r 14.11.90

Ivor Catt,
P.O. Box 99,
St. Albans AL3 4HQ
tel 0727 864257
4nov90

Hiram Caton,
Griffith University
Brisbane 4111
Australia
tel (07) 875 7538

Dear Hiram,

Suppression, Victimisation.

I have the urge to call for your attention again.
Very taxing personal problems cause me to delay the activity I promised to the group who met with you a month ago. I will do my secretarial job in a month or two from now.

My work on electromagnetic theory is broad and deep, and can be used as a tool to probe the Scientific Reception System many times and in many ways. I have made many discoveries and proposals, some of which do not need to mesh with others. An example is the assertion that a steady charged capacitor is not steady (quiescent) at all. I communicated this in Wireless World March 1983 (enclosed). This is new information which should not be controversial, but all the same it is ignored. It falls happily into the conventional paradigm for e-m theory, and yet is ignored, although obviously important. You could try it out on physics lecturers that you might know, and presumably they will refuse to make any written comment on it, even including the following;

1 The Catt March83 paper is trivial.....signed......
2 The Catt Mar83 paper is nonsense "
3 The do do unintelligible do do
4 The is important
5 is wrong
6 is right.........signed.......
7 is not new (see ref......).....signed....

What I am presumably asserting is that all (novel) comment on e-m theory (and comment on the comment) is taboo! This is a broader assertion than heretofore, and would merit testing in the field. I really do feel in my bones that discussion of e-m (except of how to teach it) is generally outlawed.
I can supply further test probes on request, to someone who has used this one.

Mahoney, Cecci et al have conducted experiments with the Scientific Reception System, and Caton has reported their results leading to certain conclusions as to the nature of the defence mechanism(s) set up by a decadent (?mature?) body of knowledge in defence against new knowledge. I pointed out to Caton and he agreed that each sub-discipline of mature (ossified) knowledge, in order to survive, could be expected to cleave to one only defence mechanism; one only being necessary and sufficient. We notice that the mechanism found by Mahoney could be said to be different from the one found by Cecci. In my fields of work, I seem to have found other defence mechanisms. In particular, I find that it is

intelligible content

if new that triggers suppression. I cite examples of unintelligible content which found publication from my own writings;

1

My first ever paper in the IEEE Trans EC, ?Feb1966?, "Time loss through.......". This contained unacceptable information because new, but was shielded (gelded) by an unintelligible title.

2

A very important, totally revolutionary communication on Maxwell's Equations, IEEE Trans on Computers, March 1977 p318. This was published in spite of content because of unintelligibility.

Note that conventional mythology would have it that one necessary criterion for publication through the referee system is intelligibility. Here we see that if the information is new, the reverse is true. More generally, we can argue that unintelligibility will expedite publication, since non-communiction threatens no interests vested in the archaic knowledge base.

This discovery cuts across the Cecci/Mahoney discoveries that status of source organisation, conformism of conclusions etc control acceptance/rejection. Apart from my earlier assertion that only one evolved defence mechanism is necessary in each discipline, I might here argue that they are dealing with softer science (say biolgy, not the brittle physics). I expect the cutting edge of weaponry to be sharper in the brittle end of science, and the paranoid fear of new information (justly) greater. (At the other extreme, the very subject is the collision between conflicting views, as opposed to the evolved (reprehensible) monolithic nature of hard/brittle science.)

To repeat; in the pure air of brittle/hard high physics, suppression attaches to contemt pure and simple, rather than the clutter of institutional origin or conclusions etc etc, which latter mechanisms arise in more soggy fields of endeavour.

Yours sincerely,

Ivor Catt

NB BM

Footnote. Brian Martin's C.V. (back cover of his "The Bias of Science") maps exactly onto that of a very able man I have met recently, Sam Falle. Sam Falle should write to Martin outlining Sam's interests and asking for Martin's publications, in the first instance.

Sam's and Martin's philosophies will enrich each other.

cc Brian Martin, University of Wollongong, PO Box 1144, Wollongong, NSW 2500, Australia. tel 042 270691

cc Sam Falle, Mantis Numetrics, 46 The Calls, Leeds LS2 7EY. tel 0532 448200 pm 5.10.90 r 6.12.90

Recent writing on this file number sequence (e.g. 8cBMart) would appear to be criticism of Brian Martin's work. On the contrary, I have great admiration for his work and merely use his very good work to illustrate certain fundamental dilemmas, which seem to show up even in work of his exceptional calibre.

Ivor Catt,
P O Box 99,
St. Albans AL3 4HQ
tel 0727 864257
5oct90

Brian Martin,
Science and Technology Studies Dept.,
University of Wollongong
P O Box 1144, Wollongong,
NSW, Australia.
(tel 042 270691)

Dear Brian Martin,

I have just read with admiration your article THE SELECTIVE USEFULNESS OF GAME THEORY, Social Studies of Science (SAGE), vol 8 (1978), 85-110. The same concern that I have voiced before arises again.

This concern arose first when I read Polanyi, PERSONAL KNOWLEDGE. He argued that pure science should have no purpose, or it would be lost. What he said was true, but only in a limited context - the context where Marxists were limiting science to that which had immediate application of value to THE PEOPLE. We now see that Marxism was a temporary phase, and should not have been allowed to pollute Philosophy of Science.

Similarly, your admirable writings, on more than one occasion, are locked into a relatively short term aberrant environment, this framework remaining unstated by you.

It is not possible by induction to prove that, since you have shown that even some of the most pure scientific disciplines are inherently value laden, it follows that all science is value laden. Such an assertion is both false and very damaging. It allows the permanent intrusion of value laden, special interest science, and the suppression of true science, on the basis of the argument that since all science is value laden and subjective, then one block of science is no better than another, so innovative proposals by, for instance, Catt, can be suppressed and ignored with impunity. The argument that all science is subjective, value laden, is one cornerstone of the INSTRUMENTALIST creed (see K Popper, CONJECTURES AND REFUTATIONS, p100). As a result, you work bids fair to be one of the main elements in the current blockage of any progress in science. Further, the better your work, the more effective it will be in helping to block progress.

Yours sincerely,

Ivor Catt

Ivor Catt 4Oct90

Politics control Physics.

The Three Cases.

1

Theocharis cites to me two independent publications which say that the first case when Ati-Semitism was linked with anti-Relativity was when Relativists claimed that anti-relativism was driven by anti-Semitism.

(The tragic implication is that the collapse of the Nazi empire caused us to be saddled with Relativity.)

2

Theocharis says that the reigning Physicists in the USSR were anti-Realitivity. Then almost overnight in around 1936, the reigning physicists disappeared, and the new set of Establishment Physicists were pro-Relativity. Theo says there is very good evidence that Stalin himself had a change of heart, and enforced it rapidly and efficiently.

3

B Martin cites Forman as saying that the anti-rationalistic element in the culture of the Weimar Republic caused German Physicists to embrace Quantum Theory (note 1).

For me, Relativity and Quantum Theory are two elements in the dog's breakfast which is called Modern Physics; a bastard pseudo-science (note 2).

The parallel between the development of scientific dogma in response to political pressure closely mirrors the development of the Nicene Creed as an aid to the political survival of the Emperor Constantine. (I strongly object to the imposition upon today's majority religion of components which relate to Constantine's political stratagems and have virtually no connection with Jesus, his life and teachings.)

Notes.

1
Forman, P.(1971) Weimar culture, causality, and quantum theory, 1918-1927: adaptation of German physicists and mathematicians to a hostile intellectual environment, Historical Studies in the Physical Sciences, 3, 1-115.

2

Catt, I., Betrayal of Science ...., Electronics and Wireless World, July 1987, p683.

4oct90 bis.

Ivor Catt expects to leave tel 0727 864257 for a period, although he may retrieve that number later on.

From 9oct90 Ivor Catt should be temporarily at 0923 55290.

Arthur Turp, Royal Mail, assures me that my P.O. Box 99, St. Albans AL3 4HQ, England will remain secure. However, at present top priority letters should be sent in duplicate, a second copy to c/o Stevens, 10 Harrisons Green, Birmingham B15 3LH.

For up to date information on how to contact Ivor Catt, please call one of the following numbers;
021 454 3089, 081 960 2040, 081 337 2980, 0923 224251.

Gwen and Ralph Stevens, 10 Harrisons Green, Edgbaston, Birmingham B15 3LH, tel 021 454 3089
Chris and Mary Penfold, 38 Kelfield Gdns, London W10. tel 081 960 2040
P D E Long, Porter & Co., 555A London Rd., North Cheam, Surrey SM3 9AE, tel 081 337 2980.
Sue Warman, 0923 224251

25.9.90 3.10.90

Recent writing on this file number sequence (e.g. 8cBMart) would appear to be criticism of Brian Martin's work. On the contrary, I have great admiration for his work and merely use his very good work to illustrate certain fundamental dilemmas, which seem to show up even in work of his exceptional calibre.

Ivor Catt,
P O Box 99,
St. Albans AL3 4HQ
tel 0727 864257
24sep90

Brian Martin,
Science and Technology Studies Dept.,
University of Wollongong
P O Box 1144, Wollongong,
NSW, Australia.
(tel 042 270691)

Dear Brian Martin,

Intellectual Suppression

Thank you for your 17sep90 letter, and thank you very much for all the material you enclosed.
I have just read with admiration your MATHEMATICS AND SOCIAL INTERESTS, pub Search July88. You have covered a massive area in your research.
Whig History is as you defined it. It appears in certain dictionaries of terms. I first heard the term two years ago.

I think I discern a fundamental dilemma, which has to be resolved if our bodies of knowledge are to go forward and not decay. I shall entitle it Multiple Whig History.

Multiple Whig History.

1
We operate within a interleaved mesh of disciplines which include science, philosophy of science, sociology of science, history of science, etc. When discussing one of these, we take the others as given. What is given is the status quo, essentially a "Whig History" status quo. The alternative, that there should exist multiples of each discipline, each based on either orthodoxy or a particular dissident sect within other of the disciplines, is impracticable. Thus, Sociology of Science will proceed on the assumption that a group of imposters has not captured the Halls of Science and driven out true science. Sociology of Science, when drawing on Science, will draw from Imperial College London, the Royal Society and so forth, regardless of whether one or all of those institutions have been

captured by the Vikings shortly before. Sociology of Science will happily build its theories on the babblings of the Vikings.

2

Traitor groups within each discipline will seek validation for their false body of knowledge by citing what they can from the other disciplines. Thus, "Modern Physics" usurpers of Science will misinterpret Kuhn, taking his pejorative descriptins of malpractice in science as merely descriptive or even prescriptive. See for instance Electronics and Wireless World jan88 p48, where Catt discusses J.W.'s (fortunately blatant) falsification of Kuhn's message. (Also, Kuhn himself nowadays betrays himself as he travels the world like a castrato singing for his supper. (Also see note 1, re you on Kuhn in Math and Soc Int, p209)

It follows that, to the extent that all of these disciplnes are in crisis, struggling to survive dilution and distortion by (?professional?) vested interest groups, it is important that those who write in these fields try to minimise possible ambiguity in interpretation of their writings. However, this means that they must include what appear to be clear value judgements, whereas other pressure (towards the appearance of objectivity) drive writers in the opposite direction.

(Tangentially relevant is the idea that when teaching, one goes from the kown to the unknown.) If (as I believe is the case) the centrepiece, Science, of these disciplines, has been captured by the vandals, and retained by them for a long time (1927-1990), it is possible that the knock-on damage to the other disciplines, reflecting back into more damage to science itself, and so on round the circle, leaves the whole matrix of disciplines damaged beyond repair. Put another way, why should Sociology of Science, or Philosophy of Science, survive the eclipse of their host discipline, science, for 63 years?

Notes

1

At least in his 1969 Postscript in The (1962) Structure of Scientific Revolutions, Kuhn should have mentioned the previous exposition of the Paradigm by Polanyi in Personal Knowledge p151, published 1958 - admittedly probably too late for mention in the original 1962 Kuhn.

Through a copy of this letter, I urge Theocharis to read B Martin, Mathematics and Social Interests, pub Search july88, p210, where Martin discusses pressure by Weimar culture towards the renunciation of causality in Modern Physics. Could Martin send him a copy? Theo is expert in that general area.

re the same Martin article, Catt has a series of articles culminating in Wireless World jan86, where he asserts that most maths is just plain false. This clashes with the Martin argument that maths is culture dependent. The Martin thesis will tend to obscure the charge that much knowledge is false; objectively false. We can't have it both ways. Does Martin realise that in arguing that knowledge is culture dependent, he is helping the vandals to hold on, however nonsensical their jabberings? Is it necessary that attempts to deliver value-free information will lead to it being eminently exploitable by vandals? Do we all have a duty to consider the political implications of our delivering information?

Yours sincerely,

Ivor Catt

r19.9.90

Ivor Catt,
P O Box 99,
St. Albans AL3 4HQ
England.
8sep90

Recent writing on this file number sequence (e.g. 8cBMart) would appear to be criticism of Brian Martin's work. On the contrary, I have great admiration for his work and merely use his very good work to illustrate certain fundamental dilemmas, which seem to show up even in work of his exceptional calibre.

The Tail wags the Dog.

Standing behind science (note 1) are a number of disciplines; Philosophy of Science, Sociology of Science and History of Science. Let us call them Background Subjects (BS). I have come to feel that the relationship between these other disciplines and science is unsound, to such a degree that it vitiates both BS and science (note 2). So long as the citadel was not under attack by the Vandals, that is, before the attack on science by Modern Physics began, these structural flaws did not come to the surface. But at that time, because no problem arose, there was also perhaps no point in having BS. (The only purpose of BS is to regulate science. However, today, after the sacking of science by an unscientific Modern Physics, the defects are apparent, and it is also clear that because of the structural defects, no BS could contribute to the defence of science, when defence became necessary.

Many years ago I said that a theory moulds its environment to suit itself; that that is the sole purpose of a theory. Similarly, each BS its environment. History, being Whig History, moulds its environment so as to fully validate contemporary science. If the citadel of science has been captured and sacked, then Whig History will by definition join forces with the vandals. Brian Martin explicitly states that sociology of science should map onto current science, which must imply that loss of science to the vandals inexorably means loss of sociology of science as a useful discipline, unless there exists another purpose than the regulation of science.

It has not been explicitly stated that Philosophy of Science should accept contemporary practice in science. (That is, there is not an apologist Philosopher of Science similar to Brian Martin's role as apologist Sociologist of Science.) Indeed, the philosophers I admire; Polanyi, Popper, early Kuhn; are able and willing to oppose aspects of contemporary fashion in science.

The Dilemma.

Notes

1

In this context, we concentrate on the most brittle science, Physics, and steer away from the softer Biology etc. This is justifiable because it is the brittle sciences which have most need of sound foundations. Furthermore, if the brittle, flagship sections of science are lost to the vandals, the rest follow. Similarly, the loss of a softer subject to the vandals would not significantly threaten the core subjects, and so science would more easily recover its lost colonial territory. However, loss of the Capital, Physics and the like, is probably irredeemable.

2

If the argument that the very purpose of the background disciplines is to bolster science itself, then if they fail to come to the aid of science when needed, it follows that the whole of such disciplines are futile, pointless.

A reappraisal of B Martin.

A re-reading of Martin might lead to the opposite impression to what I gained before. There are ample quotes which show that he might understand the issue which preoccupies me and also be sympathetic to my point of view on it.

> My view is that the presuppostitions underlying science should be brought out into the open. My hope is that many of them will be challenged and replaced. Perhaps the result will be a science so different from the present one that it should be called by a different name. - B Martin, The Bias of Science, pub. SSRS(ACT) 1979, p6.

It might be possible to insert the dualism of pre-1927 science and Modern Physics into the structure of this quote. I need response from BM to the point I have raised, about the 1927 watershed in Physics.

Towards that end, I point to my article "The Conquest of Truth, Electronics and Wireless World, Jan88.

SSS 78
Search 88

17 September 1990

Dear Ivor,

Thank you for your letter of 27 August and the additional articles and letters sent about the same time. I appreciate being able to see more of your work and reading your comments. While there is much with which I agree, let me focus here on some points of difference concerning *The Bias of Science*.

By picking a couple of quotes from *The Bias of Science*, I think you have missed its primary thrust. Actually, there is much in it to support your view about the consequences of the professionalisation of science.

I don't accept your view that my focus is on what you call "modern physics". For example, when I discuss quantum theory (p. 75, references on p. 81), I discuss a range of underlying assumptions, not just those of the Copenhagen interpretation.

I accept your assessment of the importance of engineering and engineers in the development of scientific knowledge: see p. 65.

You call *The Bias of Science* "Whig history". I understand this to mean a history that describes the past in terms of current concepts and perspectives, assuming that current views are (more) correct and that previous views are wrong and hence must be explained. Thus, a "Whig history" of science is history as told by the "winners" in science, namely those who have become the current scientific establishment. I don't see *The Bias of Science* as fitting easily into this category.

My reference to "modern science" is meant to distinguish it from ancient and medieval science, from "science" in non-European cultures, and from social science. Contrary to your assessment of my analysis, I see my focus as the science mainly developed since World War Two, namely science that is predominantly funded by the state and corporations and that is highly professionalised. My concern is much more with the political economy of science than with the philosophy of science.

A couple of articles relevant to mathematics are enclosed.

Yours,

Brian Martin

pm 29.8.90 r 4.9.90

Ivor Catt,
P O Box 99,
St. Albans AL3 4HQ
tel 0727 864257
27aug90

Brian Martin,
Science and Technology Studies Dept.,
University of Wollongong
P O Box 1144, Wollongong,
NSW, Australia.
(tel 042 270691)

Dear Brian Martin,

Intellectual Suppression

Thank you for your 18aug90 letter, and thank you very much for all the material you enclosed.

Thank you for the advice on approaches to studying the scientific reception system by tailoring articles submitted, a la Mahoney; what Mahoney would not do for me. I look forward to further advice from you.

Your work is an excellent example of Whig History, and illustrates what flows from it.

> 1
>
> I refer only to 'modern' science, the science that has developed mainly in Western Europe since the 1600's. Also when I speak of science, usually I am thinking of physical, natural, or 'hard' science. - B. Martin, The Bias of Science, pub. SSRS 1979, p7.
>
> 2
>
> Since my aim is to present a perspective for understanding science, I have not mentioned or treated the many alternative interpretations to mine. In my opinion, a large majority of them are merely convenient justifications for the current state of affairs. In most cases I disagree with the fundamental assumptions on which these interpretations (or justifications) are based. My aim is more to expose the assumptions and their implications than it is to answer every objection . - ibid p6.

Quote (1) says that you centre your analysis on Modern Physics, the body of knowledge which was formally set up at the Solvay Brussels conference in 1927 (in spite of numerous, permanent objections from Einstein). Quote (1) also either implicitly asserts (a) that science did not change direction in 1927, or (b) that you centre your analysis on the period from 1927 to today, not 1600 to today.

I have read all the material you sent to me, and find no acknowledgement that it is asserted that science changed direction in 1927 in a manner central to your field of research. At the least, you should mention this general assertion (by both contending groups in today's science, the conservatives and the Modern Physics party) in order to dismiss it.

Alternatively, you must admit that you are writing Whig History. However, this assertion would contradict the counter-assertion "He has long been active in the environmental, peace and radical science movements, and has ..... strategies for social movements." - B Martin, Discussion Paper, Analysing ... Fluoridation, pub. SAGE, 1988, p363, which means that your intention is to reform rather than justify the contemporary situation.

My position is that your two quotes should be re-written as follows;

> 1
> I refer only to 'Modern Physics', the science that has developed mainly in Western Europe since 1927. Also when I speak of science, usually I am thinking of the softer Physics that Modern Physics represents. - B. Martin, The Bias of Science, pub. SSRS 1979, p7, amended by I.C.
>
> 2
> Since my aim is to present a perspective for understanding Modern Physics, I have not mentioned or treated the perspective on pre-1927 science. In my opinion, such analysis is merely a convenient justification for the then state of affairs in science, before the triumph of the new ambience generally described as 'Modern Physics'. In most cases I disagree with the fundamental assumptions on which the interpretation (or justification) of pre-1927 science is based. My aim is more to hide the assumptions of Modern Physics and their implications than it is to answer every objection. - ibid p6., amended by I.C.

You do not reference Popper, who (in Conjectures and Refutations p100) discusses the problem that you do not acklowledge. You quote Polanyi, Personal Knowledge. As I recollect, that brilliant man, my favorite Philosopher of Science, was surprisingly obtuse on this issue (but see note 1). However, everyone in the field accepts that it is wrong to confuse the two kinds of Physics, or Science. To do so would render meaningless so much of the literature, for instance page 80 in "Physics and Beyond" by W Heisenberg and all the material in note 1. All of the gang who captured Physics from us scientists in 1927 knew very well what they were doing, and always said so.

A social reformer should help to retrieve science from unscientific usurpers in the same way as he seeks to retrieve the land for the people and away from absent foreign landlords. You should not validate the behaviour of the usurpers by promulgating Whig History.

Yours sincerely,

Ivor Catt

Notes.

1
Polanyi was concerned that Marxist countries wanted science to be practical, and so argued for the unpractical. This caused him to be muddled on the question of the concreteness of fundamental science, feeling that the anti-communists needed to be airy-fairy as an antidote to communism. Those were dangerous times for the world, and as a result, even Philosophy of Science was muddied by a great man like Polanyi, on this particular issue.

2
Discussions of the revolutionary nature of Modern Physics. (The authors

below would call it counter-revolutionary, a latter day victory over science.)

T Theocharis, Where Science has gone wrong, Nature, vol 329, no. 6140, p595, 15oct87.

I Catt, Betrayal of Science by Modern Physics, July87, p683.

I Catt, The Conquest of Thought, Electronics and Wireless World, Dec87, p1250.

K R Popper, The Science of Galileo and its new betrayal, Conjectures and Refutations, 1963, p97.

M Polanyi, Personal Knowledge, 1958, p147.

Sun-Herald, 19 August 1990 on Noel Balzer
Bias
SPP86
SPP88
Age MR 89
SSS 88

SCIENCE AND TECHNOLOGY STUDIES
Department

STS
Science
and Technology
in their
Social Context

University of Wollongong
PO Box 1144, Wollongong, NSW 2500, Australia.
Telephone: 042-270691 Telegram: UNIOFWOL Telex: 29022

18 August 1990

Dear Ivor,

Many thanks for your letter of 2 June and the accompanying material. It all arrived about 10 days ago (having come surface mail), and I have now had time to digest better what you have to say.

What you call politics of knowledge is certainly one aspect of my interests. The title of our book was Intellectual Suppression on the insistence of the publisher; our original title was Academic suppression -- and of course titles commonly are designed to attract attention more than to communicate exactly what is involved. Anyway, in my field (which can best be described as social studies of science and technology) there is quite a lot of study of the politics of knowledge, both when the knowledge is internal to a scientific community and when it becomes subject to vociferous public debate. Cases of the latter kind have attracted my attention because of the more complex dynamics involved.

I certainly sympathise with your eagerness to study the scientific reception system. My own experiences have been salutory. In one case, an article of mine (about the principles raised by a particular case of alleged plagiarism) was rejected by 9 scholarly journals in the US, UK and Australia without receiving a single referee's report. This was not one of my more radical papers -- the reason (in my assessment) was that plagiarism is too hot a topic for most scholarly journals. A much revised version was published by the tenth journal. Meanwhile, Broad and Wade obtained wide publicity for their book Betrayers of the truth. They are good journalists and, by publishing in book form, did not have to get past journal editors.

In another case, an article of mine -- dealing with politics after a nuclear war -- was rejected by 6 left-wing journals. It was then accepted by a libertarian journal. The reason, I think, is obvious: Marxists do not like criticism of state power; libertarians do. But the case is complicated by the changes that I made in the article along the way (a period of 7 years).

I think that studying the scientific reception system is highly valuable but it is also extremely difficult from the outside because of the many variables that are uncontrolled. Editors are replaced; some editors are more principled than others; referees vary enormously; fashions come and go; styles of journals vary; factors of author prestige, institutional affiliation, personal connections and deadlines all come into play. I'm sure there is a large chance factor, which includes things genuinely getting lost as well as things that are "lost on purpose". There's lots of noise in the system. Another complication is ethics procedures and committees, which tend to inhibit research.

In your case, it is quite possible that the same individuals advise different journals that give rejections. A national network can be quite effective, and so publishing in other countries is sometimes a successful strategy.

I can suggest a couple of experiments that you might try.

* Test for style of presentation. Write up your usual paper, and submit it. Then write up the same material in a cautious, careful fashion in the jargon and style of the same journal (obtaining the help of another scientist would be immensely helpful in this). The key content should be the same, but the dressing should make the articles seem quite different.

The main difficulty is identifying the author. You could use (different) false names in both cases. Institutional addresses would help make the whole thing more convincing, but institutions are likely to look unfavourably on the experiment.

* Test for different national responses. Send the same paper or papers to similar journals in several different countries -- perhaps UK, US, Germany, France, New Zealand, India. The hypothesis would be that success would be more likely in countries of lower perceived national scientific status (New Zealand, India) and in countries where you have not encountered difficulties before (Germany, France). The main difficulty here is determining which journals from different countries are "similar".

* Publish (or republish) your key insights in book form, and then send review copies of the book to a range of journals of different styles, countries, etc. This provides a way of running a variety of experiments without the ethical difficulties of simultaneous multiple submissions, acceptances that terminate a comparison process, etc.

That's all I can think of for the moment. Let me know what you think.

A few other comments arise out of your letter and the other things you sent.

Your idea of a communication net (described in "The rise and fall of bodies of knowledge") is excellent, in my opinion. A similar idea was described later by David Andrews in The IRG Solution (1984). Actually, I'm more interested in learning about and promoting alternatives to the present communications systems (scientific and otherwise) than just learning the deficiencies of the present arrangements.

You mention in your letter that someone has probably discovered a cure for AIDS. Recently I corresponded with someone who has proposed an explanation for the development of AIDS, and who has encountered severe problems in obtaining publication: (I've passed my file on this to someone else and will have to give the name and address at a later stage.)

I enclose a few other articles on related topics. I will send copies of Intellectual Suppression to Theocharis and the Burnetts, whose addresses you gave me.

Yours,

Brian

Brian Martin

pn 4.8.90 r9.8.90

Ivor Catt,
P O Box 99,
St. Albans AL3 4HQ
tel 0727 64257
(late 1990, 864257)
2jun90

Brian Martin,
Science and Technology Studies Dept.,
University of Wollongong
P O Box 1144, Wollongong,
NSW, Australia.
(tel 042 270691)

Dear Brian Martin,

Thank you so much for your gift of your book INTELLECTUAL SUPPRESSION pub 1986 Angus & Robertson NSW and London.

Harold Hillman had lent me his copy, which I still have, and I read through it perhaps nine months ago. It covers a very wide range, and I congratulate you on the great effort it must have involved from many people.

In response to your question, I think it would be well worth while to send a copy to my impoverished colleague Theocharis (he published in NATURE some 2 years ago, "Where Science has gone wrong" or some such), 200a Merton Rd., London SW18. I may append further names for copies at the end of this letter.

I have just re-read some of your book, and read some of the introduction. As Harold Hillmn says, a demarkation question arises. Hillman describes your field as "the trades union problem", for want of a better description. Following your introduction, I will call your field Intellectual Suppression, IS. Now my field, and also Caton's - who suggested you send me your book - is a different though allied one. I call my field the Politics of Knowledge, PoK. Whereas IS is an external battle, PoK is a palace battle. My field, PoK, also covers the Sociology of Science, the Politics of Science, etc.

I can show you how our interests diverge by citing my own experience of IS. While working on four massive Defence projects I became convinced that none of them could ever work. I approached the Attorney General, Defence Secretary and so on with the question; "If a technocrat working on a major defence weapon development project becomes convinced that there is no possibility that a viable weapon will result, what action can he take?" The Attorney General arranged a meeting between me and the three most senior rogues in the MoD in Whitehall who were paying taxpayers' money into these slush funds (£1 billions for each weapon project). I was still employed on these projects. If I were then fired and blacklisted, as I probably was, my case would come into your field, IS. (I have published about these rackets.)

An example of an item on the margin between IS and PoK was when in 1966 Dr Jan Narud, Fellow of the IEEE and Head of Integrated Circuit R&D at Motorola where I worked told me he would fire me unless I published in the IEEE Transactions under my own name material of which I did not approve. The IEEE told me it was a company matter, which it was not. (I mention this in my book COMPUTER WORSHIP.)

Now I am not anxious to work too hard in IS because I feel any effort I make available should go into my and Hiram Caton's field, PoK, rather than into IS. Whereas IS merely deals with racketeering in science and bullying those who threaten the financial rackets, PoK involves processes where scientists themselves go for the jugular of science and turn science itself into a racket, by subverting philosophy of science and so forth, and by blocking advances in science. PoK is outlined on p106 et seqq of THE ART OF SCIENTIFIC INVESTIGATION by W I B Beveridge pub Heinemann 1950, Mercury 1961. PoK dogs the careers of Harold Hillman and Catt, and many others. My enemies in PoK might be my allies in IS. Generally, in PoK, the whole of scientific academia is the enemy, subverting science in order to safeguard their careers by freeqing the body of knowledge. (See my letter in Wireless World, july87 p683.) Another description of PoK is in my article THE RISE AND FALL OF BODIES OF KNOWLEDGE, Journal of Information Science 2 (1980). Another treatment is in Catons's two excellent articles in SEARCH, sep 88, TRUTH MANAGEMENT IN THE SCIENCES, and Jan 89, PRODUCT CONTROL IN THE TRUTH INDUSTRY.

Generally, PoK relates to a deeper, more philosophical level than IS. Whereas IS gives short term, local advantage to self seeking groups, PoK undermines the whole of science for the future, by attacking its roots. IS only attacks aspects of journeyman science, but PoK can kill the whole of future science by destroying its foundations.

The situation is confusing because parts of the intro to your book could refer to both IS and PoK, but other parts refer only to IS. It is further confusing because you are linked with Caton, who deals with PoK while you yourself cover IS.

> Your book, p2. We are concerned with the cases in which the suppression is entirely or in part response to the expression of intellectual dissent, and in which other explanations for the suppression do not stand up to scrutiny.
>
> - this could be applied to PoK as well as to IS, but only as a result of an accident. You meant to refer only to IS.
>
> p3. Suppression is a general term, and both censorship and discrimination can be considered as types of suppression. In this book the unqualified use of the term suppression will refer to suppression of intellectual dissent.
>
> - this creates a problem, because PoK is about suppression of intellectual dissent, but it is not a subset of IS.
>
> p3. Intellectual dissent usually means dissent from the established policies or practices of elite groups.
>
> - perhaps here we can see the difference. PoK is not about policies or practices. PoK relates to dissent from the theories of elite groups. Does this mean you rule out PoK from your book?
>
> p3. The most effective way for these groups to maintain their privileged and powerful positions....
>
> - the trouble is, this accurately describes PoK, as well as IS.

While writing this, I have started to think that maybe we have a continuum with you at the left with IS, H Caton in the middle and myself at the right end with my general area, PoK. As one moves right, the whole battle tends more to be between groups of scientists and with no outsiders involved.

Next I include a copy of my 21jul89 letter to Mahoney. He replied that he could do nothing to guide me on politicking with learned institutions and journals. However, he misunderstood me, thinking that I wanted help in publishing. In fact, I wanted expert advice from ?sociologists? before I embarked on my jousts, so as to gain the maximum insight from the experience of being suppressed. I am in the special position of having major disclosures - in the class of Double Helix or Newton's Laws - which are the situation when the the performance of the suppression mechanisms are most significant; more than when they suppress the invention of a new matchbox. Is no one interested in helping to plan the key experiments on

Ivor Catt,P.O.Box99,
St. Albans AL3 4HQ,England
21jul89

M J Mahoney,
ED 1090
Univ. of Calif.,Santa Barbara,
Ca. 93106,USA

Dear Mr. Mahoney,

Thank you so much for your papers, just arrived. I am impressed by their scholarly nature, the range of research you have done, and the quality of the analysis and conclusions. I had never read anything of yours.

I cannot find the letter I wrote to you. It appears that I misnamed the MacRoberts bis article "The Scientific Reception System", whereas it should be called "The Scientific Referee System". Or are you asking for my article, "The Scientific Reception System as a servo-mechanism"? I'll send you both.

My article "The conquest of Thought" was badly edited, and the references were muddled up. The three graphs/diagrams from MacRoberts should have been included. Reduced to one graph only, the point is lost. I have written a great deal on a number of subjects in Wireless World during the years 1978-1989.

A good summary of "The Conquest of Thought" is that the establishment, in response to a new theory, repeats the old theory louder.

Theocharis added a second name P..... to his article published in Nature "Where science has gone wrong", 15oct87, vol329, no.6140, pp595-598. He knew that to get published, he had to use P's address, the prestigeous Imperial College. As a result, Bologna Univ., impressed by the article, invited P as well as T to give a paper at their 900th anniversary. So the politics of editing and refereeing has led to falsification of names of authors. Theocharis is at 200a Merton Rd., London SW18, tel 01 870 6191.

The letter of acceptance by Maddox to Theo of his paper, the 15oct87 one, was effusive; the most positive letter of acceptance that I have ever read. Theo says this is because it attacks the "Philosophy of Science" community. Maddox identifies with the "Science" community. So his response to Theo's next paper, a year or two later, which attacked the "Science" community, was a curt rejection. M used the argument of "timeliness", and in no way criticised the content of the second article. Theo's first paper and rejected second paper were merely used as material for the Science v Philosophy of Science palace battle. (This is Theo's analysis, not mine. The two letters from Maddox to Theo are very interesting, towards judging this analysis.)

Lovelock (author of the idea of Gaia) says frequently on British radio that in order to get published in Nature, he had to take an unpaid professorship at Reading Univ. so that he could use their address. He says that Nature reject any article from a private address.

Recently an article in the London Daily Telegraph discussed an article in Nature by two ?biologists? complaining that today, referees prevented any new theory from being published in ?biology? They said that Watson&Crick's DNA discovery, or similarly revolutionary ideas, could not be published today. The Telegraph also referred to Maddox, editor of Nature (and in fact the worst suppressor of the lot!) complaining about the present reality re pblication of ?biology? papers - that referees would only allow publication of data, but always rejected publication of new theories.

Possibly Maddox is pivotal in an analysis in this field. The obvious suggestion is that, editing the most prestigeous journal, he works for the establishment in suppressing new information. However, if at some point the pressure against suppression (by Caton, Mahoney, Catt, Theo and the rest) gains too much momentum, he will identify the new evolving establishment and make noises in their direction, towards a possible later shift of loyalty, while at the same time continuing his everyday suppression. The tokenism towards us will for example be the complaining letter about suppression that he published in Nature, mentioned in the Telegraph.

(Note added in jun90. Daily Telegraph mon 1may89 p18. Biologists were V Huszagh and J Infante, writing in the current issue of Nature. '.... the structure of DNA, ... in Nature in 1953, would "probably not be publishable today", Maddox laments, (because today's referees would block such a disclosure).... Maddox, editor of Nature, laments that today, communication of new theories is totally blocked.)

The key target for my publications is the I.E.E., London (IEE). They have never published anything of mine, either on electromagnetic theory (em) or Wafer Scale Integration (WSI). However, years after the event, they have published articles about the ongoing industrial development of my WSI. (e.g. Brighter prospects..... by R Dettmer, Electronics & Power, April 1986, p283-288. Dettmer is an in-house writer.) This is the "me too", following behind, element in the establishment. Around 1979 Prof. Pat Brown , President of the IEE, was involved in blocking my publications on em. More recently, Prof. Clarricoat, (who initially told me that his speciality was em,) an honorary official of the IEE, refused to intervene over my suppression (in em). He said that as a Professor (head of department) in a London college he did not have time to investigate fundamentals. Recently, Clarricoat told my wife that Brown said that Catt was "brilliant". This is a poor substitute for unjamming the publication process, but effective (false) indication that neither could have been involved in my suppression. You do not block someone who is brilliant! The 100% rejection of my writings by the IEE has now continued for nearly 20 years. However, I was recently invited to lecture to a gathering of perhaps 100 people on em in a local (non London) IEE meeting on em. In spite of the 100% rejection of my em work by the relevant institutions, IEE and Inst Phys, I am now the probably the best known authority on em in the world. My rejection slips total well over 100, spread over 15 years or more, in both em and WSI. em is discovery; WSI is invention. The blocking mechanism is the same in the two cases, and extends over some 15 years. My WSI patents have generated funancial backing of over $10millions, in spite of the blockage on publication in all learned journals. (The blockage is not so complete in the USA, but is still chronic. See for instance Aubusson & Catt, "Wafer-Scale....", IEE Vol SC-13, no.3, June, 1978.) The blockage on publication in Britain will partly explain why the recent round of financing, some $10 millions, all came from abroad - USA (Tandem), Japan (Fujitsu), Europe (SGS Thompson).

In the case of competition for financial backing, referees, who themselves are competing for the same money, are more likely to allow publication of material from abroad, this being less of a competition for local funding. All the same, the best brief conclusion to make is that all publications on innovation are blocked anyway.

The degree of suppression that I have experienced is greater than your writings indicate. This has been damaging, but also gave me the opportunity to probe the Reception System. I have been in the privileged position, from the point of view of research into the reception system, of having a number of disclosures to make of major advances. I have used these disclosures as probes to study the reception system in a way unavailable to most investigators, since they are sociologists not scientists. However, I always needed an accredited independent, preferably sociologist, to prescribe the probing. You fit the bill pretty well, since you have experimented with changing proffered papers in a controlled way (you and Cecci, in different ways). I have always so far met with indifference to my assertion that my attempts to publish my breakthroughs are of primary research value. For instance, I am very well known in my field, so the idea of submitting my work under other names arises, so as to isolate the "Catt factor", if there is one, seems a good one.

I personally think that it is the content of my disclosures which debars them from publication, not that they come from the wrong stable, or come from Catt. I argue that any communication which would leed to the need to alter school courses is taboo. Any disclosure which would lead to the need to alter first degree courses is probably taboo. However, a disclosure which would lead to the need for the addition of a further, unarticulated section in a first degree course would be permissible, since it would not seriously threaten the status quo and the vested interests. This kind of view contradicts your view, that source of disclosure is important. However, it may be that my disclosures are more fundamental, more threatening, than the material whose suppression you usually study, so we are trying to compare chalk with cheese.

I would be interested in suggestions from you as to how I should proceed in detail when I get my rejections. Normally, if I have an important disclosure, I submit it to three or four learned journals, get their rejections, and then publish in Wireless World (a semi-reputable journal) and/or in a book that I myself manufacture, which people buy. It is not far off the truth to say that more or less every attempt by me to publish in my two fields has failed during the last 15 years. In the ten years previous to that, I successfully published everything I wrote, on any subject. I moved from total acceptance to total rejection, 15 years ago. I would say that I got too advanced, and so became threatening.

Again, congratulations on your very fine work.

Yours sincerely,

Ivor Catt

Brian Martic, I note that in your 21mar90 letter you offer help in ongoing suppression cases. As I said to Mahoney, I need professional advice on whether I am conducting my experiments on the scientific reception system most effectively. We need to understand the structure of the suppression mechanism. For instance, I note that the Peters/Ceci find that prestige of source of paper controls referee response (Caton Search Jan89. Caton says in the same article that Mahoney found that the conclusions in an article were pivotal in acceptance/rejection. In contrast, I have found that the absolutely controlling criterion is content; any radical content indicating that established college courses must be altered leads to rejection. Now these three cases seem to oppose each other. However, only yesterday it occurred to me that if we start from the premise that what we are facing is the Bernstein model in control, "Knowledge is property with its own market value and trading relationships", it is likely that we are seeing three different evolved methods for protecting property in different disciplines. As one homeowner might protect with a dog and another with locks, so Physics might protect with blocking content while biology (say) protected by blocking offerings from foreign stables. Now this is an example of the kind of work that needs to be done in this field, and I am a key person in the work because I have major disclosures in more than one field which have been blocked for decades. Mahoney or Martin or Caton should exploit my situation by planning submission strategies - not to help me to get published - that won't happen anyway - but to study the blocking mechanism. I have lengthy correspondence with many editors etc and could get much more. So has Theocharis, whose disclosures are worth watching, although not as much as mine.

Mahoney is important because the group in Leicester (since disappeared?) were referencing Mahoney in around 1975 or so, and still today, Caton references Mahoney's recent writings. He's impressed people for a long time.

More generally, I need to establish who are the major contributors in the field of the Politics of Knowledge, and how do they gauge each other. As with Feminism, there will be some freeloaders, using this new discipline, The Politics of Knowledge, which is research into whether the scientific era has come to an end because communication has ceased, as a tool for their career progression. Others will be serious workers in the field. I myself see the Politics of Knowledge as likely to be one of the main academc disciplines in twenty years from now, when it will have become obvious that communications of major (paradigmal) advances has ceased.

As a practical example of the problem, I would expect that more than one man has already discovered the cure for AIDS, but cannot communicate it. That is the class of information which is 100% suppressed today. (This is if the cure is in some way radical - paradigm breaking, like the shift to meta-medicine in some way.) This is the kind of communication which, if a civilization evolves a way to suppress it or even to delay its communication, that civilization collapses. The more complex a culture, probably the more it needs efficient communication of that class of information, or it will disintegrate. (Institutionalised delay of ten years, as well as complete suppression, as a norm, might mean the end of a sophisticated culture such as ours.)

Thank you again for your book.

Yours sincerely,

Ivor Catt

cc Harold Hillman, Surrey University,
Guildford, Surrey.
cc Theocharis, 200a Merton Rd,
London SW18
cc David, Amy Burnett, 24 Bowers Way,
Harpenden AL5 4BW