advance in physics could be maintained if the number of active research physicists were to be sharply reduced.” (8, p. 372) “What we are suggesting is that science would probably not suffer from a reduction in the number of new recruits and an increase in the resources available to the resulting smaller number of scientists.” (8, p. 374)
11. E. Száva-Kováts, *Tud.Műsz.Táj.*, 1981, **28**, 337–358.
12. S.A. Goudsmit, *Science*, 1974, **183**, 28.
13. J.D. McGervey, *Science*, 1974, **183**, 28, 30–31.
14. R.J. Yaes, *Science*, 1974, **183**, 31–32.
15. J.R. Cole, S. Cole, *Science*, 1974, **183**, 32–33.
16. It is worth noting that the COLES’ basic article appeared in the Citation Indexes in 5 cited variations and the 5 publications of the »literature case« in the narrower sense in 17 cited variations.
17. E. Száva-Kováts, *Tud.Műsz.Táj.*, 1982, **29**, 475–525 and the references cited therein.
18. T. Braun, W.S. Lyon, E. Bujdosó, *Analytical Chemistry*, 1977, **49**, A682–A688.
19. J.K. Moll, F. Narin, *J.Stud.Alcohol*, 1977, **38**, 2165–2180.
20. D.L. Hull, *Animal Behaviour*, 1978, **26**, 685–697.
21. S.M. Lawani, *Bioscience*, 1977, **27**, 26–31.
22. H.G. Grubel, *Malayan Econ.Rev.*, 1980, **25**, 1–18.
23. S.M. Segal, T.V. Busse, R.S. Mansfield, *Am.Educ.Res.J.*, 1980, **17**, 491–502.
24. B.H. Waksman, *J.Immunology*, 1980, **124**, 1009–1015.
25. P. Shuchman, A. Gelfand, *Emory Law J.*, 1980, **29**, 195–230.
26. R.R. Montgomery, *J.Am.Soc.Inf.Sci.*, 1973, **24**, 402–403.
27. A. Mendez, C. Blanco, *J.Inf.Sci.*, 1979, **1**, 113–116.
28. This is a special, because ambiguous, case within the »literature case« of the “Ortega Hypothesis”: that of E. Garfield, creator of the Citation Indexes, grandiose

computerized bibliographic tools, who unintentionally made possible quantitative citation analysis evaluating scientists, and scientometrics. In 5 reference acts in his 4 indexed articles he cited the basic article and the rejoinders of the COLEs, and the comment of McGervey. Although he does not reject the work of the COLEs in any of his articles, in fact he accepts it, he is fairly critical of the method of citation analysis, and in one of his 5 reference acts (in 1975) he is the only one in the »case« to doubt the COLEs and their Hypothesis, because referring to "The Revolt" he sums up Ortega's view as follows: "This theory asserts that advances in science depend in part on the contributions of mediocre scientists." See the detailed analysis of Garfield's articles and reference acts in (17), p. 515.

29. M.H. MacRoberts, B.R. MacRoberts, *Scientometrics*, 1987, **12**, 293–295.
30. See *Scientometrics*, 1987, **12**, 297–353.
31. G.S. Green, *Criminology*, 1981, **19**, 45–52.
32. H. Boxenbaum, S.J. Ruberg, F. Pivinski, *Drug Metab. Rev.*, 1987, **18**, 553–571.
33. H. Gregersen et al., *J. Forestry*, 1989, **87**, 21–26.
34. J.W.R. Whitehand, *Prof. Geographer*, 1988, **40**, 461–462.
35. E. Foucar, *Mod. Pathology*, 1996, **9**, 1175–1177.
36. D.K. Simonton, *Psychol. Rev.*, 1997, **104**, 66–89.
37. D. Cox et al., *Stat. Sci.*, 1993, **8**, 310–317.
38. M.H. MacRoberts, B.R. MacRoberts, *Soc. Stud. Sci.*, 1982, **12**, 443–450.
39. —————, *Nature*, 1987, **327**, 456.
40. —————, *Trends Biochem. Sci.*, 1989, **14**, 8, 10, 12.
41. —————, *J. Am. Soc. Inf. Sci.*, 1989, **40**, 342–349.
42. —————, *Scientometrics*, 1996, **36**, 435–444.
43. L. Ikpaahindi, *Libri*, 1985, **35**, 163–177.
44. R. Roy, *Minerva*, 1984, **22**, 316–328; the cited title of the COLEs' article is erroneous: "Criterion Analysis" in place of "Citation Analysis", see p. 317.
45. J. Berg, R. Wagner-Döbler, *Scientometrics*, 1996, **35**, 321–346.
46. J. Anderson, D.C. Evered, *Lancet*, 1986, **2**, 799–802.
47. C.A. Schwartz, *Coll. Res. Libr.*, 1997, **58**, 19–29.
48. J. Woodward, D. Goodstein, *Am. Scientist*, 1996, **84**, 479–490.
49. E. Száva-Kováts, *Tud. Műsz. Táj.*, 1987, **34**, 523–542.
50. —————, *Tud. Műsz. Táj.*, 1988, **35**, 195–219.
51. —————, *Tud. Műsz. Táj.*, 1989, **36**, 291–319.
52. —————, *Tud. Műsz. Táj.*, 1989, **36**, 515–535.
53. —————, *Tud. Műsz. Táj.*, 1991, **38**, 83–101.
54. —————, *Tud. Műsz. Táj.*, 1992, **39**, 178–192.
55. —————, *J. Inf. Sci.*, 1994, **20**, 55–70.
56. —————, *Curr. Sci.*, 1997, **72**, 705–707; 1997, **73**, 385.
57. —————, *J. Am. Soc. Inf. Sci.*, 1998, **49**, 477–481.
58. —————, *J. Am. Soc. Inf. Sci.*, 1999, **50**, 1284–1294.
59. —————, *J. Am. Soc. Inf. Sci. Tech.*, 2001, **52**, 201–211.
60. —————, *J. Am. Soc. Inf. Sci. Tech.*, 2002, **53**, 47–56.
61. H.L. Hoerman, C.E. Nowicke, *Lib. Quar.*, 1995, **65**, 415–434.
62. R.N. Broadus, *J. Am. Soc. Inf. Sci.*, 1983, **34**, 132–135.
63. M.H. MacRoberts, B.R. MacRoberts, *Soc. Stud. Sci.*, 1986, **16**, 151–172.
64. J.H. Sweetland, *Lib. Quar.*, 1989, **59**, 291–304.

65. Hoerman and Nowicke took into account and also indicate here that the primary source of the idea and later of the term "Ortega Hypothesis" are the 1969 dissertation and later the 1970 article of J.R. Cole; see (61, p. 419).

CURRENT SCIENCE

Editors

Prof. P. Balaram

Prof. S. Ramaseshan

Ref. No. CO205

2 January 2004

Prof. N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref :CO205
Higher education in a catatonic state?

by V. Sitaramam

Dear Prof. Joshi :

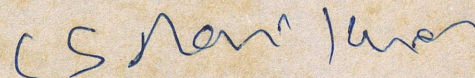
The enclosed article

Higher education in a catatonic state?

by V. Sitaramam

has been submitted for publication in the "Commentary" section of Current Science. We would appreciate if you commented on its suitability for publication. Please send your report as early as possible, preferably within ten days.

Yours sincerely,



for Editors

Commentary articles should contain expository notes on issues related to

HIGHER EDUCATION IN A CATATONIC STATE?

007205

231203

V.Sitaramam,
Biotechnology Department ,
University of Pune,
Pune - 411 007.

Phone(O): + 91 20 569 5179

Fax(O): + 91 20 569 5179

email:sitaram@unipune.ernet.in

Outcomes research is a major effort in medical practice to match the treatment with disease. Education is one area that could eminently do with outcomes research. However, even the most rudimentary considerations do not seem to have percolated to the level of teachers in Universities since research in higher education and educational psychology are virtually non-existent and hence this note more as a plea. The university establishments are as confused as the government by mixing up between mass education and mass higher education!

Long ago, Sir Ashley Cooper commented regarding orthopaedic patients that "the inside of bones is not filled with red marrow but black ingratitude", since the outcome often is unsatisfactory, especially in those days, in the way the bones set. Many teachers, particularly the contributory teachers in teaching programmes, are often driven to similar despair. How true is it that there exist a segment of students that cannot be taught? Surprisingly very little information is available in higher education.

Part of the reason for our ignorance stems from the fact that to declare a student as a nonlearner is politically incorrect and tremendously so. In any public meeting or policy meeting, the powers that be must wax eloquent with mindless mix of metaphors...about bright young minds, bubbling with enthusiasm, with pervasive and infectious enthusiasm, who are sponges that absorb knowledge and reflect creative minds and spirits, they being future of India (or whatever). Some of them even believe in this. More privately when I talk with the faculty around, it appears that not more than 10-15% of the students we have should really have entered the University portals(1). I know departments where the

faculty speak enthusiastically about the very bright student that actually studied there 3-4 years ago. There are also occasional teachers who are confident about how gifted they are in motivating students, as opposed to the rest.

The question is not so much as to whether we have non-learning (= much less than the minimum level required for a given programme) students, but rather... to define and discover what constitutes a non-learner and to define whether the phenomenon is endemic, epidemic or pandemic. The origin, evolution and manifestation of a non-learner should occupy the central stage in modern educational scene for a very scientific reason. The purpose of science is to explain the most common and not the most rare. If these represent the bulk of teaching targets, what can be done to improve the lot....not just the students, but more importantly, of teachers and institutions?

Is statistics useful?

It depends. There is a very common illusion among teachers that there is a syllabus and that if delivered, it results in marks in a Gaussian manner... some good, some not bad, the rest tailenders. In reality, it depends on the nature of the test, which the teacher continuously adjusts the level of the test in search of this Gaussian curve. Typically, tougher the expectations, more skewed with a tail to right is the response. A populist ranking may give a somewhat normal looking distribution. It is more a normalization rather than being normal in any sense of the word. If all universities were to get 5 star rating from NAAC, it represents yet another distribution, that speaks for itself.

The marks of students of biotechnology students, analyzed over a decade were published earlier in this journal (2). The current observations reflect the concerns since

then. Marks, like poverty/riches, do not simply indicate numbers. These also summarize effectively various attitudes.

Evolution of a non-learner.

Non-learners appears to be a carefully sculpted product of a caring community. He/she is, as often as not, a product of care and not of neglect. At every stage as the child falters, caring elders carefully give support to sustain the errors so as not to spoil the psychology of the youngster. Nearly all modern parents / grand parents are amateur psychologists, with experiences of having controlled huge sample sizes, 'n', ranging anywhere from 2- 3 ! Every body jostles for the front seat in fairness games: exams which are fair, plenty of choice, number of repeat exams permitted , open book exams, no negative marking, internal marks based on the act of submission of journals rather than any content, electives and so on. Once a dissociation occurs between a student and his classroom activity, a catatonic state spervenes where the teacher simply no longer communicates with the student. Catatonic states can be intrinsic or induced.

I had the occasion to observe first hand and analyze gradual deterioration of student quality over the last several years in biotechnology (2). It is amazing how reluctant the faculty are to make an open statement in any meeting. The initial days, with very limited seats, younger faculty (that was usnovelty of the programme, all geared us to high expectations and the syllabi were similarly designed. Practicals were ambitious, long hours permitted, great deal of theory was discussed. Basically the students were those who just missed getting into professional colleges. The blissful state lasted nearly the first ten years(3). Nearly every student passed the CSIR-UGC exam. More than 50% students could handle the entire syllabus as we hoped.

Then the scene crashed. The IT bandwagon removed most aspirants. The BT bandwagon that followed led to proliferation of programmes from the original six universities to over 2-3 dozen. Even when we were scraping the bottom of the barrel, the barrel did not get any significantly larger. The best students we had were those with physics and chemistry background. Now most come from biology background, some now even from biotechnology background. The consequences to the quality of teaching programme are terrible. Table 1 summarizes some of the observations over the years, all which could be handled when a major subgroup were high rankers, as opposed to the current situation where majority are low rankers.

Non-learners. Is the process autocatalytic?

The greatest fear is not about a particular mind that failed. The fear is about the collective failure that stops the society in its tracks. If the non-learners become the teachers, as many have, the process accelerates and there is no way to tell where the bottom line is.

It is a general suspicion that we have already reached this by completing more than 1 cycle. More and more students tell us about the total desolation that grips the classroom, where the mediocrity rules. What is not clear is whether it is a level off or a dive. Is the phenomenon specific to colleges or are the schools also affected? It appears that the saving grace for schools (I hope that it is practised even today) is that the teachers have some training in pedagogic process called a B.Ed.. College and University teachers mostly have no formal training even in routine matters(5). We are shocked to find instances where departments have routinely forbidden students' requests to do additional practicals on the basis that these are not in the syllabus!

It was often stated that the purpose of University teaching is to provide college teachers (1). It was equally often stated that one may not be a good researcher but one may be a good teacher. The sum total of the apologia is that we have universities filled with unattested 'good' teachers, who are training teachers who have no way to be 'learners' and can only be 'good' teachers. How can you be a good teacher unless you are also a good learner? How can you be good learner unless you have ever been exposed to a good reseach department? It is extremely easy to trace how and why our education is top down .. we teach and we do not learn, and our research is only 'me too' since we cannot formulate a problem.

The symptoms are all there. If, year after year, not one doctoral dissertation requires standard deviation to be calculated or t-test performed in a biology department, wherein lies the remedy?

Does a foreign model exist?

It is convenient if we have successful foreign models since we simply cannot afford wastage of national money on any new idea that has no guarantee of prior success abroad !

Fortunately, lousy students have been handled abroad successfully. Where else except in the God's own country, the US of A? By lousy students I mean your classmates, your juniors, some of them whom you have taught...mostly those who should not have made any grade by any stretch of imagination have done well. Most have developed even larger heads but many have actually done better than what they would have done here. Why? Come to USA/UK/Australia...wherever. Get good education that adds to your prospects. There seems to be some truth in the ads.

The American system has made most of its universities, it imports students if necessary to have continuing programmes, the campuses have research compulsarily and students are all exposed to working departments, where finding a lamp lit after 10.00 p.m. cannot only mean a burglar! A distinction is made between authentic and second hand teaching and experiences. Above all, there are usually interval review processes with some honesty.

What do we do?

When education was made a concurrent subject between the state and the centre, the distinction was not made between higher and education and mass education (1,4). Is it time we make this distinction? Should there be an education cadre? Should we leave it in the hands of amateurs and 'entrepreneurs with business models'?

If the level of students is low, then the only teaching that makes sense is that in which the students do things themselves. However, this makes the involvement of teachers even more because setting up problems that the students have to work their way through is not the way science is ever taught. The general approach is to drop dollops of instruction and protocols in the lap of students, which they have to reproduce. The "non-learner" trap is such that the teachers cannot relate to students' minds and the students also see no point in such an attempt. The committees appointed to arrive at policies relate to neither. Essentially we have gimmicks from various committees and not solutions. It is time we find a way out.

References:

1. V. Sitaramam The Indian Academy of Sciences document on science education in universities : A criticism., *Curr, Sci.* 69, 89-94, 1995.
2. V Sitaramam On recommending students: the curious case of all students being in the top ten per cent.. *Curr. Sci.* 75, 647-652, 1998.
3. V.Sitaramam Biotechnology at Pune. Beginning or the end? *Curr.Sci.* 60, 537-540, 1991.
4. V.Sitaramam, Dynamics of the psychology of the Ph.D students and the question of what to do with them. *Curr. Sci.* 68,779-782,1995.
5. V. Sitaramam The question of post-doctoral fellowship: A feasible case for apprenticeship in university teaching, *Curr. Sci.* 79, 19-21, 2000

Tab 1. Some attitudinal differences in students.

Parameter	High rank*	Low rank**
Comprehension in physical and abstract ideas	reasonable	Strictly avoided
Commitment to programme	acceptable	Marginal to absent
Assignments	Interested? Not critical, but will perform.	Disinterested. Not critical but others may do and we can copy.
Absenteeism	low	High
General reading	Acceptable to low	Very low
Choice of optional subjects	Primarily by marks; a few choose based on interest	Primarily by scores possible
Attitudes to marks	Very important	Very important but without effort
Class conduct	Can be enthused	refractory
M.Sc. as a degree	Necessary but not important further.	Necessary but not important at all.
Social practices		
Invigilation in exams	Not usually required	Required
Copying	Not generally accepted.	Acceptable >80%
Listening to seniors and peer pressure	Low	Very high
Socializing	Important	Important

*80-85% at least in 10 + 2 level. ** 65% or below

Tab 1. Some attitudinal differences in students.

Parameter	High rank*	Low rank**
Comprehension in physical and abstract ideas	reasonable	Strictly avoided
Commitment to programme	acceptable	Marginal to absent
Assignments	Interested? Not critical, but will perform.	Disinterested. Not critical but others may do and we can copy.
Absenteeism	low	High
General reading	Acceptable to low	Very low
Choice of optional subjects	Primarily by marks; a few choose based on interest	Primarily by scores possible
Attitudes to marks	Very important	Very important but without effort
Class conduct	Can be enthused	refractory
M.Sc. as a degree	Necessary but not important further.	Necessary but not important at all.
Social practices		
Invigilation in exams	Not usually required	Required
Copying	Not generally accepted.	Acceptable >80%
Listening to seniors and peer pressure	Low	Very high
Socializing	Important	Important

*80-85% at least in 10 + 2 level. ** 65% or below

CURRENT SCIENCE

Editor

Prof. P. Balaram

S. Ramaseshan

Ref. No. GA1146

13 February 2004

Prof. N.V. Joshi
Centre for Ecological Sciences
Indian Institute of Science
BANGALORE 560 012

Ref :GA1146

Is the fertility status of agricultural soils of India going down over the years?

by H. Pathak et. al.

Dear Prof. Joshi :

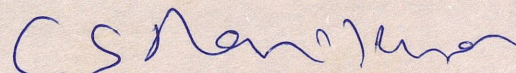
The enclosed article

Is the fertility status of agricultural soils of India going down over the years?

by H. Pathak et. al.

has been submitted for publication in the General Article section of Current Science. We would appreciate if you commented on its suitability for publication. Please send your report as early as possible, preferably within ten days.

Yours sincerely,



for Editor

General articles discuss current trends in research in a field that will be of interest to readers outside the field; interdisciplinary topics; science policy and administration; or some aspect of the application of science and technology to human needs or the impact of science and technology on society/ecosystem/life.

2 Km CA 1146
30106

Is the fertility status of agricultural soils of India going down over the years?

H. Pathak*, A. Chatterje, N. Kalra and U. K. Singh

Unit of Simulation and Informatics, LBS Building, Indian Agricultural Research Institute, New Delhi 110 012 *Email: him_ensc@iari.res.in

Intensification of agriculture and imbalanced use of plant nutrients increase risks of soil degradation. Global climate change, which has recently drawn a considerable attention, will further aggravate the problem. In this study the data from soil testing laboratories and published literature were analyzed to estimate the trend in fertility status of agricultural soils of India since 1967, when the Green Revolution was set in. Based on the soil test values for N, P and K, soil samples were classified into three categories, low, medium and high and nutrient index was calculated for soils of different states. It was estimated that in some states, West Bengal, Gujarat, Tamil Nadu, for example, nitrogen fertility has increased, while it has declining in the states of Orissa and Kerala. In the remaining states the nitrogen fertility status has remained almost same from 1967 to 1997. An increasing trend in P status is observed in case of Assam, Karnataka and Kerala. In the rest of the states it has remained unchanged. Potassium fertility has been medium in most of the states. Since 1977, K fertility status has either remained same or decreased. Therefore, contrary to the general perception, there has not been much depletion of soil fertility of agricultural soils of the country over the years.

SOIL is the critical component of the earth, functioning not only for the production of food, fodder and fiber but also in the maintenance of local, regional and global environmental quality. As the world soil resources are finite, intensive land use is inevitable to meet the global demands for food and fiber. The intensification of agriculture on existing arable land increases risks of soil and environmental degradation, which results in decline in soil fertility and productivity. The problem is aggravated due to the decrease in per capita arable land area. Descending soil quality thus has started raising the question of food security in the minds of agriculture scientists, technologists and policy makers.

Importance of soil in supplying plant nutrients is known since the beginning of agriculture. The concept was more convincingly advocated since Liebig's time around 1840, when many methods have been tried to get precise basis for estimating soil nutrient supplying capacity and predicting the fertilizer requirements of crops. Agriculture in South Asia, until the middle of the twentieth century, however, relied mostly on inherent nutrient reserve of the soil and organic manure. From mid-sixties use of fertilizers is increasing steadily along with the introduction and expansion of modern high yielding varieties accompanied by development of irrigation facilities in India. However, recent reports suggest a declining trend of rice and wheat yield in South Asia¹. Major causes which have been suggested include: gradual decline in the supply of soil nutrients causing nutrient (macro and micro) imbalances due to inappropriate fertilizer applications and decline in soil organic matter (SOM) content. With decline in yield the partial factor productivity from applied nutrients is going down resulting in increasing costs of production. This at the same time has associated environmental costs in terms of nutrient losses in gaseous form and leaching.

Ultimately, this affects sustainability of the system. Estimates say, that about 40% of world agricultural land share is being affected by human induced land degradation².

Global climate change, which has recently drawn a considerable attention, will further aggravate the problem. The soil system responds to the short-term events such as episodic infiltration of rainfall and also undergoes long-term changes such as physical and chemical weathering due to climatic change. The potential changes in the soil forming factors directly resulting from global change would be in the organic matter supply, temperature regimes, hydrology and changes in the potential evapotranspiration. The distribution of land resources will change from the present pattern on account of not only sea level rise but also due to high temperature and high evapotranspiration as schematized in Fig. 1. The most important process likely to be affected by the climatic change, particularly increased temperature, is the accelerated decomposition of organic matter, which releases the nutrients in short-run but may reduce the fertility on long term basis.

It is essential, therefore, that soil nutrient supplying capacity of soil be continuously monitored to ensure and improve sustainability. Notwithstanding the inherent drawbacks, the soil testing data are the best source to assess soil fertility under existing conditions. Now there are 533 static laboratories with at least one laboratory in each agriculturally important district and 108 mobile soil testing laboratories in the country. Each laboratory handles 2000 to 72,000 samples a year and provides fertilizer recommendations. The objective of this paper is to review and analyze the fertility status of Indian soils since 1967, when the Green Revolution was initiated.

Materials and methods

Based on the soil test values for different nutrients, soil samples are generally classified into three categories, low, medium and high (Table 1). In 1951, Parker³ and his co-workers put forth the concept of "soil nutrient index". Nutrient index is calculated giving different weightages to low, medium and high soil fertility classes as explained below:

$$\text{Nutrient Index} = \frac{(N_l * 1) + (N_m * 2) + (N_h * 3)}{N_t}$$

N_t

N_t = Total number of samples analyzed for a given area.

N_l = Number of samples falling in low nutrient status (Table 1).

N_m = Number of samples falling in medium nutrient status.

N_h = Number of samples falling in high nutrient status.

Analysis of these data over a period of years will provide the changing pattern of soil fertility down the years. The soil fertility status data of various districts of India, for 1967, 1977 and 1997 was taken from Ramamoorthy and Bajaj⁴, Ghosh and Hassan⁵ and Motsara⁶, respectively. Using the method, soil nutrient index of different districts was calculated for three different years: 1967, 1977 and 1997. From soil nutrient index of districts, nutrient index at the state level and then for the whole country was deduced for the three years.

Results and Discussion

Nitrogen fertility status

It was assessed that the nitrogen fertility status in the majority of the states is medium (Table 2). In the North Eastern states the fertility status is high. In West Bengal, the nitrogen fertility index has increased from 1.47 in 1967 to 1.67 in 1997. Similar increasing trend is observed in case of Gujarat (1.13 in 1967 – 1.71 in 1997), Tamil

Nadu (1.1 in 1967- 1.34 in 1997). But the trend is declining in the states of Orissa (1.92-1.57), Kerala (2.11-1.66). In the remaining states the nitrogen fertility status has remained almost same during the period with very minor variability. For the country as a whole the nitrogen fertility status has increased from 1.58- 1.79 during the last four decades.

Phosphorus fertility status

A perusal of data in Table 2 reveals that phosphorus fertility status has been medium in majority of the states. In the state of Haryana a decreasing trend (2.00-1.20) in the P fertility status was observed. It was opposite in case of Assam (1.00-2.13), Karnataka (1.06-1.90) and Kerala (1.11-2.35). The rest of the states did not show any distinct trend in the fertility status of phosphorus though there have been fluctuations in the fertility status over the period. For the whole country the phosphorus status has changed from 1.51 to 1.46 during the period of study.

Potassium fertility status

Potassium fertility has been medium in most of the states. A constant increase in potassium fertility status is observed in the states of Orissa (1.46- 1.93), Maharashtra (2.40-2.66) and Karnataka (2.00-2.54). A decreasing trend is found only in the state of Himachal Pradesh (2.00-1.63). In the remaining states there is fluctuation in the potassium fertility status but no specific trend is observed. In most of the states the potassium fertility has increased remarkably in 1977 from the 1967 level. But in 1997 the potassium fertility status has either remained same or deviated slightly from 1977 level. On all India basis the potassium fertility has increased from 2.00 in 1967 to 2.17 in 1997.

The findings of our study is supported by the results of LTFEs for rice-wheat cropping systems in northwest India, which showed more or less static organic carbon status for the last 25-30 years. But the organic C content of these regions are already low (0.3-0.5%). Whereas the regions with higher contents of organic C in the zone have shown a declining trend. Ram⁷ showed that continuous rice-wheat cultivation for 20 years resulted in decrease in organic C from 1.48% to 0.49% and 0.84% in unfertilized and recommended NPK treatment, respectively. Such sharp decreases are perhaps possible only when initial organic matter is very high such as in the case of Pantnagar⁸. In soils where SOC is low as in Delhi environments, there is no significant decrease, even in long-term⁹. Blank plot recorded less organic C content, which is somewhat similar to that of control plot. Since no crop was grown in this plot it had no crop residues or roots left in the field, which led to lower carbon content. In non-puddled soil with tillage treatment organic carbon was found to be maximum (0.76%) at 292 days after transplanting in FYM treatment. This coincides with the harvesting of the first wheat crop. In this soil organic C increased during the growth of the first wheat crop, then it slightly decreased and again it showed increasing trend during the second wheat crop. Similar trend was also observed in non-puddled no tilled soil. But in this case maximum value was 0.69% at 259 days after transplanting in both green manure and 100% organic source treatments. In non-puddled soils both in tillage and no tillage treatments organic carbon in 0-15 cm soil depth slightly increased than the initial value. Organic C content of 15-30 cm soil layer also showed decreasing trend in both puddled and non-puddled soil with tillage and no tillage treatments. Carter and Rennie¹⁰ observed no significant change in total organic C and N between zero and conventional (shallow) tillage systems of 2, 4, 12 and 16 years duration. However, concentration gradients of potential microbial biomass C and N and potential net mineralizable C and

N were significantly greater in the surface soil under zero tillage in comparison to conventional tillage.

From the above study it can be argued that there has not been much depletion of soil fertility over the years in India. Only soil phosphorus status has lowered, but soil nitrogen and potassium status has increased during the last four decades of intensive inorganic fertilizer use in India. Causes of such trend could be intensive cultivation, increasing growth rate of N, P and K use and appropriate technology adoption for minimal loss of nutrients. The role of climate variability on net organic C dynamics is not very clear and the direction of change to be debated on short and long term basis. Similarly, the role of climate change also needs to be studied.

References

1. Ladha J K, Dawe D, Pathak H, Padre A T, Yadav R L, Bijay-Singh, Yadvinder Singh, Singh Y, Singh P, Kundu A L, Sakal R, Ram N, Regmi A P, Gami S K, Bhandari A L, Amin R, Yadav C R, Bhattarai E M, Das S, Aggarwal H P, Gupta R K and Hobbs P R. 2003. *Field Crops Res.* **81**,159.
2. Oldeman L R, Hakkeling R T A and Sombroek W G. 1991. World map of the status of human-induced soil degradation (GLASOD). ISRIC, Wageningen, the Netherlands.
3. Parker et al. 1951. *Agronomy Journal* **48**,105.
4. Ramamoorthy B and Bajaj J C. 1969. *Fert. News* **14**(8),25.
5. Ghosh AB and Hasan R. 1980. *Fert. News* **25**(11),19.
6. Motsara M R. 2002. *Fert. News* **47**(8),15.

7. Ram N. 2000. *In* I P Abrol, K F Bronson, J M Duxbury and R K Gupta (eds) Long-term soil fertility experiments in rice-wheat cropping systems. RWC Research Series #6. New Delhi. p. 50-55.
8. Aggarwal P K, Bandyopadhyaya S K, Pathak H, Kalra N, Chander S and Kumar S. 2000. *Outlook Agric.* **29**(4), 259.
9. Rekhi R S, Benbi D K and Singh B. 2000. *In* I P Abrol, K F Bronson, J M Duxbury and R K Gupta (eds) Long-term soil fertility experiments in rice-wheat cropping systems. RWC Research Series #6. New Delhi. pp. 1-6.
10. Carter M R and Rennie D A. 1982. *Canadian J. Soil Sci.* **62**,587.

Table 1. Soil fertility ratings based on soil test values

Soil test	Low	Medium	High
Organic C (%)	<0.5	0.5-0.75	>0.75
Olsen P (kg ha ⁻¹)	<10	10-25	>25
Ammonium acetate extractable K (kg ha ⁻¹)	<108	108-280	>280

Table 2. Nutrient index over the years in different states of the country

State	Nitrogen			Phosphorus			Potassium		
	1967	1977	1997	1967	1977	1997	1967	1977	1997
Andhra Pradesh	1.55	1.38	1.55	1.50	1.19	1.57	2.30	2.54	2.52
Assam	2.00	2.00	1.76	1.00	2.11	2.13	1.33	2.20	2.14
Bihar	1.73	1.23	-	1.27	1.55	-	2.33	2.08	-
Gujarat	1.13	1.63	1.71	1.38	1.80	1.64	1.00	3.00	2.60
Haryana	1.00	1.00	1.04	2.00	1.83	1.20	2.00	2.80	2.05
Himachal Pradesh	2.00	2.25	2.34	2.00	2.00	1.93	2.00	1.95	1.63
Karnataka	2.33	1.53	2.05	1.06	1.23	1.90	2.00	2.21	2.54
Kerala	2.11	1.70	1.66	1.11	1.70	2.35	1.00	2.00	1.98
Madhya Pradesh	1.16	1.18	1.27	1.88	1.68	1.84	2.56	2.64	2.48
Maharashtra	1.57	1.36	1.40	1.38	1.40	1.16	2.40	2.52	2.66
Orissa	1.92	1.69	1.57	1.31	1.62	1.54	1.46	1.84	1.93
Punjab	1.89	1.00	1.67	2.33	1.68	1.93	2.00	2.42	2.40
Rajasthan	1.14	1.04	-	1.93	2.08	-	2.07	2.62	-
Tamil Nadu	1.10	1.08	1.34	1.60	1.40	2.11	2.00	2.54	2.40
Tripura	2.00	2.00	1.76	1.00	1.00	1.69	1.00	1.00	1.70
Uttar Pradesh	1.15	1.16	1.25	1.31	1.25	1.32	2.00	2.27	2.21
West Bengal	1.47	1.53	1.67	1.87	1.73	2.05	2.20	1.94	2.04
Country	1.59	1.47	1.67	1.50	1.41	1.46	2.00	2.07	2.17

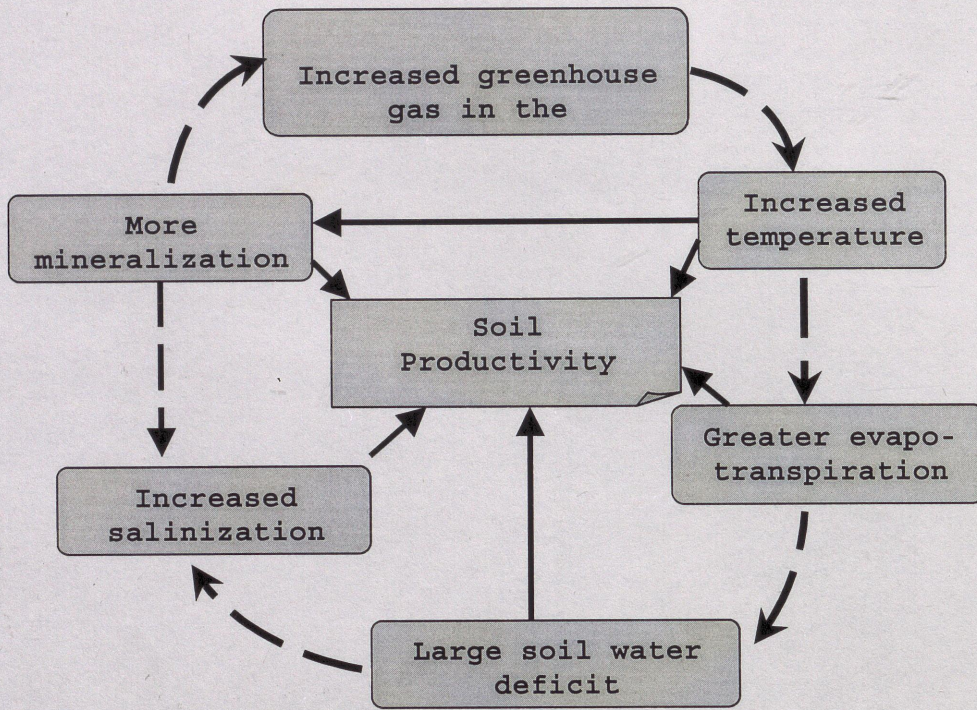


Figure 1. Impact on soil productivity due to global warming

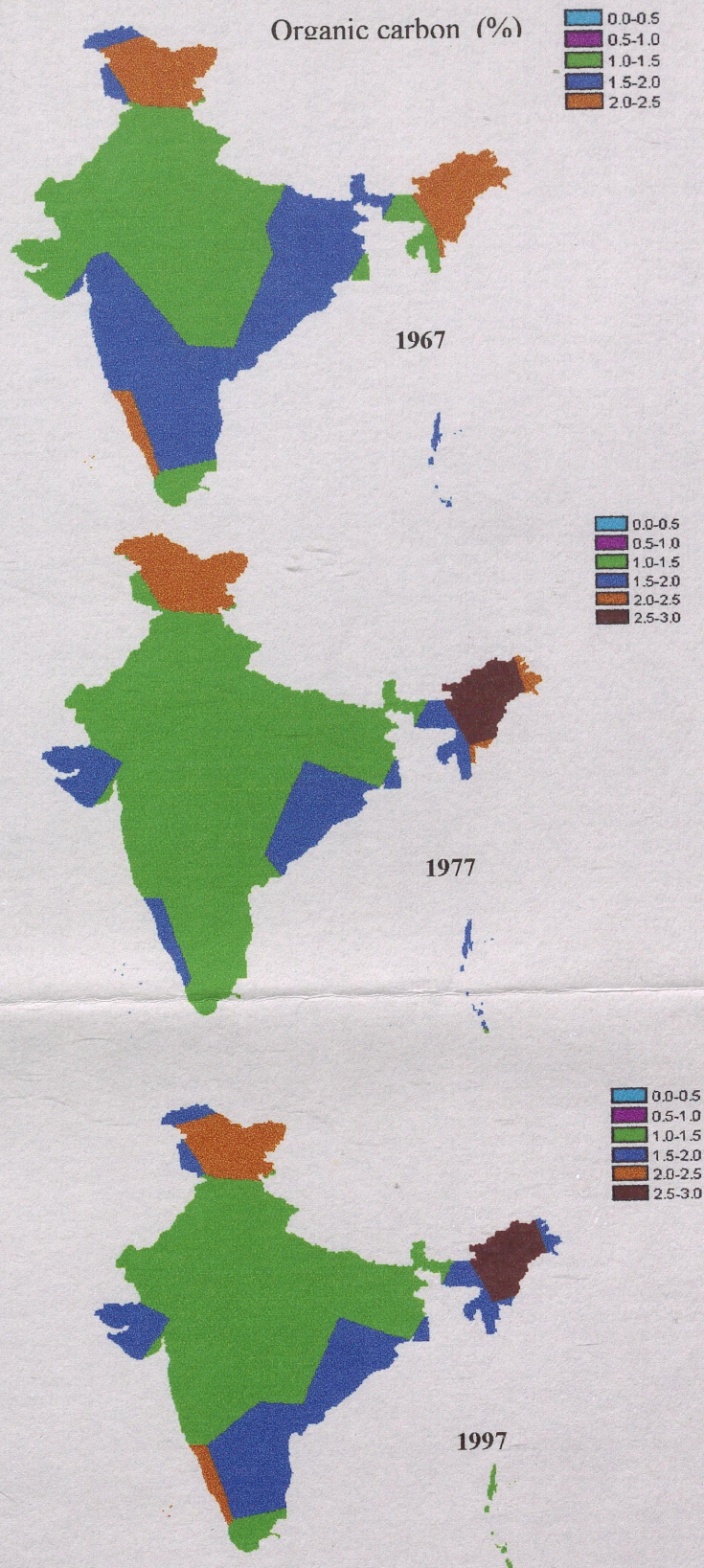


Fig 2 Temporal course of organic carbon status of Indian soils

→ Prof. N. V. Joshi

gayathri

From: "gayathri" <gayathri@nias.iisc.ernet.in>
To: <currsci@ias.ernet.in>
Cc: "N.V. Joshi" <nvjoshi@ces.iisc.ernet.in>; <gandhi@chemeng.iisc.ernet.in>
Sent: Wednesday, March 14, 2007 3:08 PM
Attach: Energy Savings and other benefits-revised.doc; figure 5.jpg; fig3.doc; figure 2.ppt; Figure 4 (a) and (b).ppt; fig1.doc
Subject: Current Science GA 1600

To:
The Editor
Current Science

Further to the teleconversation Gayathri had with Ms. Chandrika Ramesh, I am attaching revised version of the paper on Energy Savings and other benefits from advancing IST by half an hour (GA 1600) along with figures 1, 2, 3, 4(a) and (b) and (5). Please send this version to the reviewers for their review. I am also sending three hard copies of the paper through our representative today.

Could you please acknowledge receipt?

With kind regards,
Dilip Ahuja

— Original Message —

From: Dilip Ahuja
To: gayathri
Sent: Wednesday, March 14, 2007 2:24 PM
Subject: Revised Paper

3/14/2007

Date: Fri, 2 Jan 2009 16:06:11 +0530
From: ANIL <acanil@nio.org>
To: currsci@ias.ernet.in
Subject: submission to current science ravi naik et al

R10

Dear Editor,

We wish to submit a manuscript 'Skeletonema (Bacillariophyceae) in Indian waters: A reappraisal' for publication in Current Science under the research communications' section.

The names and complete addresses of all the authors are as follows: The attachment has a complete pdf file of the submission and the text also as word document.

Ravidas K. Naik

National Institute of Oceanography, Council of Scientific and Industrial Research, Dona Paula, Goa, 403 004, India (e-mail: rnaik@nio.org)

Dr. Diana Sarno

Stazione Zoologica Anton Dohrn, Villa Comunale, 80121, Naples, Italy
(e-mail: diana@szn.it)

Dr. Wiebe H. C. F. Kooistra

Stazione Zoologica Anton Dohrn, Villa Comunale, 80121, Naples, Italy
(e-mail: kooistra@szn.it)

Priya M. D'Costa

National Institute of Oceanography, Council of Scientific and Industrial Research, Dona Paula, Goa, 403 004, India (e-mail: priya@nio.org)

Dr. Arga Chandrashekar Anil

National Institute of Oceanography, Council of Scientific and Industrial Research, Dona Paula, Goa, 403 004, India (e-mail: acanil@nio.org; tel: 0832 2450404)

The names of potential reviewers which you may wish to consider

Dr. V. P. Venugopalan,

Scientific Officer G & Head BBPS

Associate Professor, Homi Bhabha National Institute

Biofouling and Biofilm Processes Section

(Chemistry Group, Bhabha Atomic Research Centre)

BARC Facilities, Kalpakkam, Tamil Nadu 603 102, INDIA

Email: vpv@igcar

Phone + 91 (0) 44 2748 0500 Ext 22607, Telefax + 91 (0) 44 2748 0097

and

Professor Dr. V N Raja Rao

Retired Professor of Botany,

University of Madras

634, 12th Cross 27th Main

HSR Layout 1st Sector

Bangalore 560102

Telephone 080-25721634

d:\currsci\acan.pdf
.. \acan.doc

Thanking you

Yours sincerely

Anil

Dr. A C Anil

Scientist

National Institute of Oceanography

Dona Paula

Goa 403 004

India

Tel: 91-832-2450404

Fax: 91-832-2450704

web: www.nio.org

[Part 2, Application/PDF 9.4MB.]

[Unable to print this part.]

[Part 3, Application/MSWORD 86KB.]

[Unable to print this part.]