

ELECTRONIC PUBLISHING: TECHNICAL CONSTRAINTS WITH POLICY CONSEQUENCES

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INTRODUCTION: RESEARCH DISSEMINATION AND NEW TECHNOLOGY

This paper aims to start a discussion on the impact of electronic publishing on the activity of research in economics. It is a sort of normative follow-up to my report of the 1997 ASSA session on electronic publication in CHEER (vol 11 #1:32-33). There as an observer I tried to report what is actually happening; here, as a consumer and producer, I'd like to suggest what ought to happen.

A FEW TECHNICAL PRELIMINARIES

In the sense which economists hold dear, namely the marginal cost of a bit more more stuff, web dissemination is virtually costless. Yet the actual business of producing, reviewing, selecting, and disseminating research articles, particularly for journals and above all for economics journals, is not exploiting the technical possibilities as once expected. Evidently the simple economics of publication do not furnish the principal constraints on the journal business, which leads us to ask what these constraints really are.

The ASSA contribution from Wayne Marr of Social Science Electronic Publishing traced the history of attempts to adapt the publication process to the web. The surprising outcome is that the internet has not - so far - imposed any fundamental change in the visible form of the principal means of scholarly dissemination, namely the journal article. Though from the early hype it might easily have been concluded that the web had sounded the death knell of the journal form, instead, the adaptations now emerging suggest that the journal is migrating to the electronic medium.

Indeed the web page itself does not seem to migrate several essential functions of economic scholarship. This is surprising because the physics community evolved it precisely to overcome the limitations of printed dissemination. First off, therefore, let us take a look at some of the technical considerations involved.

INTERACTS AND ACROBATS

The two main innovations which computerised storage and presentation have brought, namely interactivity and hyperlinking, don't seem to migrate to scholarly dissemination. They are useful educational aids, but where the prime function is to communicate and collectivise research results, they don't work.

Interactivity is a useful educational device, and might at some later time reduce the labour-time of reading, but doesn't seem to add much to the straightforward business of telling other researchers what you've found and letting them confirm it. One important exception emerged in the ASSA discussion, namely the communication of bulk data. A web publication can make raw data, and calculations themselves, available to other researchers so that they can then find out how the publication's results were reached. My personal view is that this should in short order be made a pre-requisite to publication, a step which would probably effect a notable reduction in the volume of text in circulation.

However the strangest conclusion from the ASSA session, borne out by many experiences, is that the HTML form itself is inadequate to scholarly dissemination. The reason seems to be that the journal

article, like the book, is an inherently page-formatted means of reproduction. This has led to widespread recourse to Adobe Acrobat instead of hypertext as preferred means of journal storage.

Acrobat is a page-description language in the strict sense, whereas the words 'web page' are a misnomer because there is no restriction on the length of a page, which in any case varies with the browser and the reader's viewing choices. Acrobat pages, on the contrary, are shown on the screen as originally printed. They may be simple photographs of the page as with JSTOR's electronic archive of early AEA publications, or they may be a standard page-formatted representation of the distributed text. The crucial point is that the reader is not free to change the format. Acrobat is therefore a standard independent of viewing choices.

The re-birth of a system originally designed for the print medium is surprising because one might think the only reason to divide an article into printed pages is in order to print it. If it isn't on paper, what is the page for? Here we begin, I think, to get a clue to what is going on. When scholars communicate with one another, the page is a means of location, an indexing device; as such it succeeds because it is universal. No matter what the form of the text, whether with headings or no headings, diagrams or no diagrams, footnotes or no footnotes, tables or no tables, it can always be divided into measurable chunks defined by the visual space which it occupies.

Geeks might object that this is superseded by more modern devices. For example, we could directly refer to any part of a text we chose, if authors adopted the universal convention of incorporating hyperlink targets in their text for citation purposes. Oh really? The social and institutional changes needed to make this practice universal - even with automatic aids - are staggering. But they would also be retrogressive, because it is not the author's prerogative to define what can be cited. Once in the public domain, the choice of what to cite is the public's. And the most universal public standard presently available is a visual, or more precisely spatial reference: namely a point on a page.

LINKING AND THINKING

This brings us to hyperlinking, arguably destined to transform the business of citation and reference. Again, though this is still waiting in the wings as a possibly viable accessory, its early promise has evaporated.

It is worth rehearsing what this promise consists of. If authors could include not just publication details of cited articles but links to them, they would transform the business of reading. As readers we could instantly find out whether the author has fairly represented any previous writers referred to, assess which parts of her or his work were original, and by back-linking trace the historical chain of its evolution. It may not be long before some enterprising person constructs a robot for this purpose. This might be uncomfortable for the author, but it is hard to doubt its value to the reader, and it would certainly raise the quality of the product.

Nevertheless, for reasons I think far from spurious, hyperlinking doesn't work. Bluntly, the links don't stay still. Half the work of maintaining a decent web site is that the references keep moving. The average halflife of a hyperlink in contemporary cyberspace seems to be about forty days, as far as I can see.

AUTHENTICATION: AN INSTITUTIONAL QUESTION

My principal point is that this is not just a technical question. The underlying institutional cause is that the referenced items themselves are privately maintained. One might think, for example, that the easiest way to cite Black and Scholes' celebrated formula is a link to the website of either Professor Black or Professor Scholes. But neither is under any institutional obligation to keep the document on the same URL, or indeed - as pointed out in the ASSA discussion - to maintain the integrity of the text. The obstacle which electronic production presents to research activity is precisely its greatest strength, namely it is an immensely fluid medium. It is the easiest thing in the world to generate endless drafts and versions of the same document, and this is the author's right. But the requirements of scholarship

conflict with this right, because the consumers - other scholars - must all refer to the same text, the same object. Otherwise there can be no effective principle of independent verification.

It might be thought that this is solved by libraries or their electronic equivalent, the electronic resource providers, and I suspect that the eventual solution will be a modified library principle. But these themselves cannot easily be constrained to maintain materials in a hyperlinkable form. A library can be thought of as a self-regulating document resource. It provides a means of finding scholarly objects. But, precisely because its business is provision, it must have the right to organise its method of provision. If we demanded that electronic resource providers always place their materials in an electronic location that was in some sense 'logically permanent', so that the links would always work, we would be asking for the equivalent of requiring libraries never to reshelve their books.

There may be a technical solution to this; a sort of universal document naming system maintained by something like the INTERNIC, so that even when moved around logically (reshelved) any document can always be located. Perhaps this is the way things will go. But in addition to a naming standard, it also requires some means of verifying that the named object has not changed. Someone, somewhere, has to be assigned the task of the 'curator of the objects'. This cannot be the same person as the document originator. Things are not therefore so easy.

Moreover, why wait? Such a system exists now: it is called 'ISSN'. The printed medium is a reliable authentication device for the simple reason that it is hard to alter, so that no special institution is needed. Disseminating the printed word is in itself an authentication device, as pointed out in the ASSA discussion. A researcher can with great confidence refer to 'the' seminal article on relativity by Einstein precisely because it is stored in a large number of libraries and private bookshelves, and therefore hard to tamper with.

A COMMERCIAL DIGRESSION

A great fear in the journal community, particularly among publishers, is that electronic dissemination spells commercial disaster. The resulting extension of copyright law has divorced it from its original intention, which was to protect the rights of the author. Like any system of private ownership, intellectual ownership has the function of alienating, of rendering the fruit of mental work a vendible substance, to be disposed of by others than the owner of the brain it came from - whether publishers, editors, heirs, or antiquaries. In consequence when an article is under consideration for a journal it is in its least accessible form, since every author knows that once an article is in the public domain, no commercial journal will consider it.

This is in evident conflict with the requirements of scholarship. First, it lowers quality. It is precisely at the moment when an article is being considered for the 'seal of approval' of publication in a recognised forum, that an input from the community of scholars would be most useful. Second, it interferes with the vital scholarly requirement of early dissemination. Two- and three-year waits for the results of research are not a spur to it.

TEARS, FEARS AND COMPETITION

Technology is here playing a corrosive role. Like judges, producers are discovering a further virtue of the internet over and above universality, namely speed. The Social Science Research Network model, based on relatively unselective but instant dissemination, succeeds precisely because it provides rapid access to new research, which is why it is not junk; it fulfils a necessary scholarly requirement which the standard dissemination process impedes. It is moreover striking that the first web application which the quality journals are offering their readers is pre-publication access to accepted articles. This is a response to an evident and reasonable demand: scholars want to see the damned things, and the quicker the better.

However I find the commercial reasoning behind the copyright reaction also suspect. Technology may yet combine with demand for rapid access to strand over-zealous copyright-holders. Beached whales should not be surprised if few tears are shed for them.

Restraint on pre-publication dissemination contrasts with the new practice of software companies who now regularly distribute thousands of 'beta' products almost free, before final versions are produced, so that the community of users can find as many problems as possible. But this process also puts the new ideas, albeit in raw form, in the hands of this wider community in its earliest usable form. I don't see why the same principle cannot extend to research hypotheses.

Moreover tears and fears alike are probably as premature as announcements of the death of the journal. We can get a clue to what might happen from the answer given by Reni Schultz, editor of the Journal of Finance, to the frequently asked question "The referee is an idiot: what shall I do?" His practical instructions for appeal include an equally practical request for \$150. This is nothing new: submission fees (and reviewers' fees) are widespread in mainstream journals. But if material costs fall to (marginally) zero, then the process will be reduced to its essentials, namely reviewing and assessing with a view to selecting. This is a mental labour process, and an expensive one, and I suspect a section of the journal market will find a quite adequate source of revenue by recognising that the true consumers are probably the people who want their articles in print.

A second clue comes from the ASSA session chair's description of Wayne Marr of the SSRN who was introduced, if memory serves, as 'a successful example of the modern breed of intellectual entrepreneur'. The cost of access to an SSRN journal is not on the whole large—perhaps \$10-\$20. This very cheapness is the basis for a new mass market. The JFQE might (or might not) have trouble with a \$10 subscription on its present distribution base because I suspect it is consumed via libraries. But if consumption were extended electronically to the individuals that presently used the libraries, a very different commercial base would emerge.

As competition remorselessly grinds out the merely incidental, I suspect the migration process will probably establish a different rigime. At the end of the day, why should Professor Joe and Professora Jane Doe subscribe to the JFQE instead of Rupert Murdoch? Since we buy our hamburgers from McDonalds, why not our journals? At \$20 for a swatch of journals, access is no longer commercially restricted to certified scholars. I can see absolutely nothing wrong in the renaissance ideal of universal scholarship, and anything that makes the results cheaper and more accessible is not only going to extend the commercial market and therefore the income base of the entire profession, but will - if we Do The Right Thing - subject that profession to a constraint too long lacking, namely, the informed criticism of the general public.

Which brings us neatly to our main point: the vexed question of quality.

POLICY QUESTIONS

It seems not unreasonable to suppose that the journal as a medium offers a number of features decisive to scholarly activity, which the web will not rapidly overcome. In particular it offers two essential prerequisites of scholarly activity:

- a) Institutionally reliable authentication
- b) Universal indexation

To rehearse the reason these are essential to scholarly activity : they supply objectivity of the studied object, a pompous term I just invented to say we can verify that we are all talking about the same thing, at least as far as each others' published ideas are concerned. If the only function of a journal was to disseminate, that would be the end of it. However, it isn't. Paul Malatesta of the Journal of Financial and Quantitative Economics listed four functions of the traditional journal:

- a) Archiving
- b) Dissemination

- c) Authentication
- d) Refereeing Services

The net is eroding the first two functions. What of the third and fourth? In my view, the most important consequence of the 'publishing revolution' is its impact on the actual process of journal production, from which the technical change is, as it were, peeling away successive layers of merely material production (printing, mailing, etc) to reveal the intellectual production process underneath.

THE MEANING OF QUALITY

The meaning which Reni Schultz attached to authentication, if I heard him right, was more extensive than textual integrity. What he meant was a guarantee of quality. By consuming a journal, the reader can limit the scope of reading to the essential. A journal is a human search engine.

Two functions appear to coincide. On the one hand, the sheer size and availability of the internet creates the well-known phenomenon of information overload. The proliferation of netcasting, search engines, gateways and the like are the proof that what every modern surfer needs is a method of hitting the big waves: a selection process. On the other hand, the journals offer their selection procedures as the answer to the surfers' prayer. Quality is becoming identified with selection.

The problem is now straightforward: there is not one single standard of quality, but four:

- a) the requirement of the originator
- b) the requirements of the referees and editors
- c) the requirements of the readers
- d) the requirements of science

These may turn out to be the same but there is no reason a priori to suppose that they will be. At present, I would argue that they certainly are not.

THE EDITORIAL PROCESS: A PROCESS OF SELECTION OR A PROCESS OF REJECTION?

A journal is on the face of it nothing more than a collection of related materials gathered together for the purpose of indexation and distribution. But as dissemination becomes virtually costless, there are no space constraints to consider; moreover automatic indexation, search engines and gateways remove the need to gather like articles in one physical location. If copyright is set aside, a journal in its essential form is no more than a timely anthology. The wan light of twenty-first century technology is melting away the excuses for current practice, leaving a question hanging like an icicle in an early thaw: what are editors, reviewers and publishers for?

Ostensibly they have two jobs: to help authors make a good product, and help readers choose it. In practice, if we study the actual labour process of reviewing and editing a journal article, we find its function is selection. In the context of the printed medium this has specific and restricted, though tacit meaning. Paper publishing leads to a single product and therefore a single decision: in or out. As Spinoza informs us, determination is negation, and a journal is defined not by what it puts in, but by what it leaves out. Rejection is the principal form in which the selection principle operates in the journal format. Quality is defined in practice negatively, not by what appears but by what does not.

This principle is no longer technically justified. If the purpose of selection is to assist the reader in rapid access, and if the cost of reproduction is marginally zero, then there is no commercial or technical reason to deprive the reader of access to work that has not been selected. The act of selection should be a mere act of indexation along with many others. Indeed printed journals of high quality already provide an indexation service without judgment of quality, for example the Journal of Economic Literature. Any journal could discharge all functions listed above without preventing authors from disseminating, or readers from finding, material other than that awarded seals of approval.

Indeed electronic distribution opens up a much wider range of possibilities than mere binary choice; for example a range or gradation of materials, multi-dimensional ratings applying a number of independent criteria (accessibility, relevance, originality, consistency, etc) or even, heaven forbid, a range of reviewer evaluations. To put it another way, there is no sound reason for a primary focus on rejection. If rejection persists, therefore, it must stem from some motive other than the preservation of quality.

QUIS CUSTODIET IPSUS CUSTODES?

The recent situation where (yet another) Nobel Prize has been awarded to authors of a rejected piece is only another nail in the coffin of an idea for which I can find no serious empirical evidence: the notion that the editorial and reviewing process of the contemporary economics journal plays any significant role in guaranteeing objectively-ascertainable quality.

Certainly there is no a priori reason to suppose that mere acts of rejection constitute guarantees of quality, or we would have to cast the Spanish Inquisition as the most scrupulous editorial board in history. Yet so far, migration to the electronic format has produced no significant modification of the rejection principle among the established journals. What, then, is its actual function? It would be an exaggeration to accuse economics of illiberalism. More schools of economic thought flourish than angels on a mediaeval pin. Nor can it be accused of lacking procedures to verify their output.

The basic point is that economics, in contrast with most other sciences, is almost entirely free of the annoying encumbrance of independent scrutiny. An unqualified person can check a medical result: they can see if the patient dies. We can see if houses stand up, planes fly, or clothing stays intact, and judge - at least indirectly - the theories of the physicists, chemists and biologists upon which these miracles depend. We can even point telescopes at the skies to check out the astronomers, as Galileo maintained to his cost.

But economics is loud to the point of stridency in its insistence that the only people competent to judge economists are other economists. It has an internal process of confrontation and selection, it is said, which will cast out the bad and elevate the good. That it has an internal process of selection, and that it involves much conflict, is beyond doubt. So do religions. The problem is, what guarantee can we offer that the governing law of this process replaces bad with good? Indeed, the quite rapid alternation of rival schools of thought in economics must lead to the opposite conclusion. If Keynes was wrong, how did the profession come to adopt his views? If right, how did it abandon him?

GRESHAM'S LAW OF PUBLICATION

Since ideas proliferate in economics, and since there is competition between them, it is fruitful to examine how this competition is organised.

THE JOURNAL AS UNIT OF CLASSIFICATION

A clue is offered by the instructions to reviewers and authors of most economics journals. The most striking fact is the near-total absence of objective criteria. The writer is abjured to submit so many words, double-space the copy, abstain from mathematics, to be clear, and so on. But these purely technical requirements have no bearing on the content of the work. Every author knows that the most accurate definition of what an economics submission should contain, is that it should appeal to the reviewers. Perhaps in addition it should cite and relate to recently-published work, particularly work likely to bear reviewers' names. In other words the principal criteria - if for no other reason than the lack of objective alternatives - are in practice subjective. What we can be assured of, when we read a journal, is the most coherent and forceful presentation of the general opinions of its editors and reviewers, and an exploration of their conclusions.

There is no independent guarantee that either the articles or the opinions have any other desirable qualities. Each journal presents to its readership, generally quite accurately, the fruits of the theoretical

schools of opinion which find favour with its editors and reviewers. Insofar as dissent is presented, journals do not exactly open their pages to their most forthright and effective opponents. They stage the debates they like to have; that is to say, they arrogate to themselves the luxury of choosing their own critics.

Thus the actual role of the present journal is classification. This is the only coherent basis for the rejection principle. It is because the function of a journal is to define which material conforms to definite schools of thought that it has to reject material that does not conform. This is not at all confined to mainstream journals. The general practice in economics, for anyone with a distinctive view, is either to establish a journal which represents that view or to take over one that already exists. The model of confrontation of ideas in economics is by and large a model of a conflict of schools of thought, organised by platforms. This is even advocated; the competitive process between schools, it is argued, is in itself the best guarantee of scientific merit. At the level of the winners, however the line is blurred and competitive success is defined as legitimate authority: in short, proof of success.

THE COMPETITIVE SELECTION PRINCIPLE IN ECONOMICS

But the outcome of any competitive process depends on the selection principle which elevates one above another. Socrates was forced to drink hemlock; however this competitive failure is not generally accepted as evidence against the quality of his views. We thus return to our original four conflicting selection criteria. Does the competitive selection principle in economics conform to the requirements of the readers, the authors, the editors or the truth?

Truth does not seem to play a particularly decisive role. Lant Pritchett (1997:14), a senior World Bank economist, recently concluded that convergence does not happen. Since 1870 the gap between a small group of rich countries and a much larger group of poor countries, has systematically widened. As for recent policy, in the author's words:

“From 1980-1994, growth per capita GDP averaged 1.5 per cent in the advanced countries and 0.34 percent in the less developed countries. There has been no acceleration of growth in most poor countries, either absolutely or relatively, and there is no obvious reversal in divergence...taken together, these findings imply that almost nothing that is true about the growth rates of advanced countries is true of the developing countries, either individually or on average.”

The data used to draw this conclusion has been available for at least fifteen years. Convergence is either a stated assertion, or a theoretical conclusion, of most modern mainstream theory. The policy advice given to most of the world for the last fifteen years is based on this notion, or at least makes no sense without it. Whatever principle of selection promoted these ideas, it was clearly not conformity to the observable facts.

Moreover, economic theory itself predicts a different outcome. The competitive process in economics, like any other governed by money, elevates the suppliers of products who meet the requirements of the suppliers of money. The largest market for economics, above all its policy output, is provided by banks and governments; a second distinctive feature of economics in comparison with other sciences. Economists as policy advisors are not in general hired, as are scientists, to furnish objectively optimal or independently-verifiable results but to implement and rationalise exogenous policy goals defined by private or political interests. There is no reason to suppose that either a bank or a government will pay large amounts of money for advice it does not want. The theories elevated by this competitive process are therefore the theories that best suit prevailing pecuniary interests.

A USER-CENTRED VIEW

The web offers an alternative dissemination model because it removes the technical basis for the rejection principle. In the first place this means that if the principle is maintained, then it needs a fresh justification. But in the second place it suggests there is a great deal of space for creative innovation between the two poles now established, namely instant but unrestrictive publication on the one hand,

and a mere electronic reproduction of existing practice on the others. This opens up several questions of which I want to focus on two:

- (1) *are there genuinely objective guarantees of quality?*
- (2) *could alternative dissemination models overcome the defects of the present system?*

In my view the two questions are entirely linked. The defect of the present dissemination model is that despite the apparent liberalism of the profession,

- (a) *it denies the public the opportunity of independent verification;*
- (b) *it operates to impose conformity;*
- (c) *the conformity it imposes is determined by pecuniary or political interests.*

I start from the unusual hypothesis that it is probably impossible to transcend the influence of private and political interest. Objective truth in economics is therefore unattainable by the profession itself: it cannot deliver a uniform view that simultaneously pays its bills and meets the requirements of science. This does not mean that no truth can be found in economics; just that the internal mechanisms of the economics profession will not in general promote it. Positive economics is a logical contradiction in se.

Viewed in this light, the most important step is to abandon the pretence of authority. If economics ceased pretending to purvey objective truth, and contented itself with the status something like the liberal arts, this would probably benefit the general public and would by no means damage economics.

This does not mean abandoning the notion of economic science: merely the pretence that the existing profession does it. The principle of economic dissemination which I would suggest is therefore that of independent verification, underlying which is the heretical idea that the public might be a better judge of economics than the economists, if only the economists recognised their job as helping the public to decide for itself instead of deciding on their behalf.

The objective of dissemination would then be, in every way possible, to facilitate public access to the decision-making and discussion processes of economics.

In the first place this demands universal pluralism; The function of dissemination is to furnish the general public with all views relevant to the matters it seeks to study. This does not conflict with a review process that selects for quality, nor with a review process that classifies by point of view. It conflicts only with the rejection of material on the basis of whether or not the reviewers agree with it, and in general with the use of rejection as a classificatory device.

In the second place it demands critical engagement; the place for debates is not in the contest between journals but in the discussion within each one. Instead of rejecting material that does not conform to editorial predilections, the onus would be on contributors to address the views they don't agree with. Otherwise, it cannot be said that they have given a rounded consideration to the question they are trying to study, and most importantly, the readership is denied access to the alternatives. Right of response is a further safeguard in this same framework.

Far from signifying the end of quality control, this dissemination model imposes standards of quality which may be substantially higher than those at present in operation. It suggests, for example, criteria such as the following:

DEMONSTRATION BY EVIDENCE, NOT AUTHORITY

'As noted by Fama and French' is not a proof that something is true; but a piece of information that Fama and French said the same thing first. If substituted for evidence, it is an *ipse dixit* demonstration; one which asks the reader to accept the views of an authority as proof of truth.

CORRECT ATTRIBUTION IN INDIRECT CITATION

'Keynesian theory defines equilibrium in the goods and money markets as the intersection of the IS and LM curves' is false. This is Hick's presentation of Keynes's theory. 'In Marx's theory value is given by vertically-integrated labour coefficients' is false. This is the interpretation of Marx's theory proposed by Linear Production Theory. Unless the reader can distinguish the real source of a theoretical assertion, it is impossible for her or him either to trace the assumptions that genuinely lie behind it, or to make an informed judgement on the author to whom the theory is attributed.

RULES OF EVIDENCE IN INDIRECT CITATION

The rules of evidence apply not just to economic facts but economic theories. If the claim is made that an author said something or a theory claims something, then the reader must be able to find where it is said, and the author must produce substantive evidence that the attributed interpretation is consistent with the cited text.

SPECIFICATION OF THE IMPLICIT BASIS FOR FACTUAL CLAIMS

'Real UK output in 1994 was £570,722' is a false claim. 'Output as measured by the UK NIAs, deflated using the GDP deflator, was £570,722' specifies the theoretical framework that produced the claim, permits the reader to trace the assertion back to its source, and allows the reader to judge how the conclusions of the paper would change if a different, alternative measure of output were used.

In addition to these specifically 'critical pluralist' requirements, there are perfectly sensible and un-objectionable criteria which would apply in any dissemination model but are rarely made explicit, eg:

TRANSPARENCY

The expertise assumed should be the minimum necessary to the argument. Whilst we cannot impose the principle that an unqualified person can immediately understand an economic argument, we can first of all take general responsibility for providing them with the means to do so, and second abandon the pretence that our expertise necessarily conveys any superior claim to truth.

ORIGINALITY

It is reasonable to inform the reader that work which merely repeats earlier conclusions or arguments has lower relevance than the original, exception being made of review or summary articles

RELEVANCE TO ANNOUNCED TOPIC

Journals have every right to specialise: but specialisation is primarily a question of field of interest, not theoretical framework.

CONCLUSION

Is there any reasonable prospect that such a dissemination model could take root? I think that it will be earnestly resisted. However, precisely because of the dramatic changes in the technical environment, it is for the first time a practical possibility and there is room for creative experimentation. Whether the practical migrates to the actual, is a matter for human will and commitment; the competitive selection principles that operate in a cost-free environment, however, are likely to pose a substantively greater challenge to orthodoxy than has been seen in the past. The evidence from the general public discontent with economics, and from the drive from within the profession for new modes of scholarship, suggests that the old habits have no reason to expect an unchallenged ride into the Twenty-First Century.

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LINKS (LAST VERIFIED JULY 1997)

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Social Science Research Network www.ssrn.com

Journal of Finance www.cob.ohio-state.edu/~fin/journal/jof.htm

Journal of Financial and Quantitative Economics weber.u.washington.edu/~jfq

NOTES

1: 'Stuff', as defined by the Iomega corporation, means 'anything stored on an electronic medium'.

2: Needless to say, like judges they are also finding the product doesn't always live up to the prototype.