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ECONOMICS

RESEARCH AND THE ACADEMIC: A TALE OF TWO CULTURES

by

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DISCUSSION PAPER 10.01

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Preface

I first heard of David Hendry in 1977 when I was teaching on a sabbatical at Berkeley. Berkeley decided it needed a new Professor of Econometrics and wanted to appoint the most outstanding young person in the world, in this field. That person, they decided was David Hendry. Unfortunately for Berkeley, David's Alma Mater, the London School of Economics had already recognised the talent of this young man and had him made a full professor at the age of 33. This in itself was an outstanding achievement when one considers that David only started to study Economics when he was 22. Eleven years later, he was a full professor at the top school of economics in the world as LSE was then.

In 1982, Oxford succeeded where Berkeley had failed and managed to entice David Hendry away from LSE. At the time, Oxford was in a spot of bother with respect to theoretical econometrics. The rankings of that period indicated that the University of Western Australia was ranked 62 ahead of Oxford which was ranked 80. However, Oxford knew what they were doing. Under David's leadership, Oxford has reasserted itself as the top department in the United Kingdom, not only in econometrics but also in economics, coming ahead of LSE for the first time in the latest rankings.

David Hendry has been a prolific researcher. He has made significant contributions to econometric theory, computing in econometrics, econometric methodology, the history of econometric thought, model selection and forecasting. He has published by himself and with other authors, some 19 books and over 200 articles. He is amongst the most cited economists in the world. He has 8 honorary doctorates, several university medals and the Guy Medal of the Royal Statistical Society. He is a fellow of many academic bodies including the Royal Society of Edinburgh, the International Institute of Forecasters, the Journal of Econometrics, the American Economic Association, the American Academy of Arts & Science, the British Academy and the Econometrics Society. Last year he was knighted for his services to social sciences.

In summary, Professor Hendry has achieved in his research what very few of us can ever aspire to. It is of interest then to hear David's views on research and the academic. The University of Western Australia was recently very fortunate to hear these views at a talk David gave to our senior students whilst he was visiting us as the invited Bateman Lecturer. This talk contained words of wisdom from a very experienced researcher, words that I wished I had heard as a young academic trying to establish myself as a researcher.

Professor Hendry has kindly agreed to allow us to publish this talk in our working paper series under the title 'Research and the Academic: A Tale of Two Cultures'. I heartily recommend this paper to you.

Darrell Turkington.

A health warning attaches to reading this tale, which naturally parodies the approach of which I am not enamoured.

The `Get Ahead' Model

1] **Choosing a subject to research**

Any fad or fashion will do, if it is likely to last a couple more years and is a hot topic for publication

2] **Topic**

Within that fashionable field, mainly develop theory, preferably with a catchy title, some abstruse maths (explained as little as possible, so it looks `smart'), possibly with a couple of the key steps missing in reported proofs to make referees feel inferior and worried about their reputation that they cannot check the claims without a lot of time input. Minimise novelty—just enough so your contribution is not totally redundant.

If you should dare to stray from theory, in Hendry (1987), I proposed the four `golden prescriptions' of applied econometrics:

(i) *think brilliantly*: if you think of the right answer before modelling, then the empirical results will be optimal and, of course, confirm your brilliance; failing that—

(ii) *be infinitely creative*: if you do not think of the correct model before commencing, the next best is to think of it as you proceed; failing that—

(iii) *be outstandingly lucky*: if you do not think of the `true model' before starting nor discover it *en route*, then luckily stumbling over it before completing the study is the final sufficient condition. This may be the most practical of these suggestions.

Failing this last prescription:

(iv) stick to doing theory.

3] **Journal submission**

Choose the journal with the highest impact factor, as mistakes are made and you may be lucky: see Heintzelman and Nocetti (2009), but compare Oswald (2007). Either way, make sure you cite work by the editor and likely referees--often and favourably.

Check up on which conferences he/she is attending, go and chat up editor (buy them drinks etc.)

Shorten your paper drastically during revision to ensure it gets through as short and zippy. One route is to split the original into two, three, or even four papers, parcelling up your ideas as finely as possible, all short and pithy, as small steps are least likely to fall foul of misunderstanding what you are doing or how it is done.

4] **Sell your work**

All these papers can now cite each other, but more importantly, get all your friends and colleagues to cite all of them every time they write anything remotely related, and in all possible outlets (seminars, conferences, publications etc.).

Refer to own work as `fundamental', `already a classic', preferably naming the theorems after yourself (Blog policy or Smith's curve etc.).

Assert that all known empirical evidence supports your conclusions—almost no one will bother to check, and critical comments never get published anyway (Editor's letters in economics always assert that they seek positive contributions, not negative: how many destructive pieces have you seen in print relative to ones claiming advances? Much empirical research cannot even be reproduced despite closely following what a paper claims was done. Draw wide-ranging and dramatic conclusions at the end, however flimsy their basis.

Chase every author in the same area who does not cite every one of your papers favourably, with aggressive letters, possibly even threatening plagiarism accusations if they do not acknowledge your priority.

Endlessly tour US universities presenting seminars—few researchers bother to just read journals to find interesting or relevant papers, so this is the only sure way to bring your important work to their attention, after which, they tell each other.

5] **Refereeing**

Only agree to referee rival papers so you can then reject them: fail to respond to other requests to slow down the processing and appearance in print of that author's work. You need not produce good reasons for rejection: editors have so many papers crossing their desks, the simple assertion that the work is not novel, not scholarly, or not correct etc. will suffice. I know of even less well documented reports that have been sent out, including one that read: `There are good dogs, guide dogs and hot dogs, and this paper is a hot dog.' That was the sum total of the report…..other than the editor's rejection letter.

6] **Editing**

Grab every opportunity to get control of a journal, so you can select only papers favourable to, and citing, your work. Chase `stars' for their latest papers promising speedy publication, then publish your own work in the same issue to bask in their kudos.

Sit on papers that criticise anything published in your journal—then reject them only after interest in that topic has faded so they have no hope of publishing their critique anywhere else.

7] **Colleagues**

Dump all possible administration on them, together with any avoidable marking, examining, or teaching etc. Junior colleagues make the best targets, the more defenceless the better: remember, doing so frees time for doing, or selling, your research, and theirs is worthless anyway.

8] **Students**

Only take on those who are willing to work favourably on your research themes, and also agree to all their research being joint publications with you as lead author. You can justify this on the grounds that they will have a better publication chance etc.

9] **Promotion**

Apply everywhere that better jobs appear—like the highest impact journals, mistakes are often made and you may get an offer. If so, demand salary raises and reduced workloads at your present department after every offer: regularly threaten to leave if you are not better supported by them, when you have another possibility in the bag.

10] **Funding**

Pile in the grant applications for every facet of your research, preferably have juniors writing the proposal drafts, with you as principal investigator. And don't forget to fawn to the chairs of grant awarding panels as with editors above.

These 10 steps will ensure you have a successful career, ending famous and well paid. But there is another route….

The Community of Scholars Model

1] **Choosing a subject to research**

Pick a substantive issue of concern that really interests you: poverty, inequality, unemployment, health, inflation, mergers, asset prices—whatever grabs you.

2] **Topic**

Explore every angle, theory, evidence, policy, possibly even forecasting: if needed, develop new theory, new methods, new approaches, new computing, etc. Possibly team up with some one who has complementary skills to your own to produce a high quality overall contribution.

Go for novelty—but not too much so that referees doubt your sanity or cannot understand what you are doing in the context of what they know. If it is a huge leap forward, first `fill in' intermediate ground that bridges from where the frontier lies to where you will move it: think Barry Marshall. Even so, despite such a strategy, what you thought was well established, may be too new (i.e., unknown to) evaluators. In a recent grant application rejection, I received:

``One is not totally convinced that deterministic breaks is such a useful area of research in macro-econometrics.''

I have published extensively on them since 1992 as the only possible explanation for forecast failure in economics—or any other discipline: see the monographs by Clements and Hendry (1998,1999) (Clements and Hendry, 2008, provide a non-technical discussion) Yet it is clear the referee had no idea of the advances the analysis of location shifts had led to for macro-

economic forecasting (see e.g, Hendry, 2006), and modelling (see Johansen and Nielsen, 2009). Søren Johansen is currently developing new methods of robust statistics using that approach, and my *Bateman Lecture* shows how it can greatly improve econometric modelling when there are multiple breaks in the data. Unfortunately, however, that aspect was `too novel' for the macro-economist in question.

3] **Journal submission**

Pick the journal with the most relevant readership.

But be prepared for rebuffs—they happen to everyone. As a doctoral student at LSE, I was incredulous when *Econometrica* rejected a paper by Denis Sargan.

Many of my own papers have been rejected by some journal, sometimes well taken, others without good grounds—basically referees disliked the approach so tried to find justifications for their views—and occasionally invited by the same journal (but a different editor!) for a special issue on that topic.

4] **Selling your ideas**

Prepare papers very carefully for publication and presentations: have friends read them for coherence, correctness etc. Check all results can be replicated at least by yourself, keeping a log book of precisely what was done, what data were used, their sources etc.

Ensure tables contain the right numbers, and match the graphs and the text discussion.

5] **Refereeing**

This issue has come up several times above—never write reports like those already mentioned. Be critical, but be fair, objective and constructive: cite chapter and verse for every assertion—where it appeared, or lay out any counter-examples clearly, and check you are correct.

6] **Editing**

Do not just act as a post-box when editing a journal. Read every paper that gets submitted, sketch your own report, and preferably check every the empirical model (as I did using *PcGive* for the *Oxford Bulletin* from about 1986 to 1992). Bin reports like those above, and add your own as if an `anonymous referee'. You will learn a huge amount by such reading, and stimulate your own ideas. I handled around 150 papers a year for a decade as econometrics editor for the *Review of Economic Studies* then the *Economic Journal*, and have no regrets for all that reading even if many of the papers seemed unlikely to be published anywhere.

7] **Colleagues**

Mentor and be mentored: protect more junior colleagues from overload and unfair decisions. Help their research as a co-author if that is what is needed etc.

8] **Students**

Advise all students who ask for help, even if it is only to point them towards a better source of help. As noted, assist them to achieve better publication chances.

9] **Promotion**

Take promotion as it comes from the outputs you produce, while still sensibly judging the timing and location for your future: talent will out, and offers will appear if your research merits.

10] **Funding**

Certainly apply for support if the topic needs and deserves extra resources. As with papers, be prepared for rejection: often on flimsy and unsubstantiated grounds unfortunately. Four out of my last six grant applications since 1999 have been rejected, either because referees falsely claimed either:

the research could not be done (one was for a study of forecasting breaks: with Jennifer Castle and Nicholas Fawcett, we now have a paper forthcoming in *Journal of Econometrics* on forecasting during breaks), or

was mathematically impossible (that was for a study of fitting models when there were more candidate variables than observations, and in *Computational Statistics* with Soren Johansen and Carlos Santos, we have published a proof that it can be done: my Bateman lecture showed a live demonstration of doing so when there are 650 candidates, 4 of which matter and 135 observations—yet found the data generation process; Jennifer Castle and I had also shown how to test for non-linearity despite vastly more candidate non-linear functions than observations), or

because the work had already been done (but no record of where—that was by an ESRC referee for a new approach to testing the invariance of parameters in models with future variables as proxies for expectations, building on a recently developed test for super exogeneity with Carlos Santos), or

because it would merely be `somewhat incremental and not ground breaking (although I believe it is quite fundamental and certainly not known that most mathematical derivations in inter-temporal optimization are invalid in economics because distributions shift, as we show in joint research with Grayham Mizon). I will not go on…..but:

11] **Never give up!**

Despite all my recent work being regarded as uninteresting or impossible, I knew neither was true, so I persevered—as should you in similar circumstances. This year, the failure of governmental statistical agencies to measure their economies accurately during the financial crisis led us to apply our new approach to `nowcasting'. We developed a powerful new framework for doing so in one paper and applied it in another. In April and May, we sent drafts to some researchers in the area, one of whom happened to be editing a journal special issue and requested the theory paper if we were willing to let him have it, which we were. The applied paper was written for a special issue which the *National Institute Economic*

Review was doing on nowcasting, since their own methods were not working well. In September, we received the page proofs and referee reports simultaneously and had to do both at once—and that paper appeared in October.

Guess what?—they both depended totally on the research that the ERC with the above referee described as ``**One is not totally convinced that deterministic breaks is such a useful area of research in macro-econometrics.''**

Academic life mixes the sublime and the ridiculous: all too many economists are relatively ignorant of developments outside their own narrow speciality, yet fully convinced that are omniscient. Some academics are out for themselves, who judge academia as being the lifetime income maximizing arena for them. Nevertheless, there are also many sensible and committed academics, trying to discover how economies function, to help them work better. I hope you will be in the game for the intellectual excitement of discovering something never previously known—and experience the real and lasting pleasure that can bring.

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