This is a slightly revised version of an article in *The American Economist,* Spring 1994.
It will be published in *Passion and Craft: How Economists Work,* ed. Michael Szenberg,
University of Michigan Press, 1998.

MY SYSTEM OF WORK (NOT!)

by

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Among the signals of approaching senility, few can be clearer than being asked to write an article on one's methods of work. The profession's implied judgment is that one's time is better spent giving helpful tips to younger researchers than doing new work oneself. However, of all the lessons I have learnt during a quarter century of research, the one I have found most valuable is always to work as if one were still twenty-three. From such a young perspective, I find it difficult to give advice to anyone. The reason why I agreed to write this piece will appear later. I hope readers will take it for what it is -- scattered and brash remarks of someone who pretends to have a perpetually juvenile mind, and not the distilled wisdom of a middle-aged has-been.

Writing such a piece poses a basic problem at any age. There are no sure-fire rules for doing good research, and no routes that clearly lead to failure. Ask any six economists and you will get six dozen recipes for success. Each of the six will flatly contradict one or more of the others. And all of them may be right -- for some readers and at some times. So you should take all such suggestions with skepticism. Give a good try to any that appeal to you, but don't fear to disregard all the rest.

There is also the problem of judging the target audience. What works for academic research is not best suited for policy or consulting research, and the right strategy for advancing the frontier of research is not the same as that for later work of consolidation or synthesis. I will assume that the readers of these essays are actual or potential academic economists with high ambition; they aim to excel in whatever area of research they choose, and are looking for good habits to speed their journey. In short, I will take it that the readers hope for success at the top levels of the research community.

These general difficulties are compounded by my own limitations. First, I am a theorist, albeit of a relatively applied kind. That is to say, I build mathematical models to address specific issues and contexts of economic interest, rather than abstract systems of general and overarching significance. And I try to get specific results from the models (What cause has what effect?) rather than prove theorems (Does equilibrium exist, and is it unique?). What works for me is governed by what I am trying to accomplish; the same approaches and techniques may not suit the more abstract theorists or the empirical economists.

My second limitation is even more severe. I have always worked on the next problem that grabbed my interest, and tackled it using whatever approaches and techniques seemed suitable, never giving a thought to how it might fit into an overall world-view or methodology. It is hard for me to evaluate such an unsystematic and unphilosophical approach, and even harder to give any advice based on it. But I shall try.

MY OWN EXPERIENCE OF RESEARCH

Readers of these essays are surely not too interested in the drab and dreary lives of economists for their own sake; they are in search of research methods they can emulate. But one's advice is colored by one's experiences, and I owe the reader a brief statement of the reasons for my biases.

Most of us spend hours discussing at which restaurant to have dinner, and make decisions like what career to pursue and whom to marry instinctively in an instant. So it was with my entry into economics. I got my first degree in mathematics, and had just started a Master's in Operations Research, when I was converted to economics by a chance conversation with Frank Fisher. He should get all the credit, or the blame.

I started on my research career in 1968, at a time of turmoil in the academic world of Europe and the US. The prevailing atmosphere was decidedly left-wing and anti-establishment, and research was almost required to be "relevant." Most theorists were affected by this atmosphere, and I was no exception. Important topics included problems of less developed countries, urban problems, and environmental problems.[1] I dabbled in all of these.

Looking back on those years, much of the "relevant" research in economics left little lasting mark on the subject. Problems of less developed countries and urban areas proved so political that good economic advice would have achieved nothing even if we had been able to give it. No, the topics that proved to have lasting value in economics were quite different, for example the theory of rational expectations, the role of information and incentives, and later in this period, game theory. In the early 1970s much of this work seemed abstract and irrelevant, and would have been called "politically incorrect" had that phrase existed in those days.

My own work also met the same fate. My "relevant" work is mostly and justly forgotten.[2] What has come to be regarded as a success -- for example the theory of product diversity in monopolistic competition, the theory of entry-deterrence in oligopoly, a reformulation of the theory of international trade, and some recent work on irreversible investment -- was not motivated by any sense of relevance, or any high-minded desire to do good. It is almost embarrassing to think back on how I came to work on some of these topics.

The book on international trade grew out of a lunchtime conversation with Victor Norman. He knew a fair bit about the subject, I knew almost nothing, but both of us knew a lot of duality theory and had a sense it might be useful in simplifying some of trade theory. We decided to learn by doing, and spent so long at it that we had to write a book. As we went along, more than half of the time we found that someone else had been there before. But it was much more fun to do it ourselves.

The model of entry deterrence in oligopoly came from an uneasy feeling that the accepted theories -- Bain-Sylos, and even Spence -- were not doing it right. At the time subgame perfectness had just made its appearance in the game theory literature, but I was in rural England, far removed from the centers of game theory like Stanford, and had never heard of the concept. So I had to work it out from scratch, and that took a surprisingly long time. The breakthrough came when I had by mistake gone to the airport far too early, and had to kill a couple of hours. Once the right idea came, everything worked out really fast. Since then I have often deliberately got to airports too early, but alas, with no similar success.

Much of this work was received favorably by some existing specialists in the fields, but got puzzled and negative reactions from others: "Optimum product diversity? Surely the market finds the optimum. Monopolistic competition? That's a dead end." "Duality? What's wrong with the way we have always done things?" For years Ron Jones dismissively referred to the group working on oligopoly in international trade as "Imperfect competitors." By now I expect a "long and variable lag" between the time I work on something and the time enough others find interest or use in it. But I have learnt the importance of trying to shorten the lag by conveying my ideas simply and clearly.

In this I am but one minor member of an extremely distinguished group. For example, William Sharpe struggled to get his now-famous CAPM paper published, and recalls the reaction even after its appearance in print: "I knew ... [t]he phone would start ringing any moment. After one year, total silence. Nobody cared. It took quite a while."[3]

As you can see, my approach to research is too opportunistic to have a constant direction. But taking stock of it for the purpose of writing this piece, I could see a recurrent if not dominant theme. Scale economies and sunk costs keep appearing in my papers with great regularity. Imperfect competition is the norm, and market equilibria are not socially optimal (but government interventions have more subtle effects than naive intuition would suggest, and may actually make matters worse). And therein lies an irony. The left-wing critics of the late 1960s and 1970s, who influenced many youngsters when I started out, reserved their strongest criticism for the perfectly competitive equilibrium of the neoclassical system. Of course they did little by the way of offering a viable alternative. It has been the unexciting incremental work, to which I have contributed a little, that has built into a major shift in our understanding of how the economic system operates when the assumptions of neoclassical economics fail.

That is enough autobiography, and more than enough self-justification. For the rest of the article, I shall elaborate and paraphrase my experience into statements of what I have found to be good work habits. I will find it convenient to express these as items of advice, but let me repeat my earlier caution to the readers -- be skeptical, pick what you think might suit you, and discard the rest.

ON CHOICE OF TOPICS

• My most important advice here is stark and politically very incorrect: Don't give too much weight to the social importance of the issue; instead, do what captures your intellectual interest and creative imagination. This is not to deny the importance of paying attention to the real world. Nor is it to say that abstract theory is necessarily more valuable than applied work. Nothing could be farther from the truth. But I do believe that mere relevance of an issue will not guarantee good research unless you have a genuine drive to work on it. If not, leave it to someone else. Good work on an apparently unimportant problem will have more long-run value than mediocre work on one of greater intrinsic importance. And one's judgment of importance can always be wrong; concepts of relevance can change over time.

Of course if you find genuine passion for an issue of real social importance, count yourself twice blessed.

• How can you know if you do have the real drive to do research on a particular topic? Perhaps the surest sign is that the work is fun. Richard Feynman, in a wonderful collection of anecdotes from his life ("not an autobiography," he insisted) gives a classic example of this.[4] Some students in the cafeteria were tossing around a dinner plate like a frisbee. It was wobbling, and the red Cornell medallion on the plate seemed to be revolving faster than the wobble. Feynman set out to calculate the relation between the two rates and found a remarkably simple two-to-one ratio. He showed his work to a senior colleague, Hans Bethe.

> `He says, "Feynman, that's pretty interesting, but what's the importance of it? Why are you doing it?"
>
> "There's no importance whatsoever. I'm just doing it for the fun of it." ... And before I knew it ... I was "playing" -- working, really. ... It was effortless.
>
> There was no importance to what I was doing, but ultimately there was. The diagrams and the whole business that I got the Nobel Prize for came from that piddling around with the wobbling plate.'

Feynman uses a very revealing word: "playing." If your work is as enjoyable to you as play, that is a good sign that the topic suits you.

Looking over what I have just said, I realize that I am advocating something very radical: not only a non-system, but also a non-system for non-work. But what did you expect from someone of twenty-three?

• Every bright student who passes his/her general examinations sets out to revolutionize the subject. But revolutions are not best made by setting out to make them. In Thomas Kuhn's terminology, scientific revolutions are the consequences of attempts to resolve anomalies that are observed in the course of normal science. And the best way to notice anomalies is to do normal research. Kuhn has explained the process brilliantly:[5]

> `Only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one. That is why I speak of an "essential tension" implicit in scientific research.
>
> `At least for the scientific community as a whole, work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved. How can this be so? I think it is because no other sort of work is nearly so well suited to isolate for continuing and concentrated attention those loci of trouble or causes of crises upon whose recognition the most fundamental advances in basic science depend. ... Though the ability to recognize trouble when confronted by it is surely a

> requisite for scientific advance, trouble must not be too easily recognized. The scientist requires a thoroughgoing commitment to the tradition with which, if he is fully successful, he will break.'

• Discover your best "distance." Some people are good sprinters in research. They can very quickly spot and make a neat point; they do this frequently, and in many different areas and issues. Hal Varian and Barry Nalebuff are two of the best sprinters I know. In the same metaphor, others are middle-distance runners. In fact most economists are at some point in this broad category. A few, for example Robert Lucas and James Mirrlees, are marathoners; they run only a small number of races, but those are epics, and they get the most (and fully deserved) awe and respect. In contrast, the profession seems to undervalue sprinters. But each kind of work has its own value, and the different types are complements in the overall scheme of things. Progress of the subject as a whole is a relay race, where different stretches are of different lengths and are optimally run by different people. Find out where your comparative advantage lies.

• Many ideas, and techniques for theorizing, will come to you by accident. But don't wait for such accidents to happen; facilitate them. Always be on the look-out for examples, questions etc that relate to what you are doing, or something you worked on once but set aside. A newspaper article or a current affairs program or a chance remark by a colleague can get you started. A totally unrelated theoretical article may use a technique that proves useful for your problem, and gets you re-started on something that had stalled. Seemingly far-fetched analogies turn out to have some deep basis. Therefore you should keep all of your work in your semi-active memory all of the time -- the work in progress as well as that not making progress.

• Learn to manage your time. When asked to contribute to a collective volume, or present a paper at a conference, unless the assigned topic happens to coincide *exactly* with your interests, follow the Nancy Reagan strategy: "Just say no." You will invariably find the demands of such assignments crowding out the time that you could have spent on ideas of much greater intellectual interest to you. (In fact I took on the task of writing this article just to get that out.) Stick to what you would best like to do; if you are successful, some years later people will be holding conferences on your topic. (Of course by then you will be interested in something else.) In the meantime, you will have much more fun working on something that you really like. And even the material rewards of a successful frontier research article easily exceed the honoraria of ten conference articles of topical interest.

There are people who can turn a conference assignment into real research. Or to be accurate, there is one such person -- Paul Krugman. Unless you have that very rare skill, get your priorities straight.

ON HABITS OF WORK

• Management of your time is again of paramount importance. This is especially true when on occasion you are forced (or just irresistibly tempted) to violate the Nancy Reagan Strategy and take on a conference-type assignment. Then I recommend the Nike strategy: "Just do it." Don't procrastinate to the deadline. If you do, you will waste a great deal of time all the while, thinking about the assignment and its impending deadline. You will also expend a lot of mental energy feeling weighed down by the task. Much better to get it out of the way as quickly and effortlessly as possible, and get back to the real stuff.[6]

• On the other hand, when doing frontier research of real intellectual importance and challenge, do not be afraid to spend a lot of time thinking vaguely, or even "day-dreaming" around the subject. This time is not wasted. All the associations you ponder, and all the calculations you try for a few lines and abandon, will prove a useful input to the process that ultimately leads to the answer.

• Having posed the question and worked on it for a while, give the subconscious a chance. Perhaps the best advice on this comes from the mathematician J. E. Littlewood, in his lovely article, "The Mathematician's Art of Work."[7] He distinguishes four phases in creative work: preparation, incubation, illumination, and verification. "In preparation, [t]he essential problem has to be stripped of accidentals and brought clearly into view; all relevant knowledge surveyed; possible analogues pondered. It should be kept constantly before the mind during intervals of other work. ... Incubation is the work of the subconscious. ... Illumination, which can happen in a fraction of a second, ... almost always occurs when the mind is in a state of relaxation, and engaged lightly with ordinary matters." Littlewood recommends "the relaxed activity of shaving" as a fruitful time for illumination; I shudder to think how much *more* David Kreps, Paul Krugman, and Lars Svensson would have accomplished if they had known this.

• In our profession it is customary to stress the importance of economic intuition, and deride abstract or formal thinking. I have found this to be right on balance, but not to the point of dogma. People and problems vary in the kind of thinking that suits them best. For example, it appears that

John von Neumann had a very abstract kind of mind. He once advised a co-worker: "Oh no, no, you are not seeing it. Your kind of visualizing mind is not right for seeing this. Think of it abstractly. What is happening [on a photograph of an explosion] is that the first differential coefficient vanishes identically, and that is why what becomes visible is the trace of the second differential coefficient." [8] How many of us, hearing such an explanation from a colleague or a student, would have admonished them to "be more intuitive?"

• Keep a "portfolio" of problems to work on. If you are not making progress on one, switch to another. You will not only diversify your risks, but also increase your chances of success on each, because your mind will stay fresher and you will feel less depressed about the lack of progress on one problem. But don't switch too rapidly; if a problem is at all challenging, less than a month's concentrated thinking about it may not be good enough.

• Joint research is becoming more common in economics, and that is a good thing. A good research collaborator is worth any number of casually interested readers of your papers. The close but sympathetic criticism at an early stage that comes from a fellow worker helps you avoid many blind alleys, or wrong tacks from which you might otherwise never recover. As Francis Crick put it, "The advantage of intellectual collaboration is that it helps jolt one out of false assumptions."[9] You and your ideal co-author will have enough overlap to give both a common frame of reference and language for thinking, but enough difference to generate real synergy and complementarity rather than mere duplication.

• Reserve your best and most alert period of the day for real research, and use your tired, dull or slack stretches for correspondence, meetings, administrative chores etc. Alas, this is often not possible. Keep in mind, too, the possibility that your best period changes with the seasons, age etc. I have heard Paul Samuelson claim that for most people a switch occurs at around 35 years of age: morning becomes a better time for research instead of late at night. (Here I mean physical age; of course your mental age should stay constant at 23.) My own experience confirms this.

• Continue revising your papers to improve them, but not forever. The Austrian capital theory that you learnt as a dry textbook model has practical application. Papers should be improved only to the point where the rate of improvement equals the rate of interest. The latter rate will vary over your life-cycle, but striving for absolute perfection is wrong for most people at most times. From a private perspective, it will delay the spread and impact of your work too much, and risk pre-emption. From a social perspective, public release of something that is less than perfect has value; it may be someone else's comparative advantage to contribute the next step of improvement.

• Read other people's papers either seriously, or not at all. When you read them seriously, read them as you read papers when you were a graduate student, checking all the details and questioning everything. This is a good way to get new research ideas of your own. I owe my own understanding of the importance of this principle to Richard Feynman. He describes how he came to discover the law of beta decay.[10]

> `At that particular time I was not really quite up to things. Everybody seemed to be smart, and I didn't feel I was keeping up. ... At one point there was a meeting in Rochester, ... and Lee was giving his paper on the violation of parity. ... I was staying with my sister in Syracuse. I brought the paper home and said to her, "I can't understand these things Lee and Yang are saying. It's all so complicated." "No," she said, "what you mean is not that you can't understand it, but that you didn't invent it. You didn't figure it out your own way, from hearing the clue. What you should do is imagine you're a student again, and take this paper upstairs, read every line of it, and check the equations. Then you'll understand it very easily." '

She was right. Not only did Feynman understand the paper, but he remembered something he had done a while ago, used that method to simplify Lee's solution, and forged ahead to develop the whole new theory.

Oddly enough, when I read this I was in a somewhat similar state of mind with regard to the literature on trade policy with asymmetric information, and the same recipe worked for me.[11]

ON WRITING

• My first suggestion is: Keep it simple. The temptation to show one's technical wizardry is overwhelming, particularly for the fresh Ph.D. Resist it. It will only make your paper less easy to read, and reduce its impact. If an idea can be conveyed in a simpler way, without spelling out every epsilon and delta, do so. Littlewood says of Jordan that if he wrote an article with only four symbols they would be called a, M'_3, ε_2, and $\Pi''_{1,2}$ instead of a, b, c, d; don't be like that.[12] If needed for completeness, put the more formal proof in an appendix. However, I find totally unacceptable the current and growing practice of many papers in economic theory, which merely state the results in the text without any explanation at all, and then relegate the proofs to an appendix.

I said earlier that pure economic intuition may or may not be the right way to *think* in research. Its importance increases when one *writes* research results, and even more when one *talks* about them, particularly if the intended audience is larger than that of specialists in a very narrow area. (Many fresh Ph.D.'s giving job talks do not realize the importance of a simple and intuitive exposition, and this costs them dearly.)

• My second suggestion is: Keep it short. In this I agree with Piet Hein, the Danish scientist turned poet who wrote aphoristic verses called Grooks. He preferred writers

`who find their writing such a chore
they only write what matters.'

But this seems a lost cause. Over the last two decades the average length of economics papers has increased quite a lot. Advances in word-processing technology have greatly reduced the cost of producing words, but not the cost of producing ideas, with the result economists should expect -- massive substitution.

My ideal is neatly captured in a question Frank Hahn posed to an author. As an editor of the *Review of Economic Studies*, Hahn asked the author to cut down his paper from 40 pages to its essential core of three pages. When the author wrote a long and indignant letter, Hahn responded in two sentences: "Crick and Watson described the structure of DNA in three pages. Kindly explain why your idea deserves more space." An ideal that, alas, neither I nor Frank Hahn nor anyone else seems to come close to.

• Listen to referees: Referees may be prejudiced, they may be hurried, but they are almost never stupid. If you are doing innovative work, be prepared to meet bias, and be prepared to meet careless dismissal. Give such reports due consideration -- even they may contain useful tips for revision -- but if you have basic confidence in what you are doing, press on. If you meet sheer incomprehension, however, take that as a sign that your writing has failed. Clarify, if necessary overhaul the whole notation of your formal model, and try new drafts on colleagues and students, until you communicate better. I come across many economists who constantly complain that "referees don't understand them." When I hear this, I think of Tom Lehrer's remark: "If a person can't communicate, the very least he can do is to shut up."

• There are conflicting considerations on how hard to sell your work. On the one hand, if you don't sell your own work, the chances are that no one else will. Littlewood has the mot juste once again:[13] "He that bloweth not his own trumpet, his trumpet shall not be blown." On the other hand, excessive claims about the importance of your work will get you a bad reputation in the profession, and will jeopardize the reception of your future work. I prefer to claim a little less for my work than I feel it deserves.

If you must exaggerate, do so in a skillful way. Joseph Schumpeter claimed that he set out to become the best horseman in Vienna, the best lover in Europe, and the best economist in the world, and had achieved two out of the three. This is brilliant exaggeration, reducing the risk of being found out – anyone who could personally assess Schumpeter's prowess in any one of the three things would give him the benefit of the doubt and assume that he had excelled in the *other* two.

A CONCLUDING WORD

I have saved for the end the most important lesson I have learned from my experience, and which I believe has very general validity. Maintain a youthful sense of freedom to choose problems and the directions of work on them. Imagine yourself at twenty-three, not yet labeled or confined to a particular "field," and not yet pressured to produce something quickly for the approaching tenure review. Try to preserve this mental frame in your research, even as your body, and the part of your mind dealing with other matters, continue to age and decay.

Unfortunately, in the US most academics do not regain this freedom until they are thirty-five, by which time it is too late for many of them to be twenty-three. Their research brain is beyond rejuvenation, and it is time for them to leave the research frontier and join the conference circuit or the policy community. My reaction as a theorist echoes what Clemenceau said on hearing that the famous pianist Paderewski had become the President of the newly founded Polish Republic: "What a come-down!"

NOTES

1. And for reasons that escape me, quite abstruse arguments in capital theory that acquired inexplicable ideological significance. But that fashion died, as it richly deserved to.

2. I wrote one paper in urban economics -- a model of the optimum size of a city trading off scale economies in production and congestion diseconomies in transport -- that achieved some success. I like to think that even now, when theoretical urban economists meet for a beer at a conference, someone might remark: "Wonder what became of that guy Dixit. He wrote one paper that wasn't bad, and was never heard from again. I guess some people just don't have staying power in research."

3. Quoted in Peter Bernstein, *Capital Ideas*, The Free Press, 1992, p. 199.

4. Richard Feynman, *"Surely You're Joking, Mr. Feynman!"*, New York: Norton, 1985, pp. 157-8.

5. Thomas S. Kuhn, *The Essential Tension: Selected Studies in Scientific Tradition and Change*, University of Chicago Press, 1977, pp. 227, 234-5.

6. I have to confess that I have not optimized my own time as I advise you to, and that I have too often violated both the Nancy Reagan Strategy and the Nike Strategy. These are merely what in the light of hindsight I wish I had done consistently.

7. *Rockefeller University Review*, 1967, reprinted in *Littlewood's Miscellany*, ed. Bela Bollobas, Cambridge University Press, 1986.

8. Norman Macrae, *John von Neumann*, New York: Pantheon Books, p. 211.

9. Francis Crick, *What Mad Pursuit: A Personal View of Scientific Discovery*, London: Penguin Books, 1990, p. 70.

10. Feynman, op. cit., pp.227-8.

11. But, as with the Nancy Reagan and Nike strategies above, I must confess that I have not followed my own advice on serious reading as consistently as I should have.

12. p. 60 of the Bollobas (ed) book cited above. Incidentally, on pp. 49-53 of the same book, Littlewood gives a beautiful example of how not to, and how to, write up a mathematical argument; I urge every young theorist to read it and absorb its lesson.

13. In ed. Bollobas, op. cit., p. 158.