Como eu pesquiso

My life philosophy: policy credos and working ways*

Paul A. Samuelson§

Ethics

Many economists - ALFRED Marshall, Knut Wicksell, Léon Walras, ... - became economists, they tell us, to do good for the world. I became an economist quite by chance, primarily because the analysis was so interesting and easy - indeed so easy that at first I thought that there must be more to it than I was recognizing, else why were my older classmates making such heavy weather over supply and demand? (How could an increased demand for wool help but lower the price of pork and beef?)

Although positivistic analysis of what the actual world is like commands and constrains my every more as an economist, there is never far from my consciousness a concern for the ethics of the outcome. Mine is a simple ideology that favors the underdog and (other things equal) abhors inequality.

I take no credit for this moral stance. My parents were "liberals" (in the American sense so the word, not in the European "Manchester School" sense), and I was conditioned in that general *Weltanschauung*. It is an easy faith to adhere to. When my income came to rise above the median, no guilt attached to that. Nor was there a compulsion to give away all my extra coats to shirtsleeved strangers: my parents would have thought me daft to do so, and neurotic to toss at night for not having done so. Some personal obligation for distributive justice liberals do expect of themselves: but what is far more important than acts of private clarity is to weight the counterclaims of efficiency and equity, whenever public policy is concerned, in the direction of equity. As my University of Chicago teacher and friend Henry Simons used to say, "*Any good cause is worth incurring some costs for. Everything should be pushed beyond the point of diminishing returns (else, why desist from pushing it still further?).*"

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Persons who will not volunteer to serve in the army can with good logic vote to pass a fair conscription law that will entail their being drafted with the same positive probability as any other persons. I have generally voted against my own economic interests when questions of redistribute taxation have come up. The fact that I have favored closing tax loopholes has not precluded seeking some advantage from those left in the tax code. But too avid an effort in that direction would seem not only unaesthetic but also a source of some discomfort and self-reproach.

Without exception all the economists I know regard themselves as humanitarians. This includes communists who toe the Stalinist line and Chicago-school zealots for laissez-faire. Yet we all pretty much know what to expect of each other when it comes to policy recommendations and judgments. It is not unanimity. If political economy were an exact, hard science, then more agreement on probable outcomes would occur. If economics were no science at all, only a tissue of value judgments and prejudices, then soliciting an opinion from an economist would tell the Prince or Parliament nothing about the merits or demerits of the proposal under deliberation but only give a reconfirmation that Economist Jones is a bleeding-heart liberal and Economist Smith a selfish elitist.

Political economy as we know it falls in-between. Economists do agree on much in any situation. Where Milton Friedman and I disagree, we are quick to be able to identify the source and texture of our disagreements in a way that non-economists cannot perceive. The disparity of our recommendations is not an unbiased estimator of the dispersion of our inductive and deductive beliefs. With my social welfare function (or, in Waldian statisticians´ terminology, my "loss function") concerning the relative importance of unemployment bad business freedoms, I could disagree 180° with his policy conclusion and yet concur in diagnosis of the empirical observations and inferred probabilities. Yet such is the imperfection of the human scientist, an anthropologist studying us academic guinea pigs will record the sad fact that our hearts do often contaminate our minds and eyes. The conservative will forecast high inflation danger on the basis of the same data that lead the do-gooder to warn against recession. (Conscious of this unconscious source of bias, as the subsequent discussion will elaborate on, I make a special effort toward self-criticism and eclecticism - with what success, the record must testify to.)

An economist who has been preoccupied over the years solely with *Pareto optimality* wrote me long ago that I would be surprised to know how liberal he is. Indeed I would be. Reflecting on his writings, I wondered how he knew he had a heart: it had been so long since he had used it. Organs atrophy without exercise. "Use it or lose it" is nature´s law.

It is not only the arteries that harden with age. Economists are said to appear to grow more conservative as they rise in seniority. This they often deny.

In my own case, I do not perceive that my value-judgment ideology has changed systematically since the age of 25. For a decade now mainstream economics has been moving a bit rightward. But I have not been tempted to chase it. What does tend to change with the accumulation of years and experience is one's degree of optimism about what is feasible and one's faith in good intentions alone. My enhanced skepticism about government ownership of the means of production or the efficacy of planning is not a reflection of ossifying sympathies and benevolence, but rather is a response to the testimony of proliferating real-world experiences.

I am conscious of one occasion in which my respect for the market mechanism took a quantum leap upward. This change had nothing to do with improved performing of the market system. Nor was it related to any new arguments brought forward by Hayek about generating and utilizing information, or to any old arguments about market efficiencies and freedoms by Adam Smith, Frederic Bastiat, or Frank Knight. Rather my changed viewpoint came from observing the communist witchunting episode of the 1950s.

The McCarthy era, in my judgment, posed a serious threat of American fascism. I knew plenty of people i government and the universities whose civil liberties and careers came into jeopardy. I observed at close hand the fears and tremblings that the Harvard and MIT authorities experienced, and these were the bolders of the American academic institutions. As Wellington said of Waterloo, it was a close-run thing that Senator McCarthy was discredited: the Richard Nixon "enemy list" was a joke in comparison, and my being named on it only added to my fading credentials as a New Dealer. What I learned from the McCarthy incident was the perils of a one-employer society. When you are blackballed from government employment, there is great safety from the existence of thousands of anonymous employers our there in the market. I knew of people who got some kind of work in private industry, usually smaller industry since large firms tend to try to keep on the safe side of government. To me this became a newly perceived argument, not so much for laissez-faire capitalism as for the mixed economy.

How did free-market advocates among the economists score as defenders of personal freedoms and civil liberties? This was a subject of great interest to me and over several years I kept a quiet tally of the behavior and private utterances of scores of the leading American and Continental libertarians, almost all of whom I knew intimately. Like a visiting anthropologist I would ask innocent questions designed to elicit relaxed and spontaneous views. If it was churlish to keep a record of private conversations, then I was a churl. The results surprised and distressed me. Worshippers of laissez-faire à la Bastiat and Spencer were insensitive and on the whole unsympathetic toward the rights and personal freedoms of scholars. Alone among the members of the Mt. Pelerin Society the name of Fritz Machlup stood out as one willing to incur personal costs to speak up for John Stuart Mill values. It is not the failure of people to be heroes that I am speaking about. There is little of the heroic in my own makeup and I have learned not to expect much of human nature. What my research found was a sad lack genuine concern for human values.

I was taught at the University of Chicago that business freedoms and personal freedoms have to be strongly linked, as a matter both of brute empirical fact and of cogent deductive syllogism. For a long time I believed what I was taught. Gradually I had to acknowledge that the paradigm could not fit the facts. By most Millian criteria, regimented Scandinavia was freer than my America - or certainly at least as free. When I used to bring up these inconvenient facts to my conservative friend David McCord Wright, he would warn: "*Just you wait. British and Swedish citizens, it is true, have not yet lost their freedoms. But it cannot last that the market is interfered with and people remain politically free.*" We have all waited for more than thirty years now.

Friedrick Hayek wrote his bestseller, The Road to Serfdom, at the end of World War II, warning that partial reform was the sure path to total tyranny. Cross-sectional and time-series analysis of the relationship between politics and economics suggest to me important truths.

1. Controlled socialist societies are rarely efficient and virtually never freely democratic. (There is considerable validity then for the non-novel part of Hayek's warning.)

2. Societies which resisted partial reforms have often been those over taken by revolutionary change. If it is the free market or nothing, often it has then had to be nothing. Indeed, after midcentury the finest archetypes of efficient free markets have often been quasi-fascist or outright fascist societies in which a dictatorial leader or single party *imposes* a political order - without which imposition the market could not politically survive. Chile with its military dictatorship cum-the-Chicago boys is only one dramatic case. Taiwan, South Korea, and Singapore are less dramatic but more representative cases.

3. I can nurture a dream. Like Martin Luther King, I have a dream of a humane economy that is at the same time efficient and respecting of personal (if not business) freedoms.

Much of producing and consuming decisions involve use of the market mechanism. But the worst inequalities of condition that result from reliance on market forces - even in the presence of equality of ex ant opportunity - can be mitigated by the transfer powers of the democratic state. Does the enhancement of equity by the welfare state take no toll in terms of efficiency? Yes, there will be some trade-off of enhanced total output against enhanced equality, some trade-off between security and progress. I call the resultant optimizing compromise *economics with a heart*, and it is my dream to keep it also economics with a head.

My methodology

It is some relief to move from the exalted realm of philosophical ethics to the mundane realm of scientific methodology. However, I rather shy away from discussions of Methodology with a capital M. To paraphrase Shaw: Those who can, do science; those who can't prattle about its methodology.

Of course I can't deny that I have a methodology. It's just that there seems little appeal in making it explicit to an outsider. Or for that matter, in spelling in out to my own consciousness.

I am primarily a theorist. But my first and last allegiance is to the facts. When I began study at the University of Chicago, Frank Knight and Aaron Director planted in me the false notion that somehow deduction was more important than induction. This was a confused tenet of Austrian methodology at the time, and I certainly do not mean by the word "Austrian" the logical positivism of the Vienna Circle. Rather, such direct and indirect disciple of Carl Menger as Ludwig von Mises, Friedrich Hayek, and Lionel Robbins seemed to put on their own heads the dunce caps of the classical Ricardians who believed that by thinking in one's study one could arrive at the basic immutable laws of political economy. I remember believing Director when he pooh-poohed Wesley Mitchell's empirical work on business cycles, claiming instead that the greatest breakthroughs in the subject were coming from Hayek's a priorisms on the subject.

I grew out of this phase fast. Once Lionel Robbins explained lucidly in the first edition of his *An Essay on the Nature and Significance of Economic Science* his claims for Kantian a priorism in economics, his case was lost. Logical positivism is now judged to be an oversimplified doctrine, but it was enormously useful in deflating the pretensions of deductionists. If one had to choose between the methodologies of the warring brothers -

Ludwig the economist and Richard von Mises the mathematical physicist - Richard would win hands down.

Let me not be misunderstood. I abhor the sins of scientism. I recognize that, as social scientists, we can have relationships with the data we study that the astronomers cannot have with the data they study. I am aware that my old friend Willard van Orman Quine, one of this age's greatest logicians, has cast doubt that anyone can in every case distinguish between "analytic" a priorisms and the "synthetic" propositions that positivists take to be empirical facts. Furthermore, Wesley Mitchell's empiricisms on the business cycle do seem to me to have been overrated - not because they are empirical, but rather because his was an eclecticism that never had much luck in discovering anything very interesting, as the lifecycle profile of his post - 1913 career sadly reveals. Some of the skepticisms of Knight and Jacob Viner concerning the empirical statistical studies that their colleagues Paul Douglas and Henry Schultz were attempting, I readily admit, were well taken - just as some of Keynes's corrosive 1939 criticisms of Jan Tinbergen's econometric macrodomes were. But it is on *empirical* grounds that these empirical attempts have to be rejected or accepted, and not because deductive syllogisms can claim a primacy to vulgar fact grabbing. What was wrong with the German Historical School was not that it was historical, but rather that its sampling of the facts was incomplete and incoherent. The facts don't tell their own story. You can't enunciate all the facts. And if you could, the job of the scientists would just begin - to organize those facts into useful and meaningful gestalts, into patterns that are less multifarious than the data themselves and which provide economical *descriptions* of the data that afford tolerably accurate extrapolations and interpolations.

Whatever logical positivism's faults and superficialities are in science at large, it gets an undeservedly bad name in economics from being confused with Milton Friedman's peculiar version of positive economics. Much of what is in Friedman's 1953 essay on this topic is unexceptional and a story so old as to seem almost platitudinous. But what is novel in his formulation and commands most attention is that which I have called "the F twist" - the dictum that a scientific theory is none the worse if its premises are unrealistic (in the usual meaning of "unrealistic" as stating hypotheses that are false and/or far-from-true assertions about what obtains in the actual world), so long as the theory's "predictions" are usefully true. Thought suggests, and experience confirms, that such a dogma will be self-indulging, permitting its practitioners to ignore or play down inconvenient departures of their theories from the observable real world. A hypothesis's full set of predictions includes its own descriptive contents: so, literally understood, an unrealistic hypothesis entails some unrealistic predictions and is all the worse for those false predictions - albeit it is all the better for its (other) empirically correct predictions. We are left then validly with only the prosaic reminder that few theories have all their consequences exactly correct; and it can be the case that a scientific theory is deemed valuable because we have reason to give great weight to those of its predictions that happen to be true and to give little weight to those that are found to be false. In no case is unrealistic falsity a virtue; and there is danger of self-serving Humpty-Dumptyism in letting the theorist judge for himself which of his errors his is going to extenuate or ignore.

Unpopular these days are the view of Ernest Mach and crude logical positivists, who deem good theories to be merely economical descriptions of the complex facts that tolerably well replicate those already-observed or still-to-be-observed facts. Not for philosophical reasons but purely out of long experience in doing economics that other people will like and that I myself will like, I find myself in the minority who take the Machian view. "Understanding" of classical thermodynamics (the archetype of a successful scientific theory) I find to be the capacity to "describe" how fluids and solids will actually behave under various specifiable condition. When we are able to give a pleasingly satisfactory "HOW" for the way of the world, that gives the only approach to "WHY" that we shall ever attain.

Always when I read new literary and mathematical paradigms, I seek to learn what descriptions they imply for the observable data. The paradigm's full set of entailed descriptions is what is of interest and forms the basis for a complete judgment on it. My work in revealed preference, in Foundations of Economic Analysis, and in the several volumes of Collected Scientific Papers, consistently bears out this general methodological procedure.

I dislike being wrong. Long before knowing of Karl Popper's writings, I sought to be my own strictest critic. Why give that fun to the other chap? All this explains why I am an eclectic economist. It is not because of inability to make up my mind. I am eclectic only because experience has shown that Mother Nature is eclectic. If all the evidence points to a single-factor causation, I have no internal resistance to accepting that. But there is a big "if" involved in the previous sentence.

Being prepared to be eclectic does not have to inhibit bold theory building. One creates boldly knowing that this does not commit one to exaggerated belief in the sole potency of one's brain child.

We all have secret vanities. He prides himself on his good looks. She takes satisfaction in her sense of humor. I do delight in producing still another beautiful model that illuminates important terrains of economics. But in my heart of hearts I nurture the claim that I have good judgment. Be wise, sweet maid, and let them who will be clever. My theories must run the gauntlet of my judgment, an ordeal more fearsome than mere peer review. (Of course one can have one's cake and eat it too by presenting a theoretical gem as an unpretentious mirror of some aspects of some corner of the economic terrain under observation.) Why let sagacity degenerate into well-informed nihilism? The mindless naysayer is no better than the mindless yeasayer. Neither adds anything to the silent scientist's cipher.

Joseph Schumpeter, who all his life whored after beautiful theories, just before he died testified at the 1949 National Bureau conference on business cycles: If he had to choose between mastery of mathematics and statistics, or of economic history, he would have to choose mastery of economic history. I won't disagree. But I deny the need for dichotomous choice. Give apes in the Widener Library a data bank of all that's there and you don't get a master economic historian. What you get back is the data bank and a curator.

Let me make a confession. Back when I was 20 I could perceive the great progress that was being made in economic *methods*. Even without foreseeing the onset of the computer age, with its cheapening of calculations, I expected that the new econometrics would enable us to narrow down the uncertainties of our economic theories. We would be able to test and reject false theories. We would be able to infer new good theories.

My confession is that this expectation has not worked out. From several thousands of monthly and quarterly time series, which cover the last few decades or even centuries, it has turned out not to be possible to arrive at a close approximation to indisputable truth. I never ignore econometric studies, but I have learned from sad experience to take them with large grains of salt. It takes one econometric study to calibrate another; a priori thought can't do the job. But it seems objectively to be the case that there does not accumulate a convergent body of econometric findings, convergent on a testable truth.

Does this mean that I belong to the camp which regards truth as in the eye of the beholder? Which denies the existence of an objective truth out there, in political economy as well as in astronomy and biochemistry? Which recognizes in the truth of mainstream economics only the class interests of the bourgeoisie, and in the truth of Marxian economics either the class interest of the nascent proletariat or the objective truth of the final classless and universal society?

No. Observing myself over fifty years and a vast number of scientists in various disciplines, I do recognize that truth has many facets. Precision in deterministic facts or in their probability laws can at best be only partial and approximate. Which of the objective facts out there are worthy of study and description or explanation depends admittedly on subjective properties of the scientists. Admittedly, a given field of data can be described in terms of alternative patterns of description, particularly by disputing authorities who differ in the error tolerances they display toward different aspects of the data. Admittedly, observations are not merely seen or sensed but rather often are perceived in gestalt patterns that impose themselves on the data and even distort those data.

But still, having admitted all the above, as you observe scientists and study the developments of disciplines when schools evolve and paradigms are born and die, it is forced upon you that *what ultimately shapes the verdicts of the scientist juries is an empirical reality out there.* When a Marxist scores a triumph it is not by employing a useful alternative to 2 + 2 = 4 logic, or cultivating a different Hegelian dialectic. We esteem a Pavlov, Lysenko, Haldane or Bernal, Landau or Baran for what they can or cannot accomplish with respect to animal experiments, plant breeding, hydrogen-bomb exploding or phase transitions, or insights into the observable paths of economic development.

When Thomas Kuhn's book, *The Structure of Scientific Revolutions*, came out in 1962, I made two lucky predictions: one, that in the physical and life sciences its thesis would have to be modified to recognize that there is a cumulative property of knowledge that makes later paradigms ultimately dominate earlier ones, however differently the struggle may transiently look; two, that Kuhn's doctrine of incommensurability of alternative paradigms would cater to a strong desire on the part of polemical social scientists who will be delighted to be able to say. "*That's all very well in your paradigm, but your white is black in my paradigm and who's to say that we'uns have to agree with you'uns.*" Kuhn has correctly discerned the warts on the countenance of evolving science. His readers must not lose the face for the warts.

How I work

As a theorist I have great advantages. All I need is a pencil (now a ball pen) and an empty pad of paper. There are analysts who sit and look vacantly out the window, but after the age of 20 I was not one of them. I ought to envy the new generation who have grown up with the computer, but I don't. None of them known to me sit idly at the console, improvising and experimenting in the way that a composer does at the piano. That ought to become increasingly possible. But up to now, in my observation, the computer is largely a black box into which researchers feed raw input and out from which they draw various summarizing measures and simulations. Not having access to look around in the box, the investigator has less intuitive familiarity with the data than used to be the case in the bad old days.

I have been blessed with an abundance of interesting problems to puzzle out. Many artists and writers run into long fallow periods when new creative ideas just will not come. Luckily, that has not been my experience. Perhaps I am insufficiently self-critical to recognize when problems of lower quality are involved. In any case mine has never been the Carlylean view of Schumpeter that only the greatest ideas count, and only a few great men are important in history and in the development of science. One tackles the most important unsolved problem at hand. Then the next one. If that leads down the path of diminishing returns in the absence of dramatic new challenges and breakthroughs, so be it.

"What are you working on now?" This is a question I have been asked all my life. And never in my life have I known how to answer it. At any one time I have several balls in the air. And always there is an inventory of questions just below the threshold of my explicit attention. Some of these slumber in that limbo for two decades. There is no hurry: they will keep. Some morning (or at night in the dream) the evolving wheel of chance will turn their number up.

Poets testify that often their lines gush up from within. They merely write down what their muse is dictating. That sounds rather highfalutin, but there is something in it. When I was young I used to explore a topic; write down equations and syllogisms dealing with different aspects of it; then outline the final work. After that the final draft could be written out. Perhaps what I am describing is the optimal way to write a paper.

Increasingly after the age of 35 that is not how I have in fact operated. Instead I have often let the paper write itself. A problem is posed. One begins to solve it, writing out the steps in the solution, One development leads naturally to another, as one exposits in writing. Finally, what can be solved of the problem has been solved. The paper is finished. What has been finished is not something that has ever been envisaged, waiting only to be written down. All this is reminiscent of Franklin Roosevelt's dictum, "*How do I known what I think until I hear myself saying it?*"

This means that some articles might be composed in half a day. Of course the first draft need not be the final draft. There may follow many hours of revising, involving additions, deletions, rearrangements, and corrections. Perhaps it would be better to follow the first draft with a completely new rewrite. But that is not my usual practice, as I trade some perfection against more time for new topics. This means I am a prisoner of my first drafts, and it is a source of exquisite pain if a manuscript is lost: my mind rebels at having to reconstruct a lost argument, and impatience is likely to make a recollected version abridge some essential matter.

Prolific scholars are addicted to writing. A day spent in committee meetings is for me a day lost. After an interval of fasting, you are hungry. After an internal o doing no analytical research, there is so to speak a fluid inside you that wants to get free. I used to think that the unconscious mind, which Henri Poincaré described so beautifully as working away at specific puzzles the mathematician is interested in, was accumulating findings on the particular problems that routine duties prevented me from dealing with. But I have come to think that not to be quite correct. For *any* new topic can capture one's enthusiastic and fruitful attention after a period of deprivation. One snowy day in New England I was told at the airport gate that Washington was snowed in. A friend hearing me inquire, "*Can you go New York?*" asked, "*Are you just bound to go somewhere this day?*" That's exactly what it's like with the creative urge: It doesn't have to spend itself on the theory of capital that has been engaging the scholar's recent attention; it just wants to go about doing something creative, and its motors seem revved up to be effective in whatever direction it is pointed.

Reporters used to speak of a nose for news. What is important in scholarship is an aesthetic sense for what is an important problem. Otherwise the facile mind can spend itself on patterns that are merely pretty. For recreation I would rather play tennis than play chess, or read pedestrian detective stories than solve the mathematical conundrums that appear in the back pages of learned journals. My unconscious motivation, I suspect, is that chess and problems-solving involve the same energies as innovative scholarship does. They will usurp some of the limited supply of precious brainpower that might better go toward learning something new: and, involving use of the same workday muscles so to speak, those recreations do not provide as refreshing rest periods. I daresay that the powerful pure mathematician faces a different problem from the applied scientist. A great mathematician is only as great as his greatest deeds. The revolutionary idea that might lead to great deeds comes very rarely.

One marks time in between and one might as well mark time while keeping the brain tuned up in chess or bridge as in any other way. However, I do not have too much confidence in the distinction that I have just made. For it certainly does not cover the case of prolific mathematicians such as Poincaré or Euler. A mathematical snob like G.H. Hardy might judge that much of Euler and Poincaré could just as well have never been written. But even from the snobbish viewpoint, we must reckon with the fact that some of their best work would not have gotten done if it had not been as outgrowth of some of their less transcendental achievements.

I said that my working tools are only pen and paper, and that an airplane cabin provide as good an environment for research as a library study. That is true as far as analytical creativity is concerned. On the other hand to stay well informed on what it is that is important to be done, a scholar must have access to books and to learned journals. In this regard I have always been very lucky. Whatever works the MIT libraries have not had, the neighboring Harvard libraries can be counted on to provide. These are very few great scholars working of by themselves with paper and pen far from the centers of creative economic thought. Those who pride themselves on being most autonomous usually end up most idiosyncratic.

Long ago I set myself the grandiose challenge of not being merely subjectively original. More useful to science - and more truly fulfilling if you can bring it off - is to try to stay informed on what other scientists have done and to advance the frontier by your own quantum jumps. In terms of the old song: "*Good work if you can get is. And you can get it if you try.*"