

Ivor Catt, 121 Westfields,
St. Albans AL3 4JR, England.
01727 864257
13jan96 [This copy printed 25/03/97]

Brian Martin, Department of Science and Technology Studies,
University of Wollongong,
NSW 2522, Australia

Dear Brian,

Thank you so much for sending a copy of the article by John Hendry, "Weimar Culture and quantum causality", *Hist. Sci.*, xviii (1980), pp155-180, which is the refutation of the Forman Theses, 1971

Hendry's note no. 111, p179, points to my dilemma; "... One is even tempted to ask whether extrinsic influences led Paul Forman to ardently hope for, and willingly embrace the theses he puts forward."

Did Forman disapprove of Modern Physics, and Hendry support it? Are these two papers a piece of shadow-boxing, when the argument should be the direct one, about the validity of Modern Physics? I need to know this.

Over the decades, I have become increasingly sceptical about Modern Physics, and increasingly saddened at the way it has drawn Philosophy of Science, History of Science and much else into its maw.

History of Science as it is practised seems to be so heavily delimited. Years ago I discovered that there was no overlap between all of Heaviside's work and electrical theories, and Modern Physics. Nobody in Modern Physics ever read any Heaviside. This seems to apply to historians of science as well, at least to the sub-tribe which includes the Forman-Hendry jousting. I suggest that Heaviside will not find mention in the writings of Forman and Hendry. Yet Einstein said that Maxwell's Equations of the Electromagnetic Field were the foundation of Relativity. I rate Heaviside and Faraday as the most important contributors to electromagnetic theory.

Am I wrong to drift between relativity and quantum electrodynamics? Do Forman/Hendry treat of the latter and not the former?

"The special theory of relativity owes its origin to Maxwell's equations of the electromagnetic field." - written by Einstein in Schilpp P A, "Albert Einstein, Philosopher-Scientist", Library of Living Philosophers, 1949, p62. Also, on p17, Einstein writes; "... the approach to a more profound knowledge of the basic principles of physics is tied up with the most intricate mathematical methods."

Perhaps on the basis of those two remarks by Einstein I should write off the whole shebang - modern physicists and their client historians of science, in view of what I have found out about Maxwell's Equations and intricate mathematical methods, as published in my articles in *Wireless World* march80 and *Electronics and Wireless World* nov/dec85. I discovered that Maxwell's Equations contained virtually no information, and were just a mirage of mathematical manipulation; plausible in Maxwell's time but not so today. We now know the TEM Wave, whose discovery should have ended the reign of Maxwell's Equations. Heaviside is not culpable, because he was struggling to *establish* the TEM Wave, which he discovered. However, a century later, there is no justification for ignoring it, and its implications for Maxwell's Equations. (The TEM Wave is only cursorily mentioned in today's books on electromagnetism, and the amount of discussion continues to diminish with more recent books.) For instance, the fact that E and H are *in phase* in the TEM Wave knocks the bottom out of Maxwell's Equations. They survive so long as we continue to vaguely think that in the TEM Wave, they are out of phase.

Heaviside wrote with curiosity about Einstein's Relativity in letters late in life. Einstein never read Heaviside, who is my mentor.

Modern Physics does not contain the impedance of free space, $\sqrt{(\mu_0/\epsilon_0)}$ (=377 ohms), which is one of the primitives for the electronic engineer. (I pointed this out in *Wireless World* sep84 and elsewhere.) Although Modern Physics romances about the velocity of light,

$1/\sqrt{(\mu_0 \epsilon_0)}$, it does not touch base with the very similar, and very important, $\sqrt{(\mu_0/\epsilon_0)}$. A little while ago, when I phoned him, my co-author Dr. David Walton seemed to think I should dismiss them all as rather ill-informed and so of little consequence. He and I are in the tradition of Heaviside, and owe nothing to the twentieth century. It seems incredible that we can gain nothing from a century littered with apparently hard-working researchers.

I was in the semiconductor industry at the crucial stage in its development, from 1962 to 1968. The advances it made owe nothing to modern physics including quantum electrodynamics. My industries, computers and microelectronics, are not examples of the success of modern physics. My work on high speed logic (electromagnetic theory) is based on Heaviside and Faraday in the nineteenth century. I pioneered the interconnection of high speed (1 nsec) logic at Motorola in Phoenix Arizona in the sixties.

I knew Lawrence of the Lawrence and Warner curves for sidewall capacitance. I was there when he developed MNS as a hoped-for more stable alternative to MOS. When it went the other way, I was at Sperry Sudbury when they tossed a coin over which of two theories to promote to get funding to push forward with their newly developed EPROM work using the *less stable* MNS. None of this work derived any benefit from relativity or quantum electrodynamics, or from Modern Physics for that matter. It was very much 'cut and try'.

Should I dismiss the twentieth century as a gap in the advance of science?

Yours sincerely,

Ivor

cc Dave Walton,
103 Cromarty, Ouston,
Chester le Street, Co. Durham

To Brian Martin From Ivor Catt 14jan96.

Thank you very much for your further 78jan96 letter, just arrived, which mentions James T Cushing, *Quantum mechanics: Historical Contingency and the Copenhagen Hegemony* (Univ of Chicago Press, 1994), which argues that the Copenhagen interpretation became dominant because it got there first. I shall pursue it.

I do so hope your theoretical position does not exclude the possibility of *error*. My associates argue that the move into Modern Physics was a *mistake*. Further, it is not only we who argue that Modern Physics is anti-scientific. If we are correct, then the minutiae of Modern Physics do not bear on the nature of science. One would not learn about the functioning of democracy if one studied the behaviour of a military junta which had temporarily hijacked a traditionally democratic country. It would be particularly important not to adapt one's model of democracy while under the temporary tyranny of the junta, adjusting it to the way the junta operated, even if the junta said they were democratic. That would validate and entrench the junta, in the same way as Establishment historians of science and philosophers of science are currently validating and entrenching Modern Physics, merely because its tyranny has lasted so long. The fact that the Stalin empire lasted so long has little bearing on the theory of how a democratic society functions

Perhaps the twentieth century was taken up by the one exercise; of proving that professional (paid) scientists freeze science, and cannot take it forward, even if the latter is explicitly what they are paid to do. If this is true, then picking over the degeneration they instituted would not tell us about the arcane aspects of properly functioning science, any more than study of a motorway pile-up would teach us about traffic flow.

To Brian Martin From Ivor Catt 14jan96bis

I suppose I am discussing the trap which is Whig History. In order to get funding, historians of science will presumably take the whig view of history, and then the Copenhagen Hegemony will authorise their funding.

A Whig Historian, one committed to the view that current science is the best destination of past science, can only ever be the handmaiden of an entrenched scientific Establishment. A whig historian will never enable us to study the nature of the historical errors which led to our present imperfect situation.

The astounding thing is that part of the current Establishment ideology is that there exist no absolute truths! The idea of whig history in such a situation is bizarre.

To Brian Martin 25mar97

Thank you so much for your 1997 book SUPPRESSION STORIES, ISBN 0 646 30349 X, which I read with great interest.

On p114 you mention articles in *Search*. Why do you not give Caton's important and very relevant articles in *Search* sep88 and jan89 a mention?

Ivor



Department of Science and Technology Studies
University of Wollongong, NSW 2522, Australia

8 January 1996

Ivor Catt
121 Westfields
St. Albans AL3 4JR
England

Dear Ivor,

Another reference on history of quantum theory: James T. Cushing, *Quantum Mechanics: Historical Contingency and the Copenhagen Hegemony* (University of Chicago Press, 1994). He argues that the Copenhagen interpretation became dominant because it got there first; if a Bohm-style deterministic formulation/interpretation had been promoted in the 1920s, it might have become dominant instead. This supports the idea of underdetermination of scientific theories (especially at the level of ideas of the ultimate nature of reality)—namely that nature does not force us to choose a particular explanation—and is compatible with the “weak Forman thesis” (that Weimar milieu was an influence in a situation where choice between theories was occurring, not a determination of the form of theory or its interpretation). This book provides lots of references to earlier work in the area, e.g. Forman, Hendry, and so is a good source.

Yours, 

Brian Martin

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*Department of Science and Technology Studies
University of Wollongong, NSW 2522, Australia*

4 January 1996

Ivor Catt
121 Westfields
St. Albans AL3 4JR
England

Dear Ivor,

Thanks for all the letters and copies of letters that you've sent me in recent months. My apologies for not responding sooner. Recently I've been finishing a draft of a book on "technology for nonviolent struggle," which has taken a lot of time.

Enclosed is a copy of the article by Hendry that I mentioned in my last letter. It is a detailed analysis of Forman's argument, going into the beliefs of the individuals involved in developing quantum mechanics. Hendry argues that Forman's model of social determinism does not stand up due to the influence of internal factors in physics, the insulation of scientists from the anti-causality milieu, the variety of positions within the physics community, etc. Hendry affirms an interactive position (i.e. knowledge and society interact) while attacking Forman's allegedly totally externalist picture (i.e. society determines knowledge).

It seems to me that Hendry, by working on a micro scale, is less likely to affirm any sort of external determinism, and that he does not give recognition to the possibility that physicists could have forged a causally interpreted quantum mechanics with much the same mathematics as was actually developed. As I said in my last letter, the history of science can be just as contentious as science itself.

I appreciate all your comments on my paper on plagiarism, but can't do them justice here. In my paper I canvassed various arguments against plagiarism, but they are not necessarily arguments with which I agree. I agree with you—if I understand your position correctly—that it is important to assess both claims and authors or, in other words, the message and the sender. It is in situations with institutionalised plagiarism that the implicit assumption is that correct attribution of authorship does not matter. My reason for raising the idea of self-management is to highlight a system which is quite incompatible with institutionalised plagiarism. There are many more references on self-management than the few I provided, but that might take us too far afield; perhaps we can agree to disagree about the value of participatory democracy. It is worth mentioning, though, that the classical model of the scientific community is that it is epistemologically self-managed, in the sense that decisions about ideas are made by a community of scholars based on the evidence and credibility established by intellectual contributions, rather than decisions being made by bosses. (Of course, the actual practice of science deviates from the ideal in many ways, and there is plenty of institutionalised plagiarism.)

Yours,



Brian Martin

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pm 26.12.95 r 31.96

Ivor Catt,
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0727 864257
24dec95 [This copy printed 24/12/95]

Brian Martin, Department of Science and Technology Studies,
University of Wollongong,
NSW 2522, Australia

Dear Brian,

Plagiarism: A misplaced Emphasis [PAMC]
Brian Martin
Journal of Information Ethics, Fall 1994, vol.3 no.2 pp36-47

I have just re-read the above article for the second time, making a total of three readings. This time it was with my friend Sue Warman.

Sue and I agree that there is no logical connection between the admirable first portion up to p43 and the third rate last 2pp "Plagiarism in a Self-managed Society", which is not described but for which you admittedly give four sources. Do you assume that the new political structure is well known?

Both Sue and I are or have been in high administrative positions (Sue is Associate Dean in charge of perhaps a budget of £5 millions p.a. and 2,000 students - I guess these numbers. I was Principal Lecturer in a very large department.) Neither of us see the majority of administrative problems as being tightly linked to ideology. In particular, we find democratic decision making tedious, expensive, divisive and hard. Decision making is something best delegated to decision makers with occasional correction by the Will of the Group. "Self-managed Society" makes no sense to me, so perhaps I need to read your four sources. Sue is conscious of the need for a bureaucracy to command respect, while you seem to feel that the less respect it has, the better.

I am surprised that you appear to think that changing the structure of society would bear significantly on the problem of plagiarism, which we think inheres in the human condition. Perhaps you were bullied by an editor into pretending to have a solution to the problem you describe - plagiarism.

Yours sincerely,

Ivor

Ivor 25dec95

Yours sincerely,

~~assertion. Some cases are blindingly obvious, as when an author attacks his own earlier position~~
During the last decade I came fully to realise that not only the source of an idea is important, but also the date of the point. It is important. I do however appreciate that you may be referring to a view, not necessarily your own. Please forgive me if I overargue out. Such an idea seems orthogonal to your assertion in your plagiarism article that the important thing is that truth should be read. I recently wrote to a reader that the mass of mathematics in my 1994 paper is there for political reasons, and should not be writing in counterpoint to whom, and when and why. I oppose the Kuhn assertion that scientific progress is a matter of continuous accretion. However, from my and Kuhn's perspective, the question of the pedigree of assertion becomes more, not less important. For us, it is so important to know who meant. Your assertion cuts across such ideas. Heaviside wrote along the lines of "When one reads someone, one should try to establish, not what he wrote, but what he meant. Your assertion that however new information comes out, the important thing is that it come out, becomes more apparent. I need information of Forman to know the totality of meaning of his article, and also to know how I should use it. The weakness of your assertion that however new information comes out, the important thing is that it come out, becomes more Having just completed Forman's 100particel, the importance of my earlier letter, saying that the pedigree of information/ideas is so important, struck me forcibly.

Plagiarism: A misplaced Emphasis [PAMC]
Brian Martin
Journal of Information Ethics, Fall 1994, vol.3 no.2 pp36-47

Dear Brian,

Ivor Catt,
121 Westfields,
St. Albans AL3 4JR,
England
☎01727 864257
26may95

Second copy sent 18aug95 Third copy sent 3sep95
[This copy printed 24/12/95]

The Secretary,
Institution of Electrical Engineers,
Savoy Place, Strand,
Central London. WC2R 0BL

Dear Sir,

The Catt Anomaly.

An essential component of classical electromagnetism remains unstated. There is disagreement about this feature by accredited experts, Professor Howie FRS, Professor Pepper FRS, McEwan Reader in Electromagnetisc, but no discussion by them to resolve the matter.

Is the IEE the accredited institution with a primary responsibility for Electromagnetic Theory? How does the IEE proceed in a situation like this, where the theory which is the basis for its rason d'être turns out to be unstated and unclear?

Yours sincerely,

Ivor Catt

encl.

21june93 statement on the Catt Anomaly by Pepper

20apr95 statement on the Catt Anomaly by McEwan

apr95 Half page note from Symonds to McEwan plus description of the Catt Anomaly

Catt letter to Electronics and Wireless World, May95

Summary of disagreement, or confusion, in classical electromagnetism, below.

Summary of disagreement.

"Dear Professor John Gardner, As part of our [Bradford university] program, 'What is Education For?', we need comment from the accredited Bradford University expert on the subject below" - Kathy Symonds, 4apr95.

"[Professor] John Gardner has passed this on to me - I think I can claim to be reasonably competent to discuss it.... the new charge required in the one foot of cable **DOES** flow from somewhere to the left! The charges **DON'T** have to travel anywhere near the speed of light to do this! It may be obvious to the untutored mind [plus Pepper FRS] because they haven't had the [Bradford univ.] theoretical training ¹.... The [Catt] 'anomaly' is very instructive educationally...." - Neil McEwan (Dr), Reader in Electromagnetics [Bradford University], 20apr95.

".... As the wave travels at light velocity, then charge supplied from outside the system [i.e. from the left, or west,] would have to travel at light velocity as well, which is clearly impossible.we have a lattice of positively charged atoms surrounded by a sea of free electrons which move in response to an electric field...." - Pepper, 21june93.

".... as a TEM wave advances so charge within the conductor propagates at right angles to the direction of the wave." - Professor M. Pepper, FRS., Cavendish Laboratory, Cambridge, 23aug93.

¹".... When the wires are electrically neutral, they are actually composed of vast numbers of positive charges and negatively charged electrons in equal densities - the total charge balances out...."

Dear Mr. Catt,

Thank you for your letter of 18 August, to which the [IEE] Secretary, Dr Williams, has asked me to respond.

.....

The Institution has a responsibility to 'promote the general advancement of electrical science and engineering and their applications and to facilitate the exchange of information and ideas on those subjects to members of the Institution'. The general view of the experts within the IEE is that the so-called 'Catt anomaly' is not an anomaly at all, and does not, therefore, require discussion or exposition. The favoured explanation aligns with the statement to which you refer, attributed to Professor Pepper, namely that as a TEM wave advances, so charge separation occurs close to the conductor surface effectively giving a transitory current flow at right angles to the direction of wave propagation.

Yours sincerely,

Professor Philip E Secker Deputy Secretary 4sep95

Dear Mr Ivall

.... so-called Catt Anomaly.

I believe the whole question will be cleared up, finally when the IEE publishes a review of Ivor Catt's book, in the near future. Dr. McEwan really has the answer; Ivor Catt's basic statements are fallacious. The reason that the Catt anomaly has been around so long is that the 'experts' have not thought it of sufficient standing to take the trouble to demolish it!

Kind regards Yours sincerely

Professor Philip E Secker Deputy Secretary 25oct95

[McEwan wrote, 20apr95; "The sentence [by Catt] that begins 'Not from somewhere to the left' is fallacious such charge would NOT have to travel at the speed of light...." This is the only mention of fallacy by McEwan, Compare with Pepper FRS 21june93; "... charge supplied from outside the system [=from the left] would have to travel at light velocity as well, which is clearly impossible." This contradiction between Pepper and McEwan is the reason for Catt's 18aug95 letter to the IEE. The request to the IEE, to resolve this contradiction, has nothing do to with any theory, ideas or books of Catt's.]

Dear Dr. Simmonds,

Thank you for your letter of 30 September;

I should explain that I am no expert in the area to which the 'Catt Anomaly' refers, but I have read sufficient erudite comment - for example from Professor Pepper - to believe there is no anomaly and that a simple explanation based on our understanding of conduction processes in metals suffices.

Yours sincerely

Professor Philip E Secker Deputy Secretary(A) 26oct95

Dear Mr Hockenjos

Thank you for your letter of 25 November 1995.

Contrary to your comment, the IEE is in no disarray whatsoever.

.....

Professor Secker has consistently pointed out that the expert opinion within the IEE is that the so-called Catt anomaly is not an anomaly at all, can be simply explained, and has referred to statements from many experts. The book reviewer would appear to concur with the views of these experts.

.....

Yours sincerely

John C Williams [Secretary, IEE] 6dec95



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Secretary
J C Williams
PhD Feng FIEE

Mr Ivor Catt
121 Westfields
St Albans AL3 4JR

4 September 1995

Our Ref: PES/mlh 839
Direct Dial: 0171 344 5486

Dear Mr Catt

Thank you for your letter of 18 August, to which the Secretary, Dr Williams, has asked me to respond.

Firstly, I should mention that we have had your book reviewed and that the resulting report will be published in the Electronics and Communication Engineering Journal - either in the October or December issue.

The Institution has a responsibility to 'promote the general advancement of electrical science and engineering and their applications and to facilitate the exchange of information and ideas on these subjects to the members of the Institution'. The general view of the experts within the IEE is that the so-called 'Catt anomaly' is not an anomaly at all, and does not, therefore, require discussion or exposition. The favoured explanation aligns with the statement to which you refer, attributed to Professor Pepper, namely that as a TEM wave advances, so charge separation occurs close to the conductor surface effectively giving a transitory current flow at right angles to the direction of wave propagation.

Yours sincerely

Professor Philip E Secker
Deputy Secretary

Departmental Fax Numbers: Council Officer 0171-379 7707; Learned Society 0171-497 3633; Conference 0171-240 8830; Library 0171-497 3557; Qualifications 0171-497 3609; Technical Affairs 0171-497 2143; General 0171-240 7735.

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Secretary
J C Williams
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File to copy

Mr K Metzger MA MIL AFIMA
241B Minehead Court
Alexandra Avenue
Harrow
Middx HA2 9DT

19 September 1995

Our Ref: PES/nlh 848
Direct Dial: 0171 344 5486

Dear Mr Metzger

Thank you for your letter of 14 September in support of Mr Catt.

As far as the IEE is concerned, I would assure you that where Mr Catt received his secondary education is of no influence whatsoever. The problem with the 'Catt Anomaly' appears to be that the 'experts' do not see it as an anomaly and certainly have never accepted as a starting point, even for discussion, the physical model that Ivor Catt has postulated.

I agree with you that it is unwise to leave an anomaly unresolved for many years. But most scientists, I gather, felt the matter was settled a long time ago.

The review (which we shall be publishing soon) dealing with Ivor Catt's book contains a section on the 'Anomaly', which will hopefully finally resolve the issue.

Yours sincerely

Professor Philip E Secker
Deputy Secretary

Departmental Fax Numbers: Council Officer 0171-379 7707; Learned Society 0171-497 3633; Conference 0171-240 8830; Library 0171-497 3557; Qualifications 0171-497 3609; Technical Affairs 0171-497 2143; General 0171-240 7735.

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Secretary
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copy

J C Simmonds MSc(Eng) PhD FIEE
Walkwood Lodge
Beaconsfield
Bucks HP9 1PR

26 October 1995

Our Ref: PES/mlh 869
Direct Dial: 0171 344 5486

Dear Dr Simmonds

Thank you for your letter of 30 September; I apologise for the delay in replying - this is a very busy time of year at the IEE.

I should explain that I am no expert in the area to which the 'Catt Anomaly' refers, but I have read sufficient erudite comment - for example from Professor Pepper - to believe there is no anomaly and that a simple explanation based on our understanding of conduction processes in metals suffices. The IEE is shortly to publish a review of Mr Catt's recent book; the reviewer deals very positively with the 'Catt Anomaly'. I do not see that any further coverage of the topic will be either necessary or justifiable.

Kind regards

Yours sincerely

Professor Philip E Secker
Deputy Secretary (A)

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Secretary
J C Williams
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Dr J C Simmonds CEng FIEE
Walkwood Lodge
Beaconsfield
Bucks HP9 1PR

8 November 1995

Direct Tel: 0171 344 5415

Dear Dr Simmonds

Thank you for your letter of 3 November, to which I am replying in Professor Secker's absence.

The Institution does not have Technical Committees which address scientific principles. Consequently, I do not believe that there is any Committee of the Institution to which your earlier letter could usefully be referred.

Yours sincerely

Andrew F Wilson
Support Executive Secretariat

Departmental Fax Numbers: Council Officer 0171-379 7707; Learned Society and Conferences 0171-497 3633; Library 0171-497 3557; Qualifications 0171-497 3609; Technical Affairs 0171-497 2143; General 0171-240 7735.
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Even 5 sentences written by you
to J C Williams, IEE, would be
*very helpful. JC 22 Nov 95

pm 23.11.95 r 28.11.95

Ivor Catt,
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22nov95 [This copy printed 22/11/95]

Brian Martin, Department of Science and Technology Studies,
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NSW 2522, Australia

Dear Brian,

Plagiarism: A misplaced Emphasis [PAMC]

Brian Martin

Journal of Information Ethics, Fall 1994, vol.3 no.2 pp36-47

I have just re-read the above article.

With the caveat that perhaps I cover too wide a range and may not realise that you are my window to a large body of knowledge that I do not know at first hand, (see for instance the set of references in this paper), I still would assert that it is a very fine piece of work. I know that your range is very broad anyway. In any case, I am expert in the Politics of Knowledge, which is not far from the subject of plagiarism.

Comments on PAMC.

My comment on your p41 (below) is partly made, but not strongly enough, by you yourself in your p43; "... pragmatic, utilitarian allocation of credit." It is also implied in a sense on p45; "... cultural context....".

p41. There is here a tendency to imply that a knowledge advance, what I call *new* knowledge, is self-contained. Opposed to this is the concept of the name of an Ent (elaborated by Catt), in *The Fellowship of the Ring*, Tolkein. It is rude to address an Ent by less than its full name, and the name of an Ent is the life history of the Ent. *new* knowledge sits at the paradigmatic apex of a triangle of old knowledge.

Definition. *new* knowledge is knowledge leading to a change in an A level (students aged 16-18) text book, or the need to change a section of a first degree syllabus. Information is not *new* if it does not disturb the incomes of the main bulk of professional knowledge brokers - teachers and lecturers, their course notes and their text books. Information is not *new* if it calls for an additional section in a first degree syllabus (meaning merely *more* money for the entrenched knowledge broker). Each block of lecture notes brings in one hour's salary to the lecturer each year. Those lecture notes are *capital*, generating yearly *interest*. Such capital must be defended against *new* knowledge. This is why any allegedly *new* Catt idea is dubbed not *new*, but merely a (poor) new way of describing old knowledge. Then, the *capital* survives.

Putting it another way, part of the meaning of *new* knowledge is its (paradigmatic) pedigree, (usually) none of which is *new*.

It is possible to register new knowledge without giving its pedigree, but when this is done, the *new* knowledge, already difficult to comprehend, will (usually) not be (fully) comprehended. That is, clear disclosure of *new* knowledge includes (an outline of) its full pedigree.

Plagiarism confuses the pedigree of *new* knowledge, and so confuses the knowledge. As an absurd illustration, Catt had a good idea called Catt Spiral, which received £16 millions of funding. Even with proper referencing, which unfortunately there *was*, the superposed silly idea "Cobweb", which gained funding for third parties and so forth, confused the original Catt Spiral, its method, economic value and meaning. How much more would *plagiarism* have confused and damaged Catt Spiral! (A cobweb contains a spiral, but to think of a coil of rope as a rudimentary cobweb [open to enhancement into cobweb] creates a mental shambles.) Thus, the environment of *new* knowledge is fragile.

The previous paragraphs above show that, your p41; "... since the knowledge is disseminated whoever gets credit for it." is a dangerous idea. As another instance, the suppressed statement of the *new* knowledge, "A capacitor is a Transmission Line", Catt et al., *Wireless World* Dec78, depends heavily on context for it to have meaning. Lacking context, it will be reinterpreted as rather curiously stated *old* knowledge.

For further tangential illustration of this point, read Catt, "The Rise and Fall of Bodies of Knowledge", *The Information Scientist* 12 (4) dec78, p139 (refZ);

"..... the meaning of a word [and thus of a paragraph or more, particularly containing *new* knowledge] is a creature of the frame of reference [which includes the list of relevant speakers] within which it has traditionally been used." Read the page before and the page after this quote.

Thus, plagiarism causes the destruction, or at least the obscuration, of *new* knowledge.

