The Caldwellian Methodological Pluralism: Wishful Thoughts and Personal Tendencies

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The Caldwellian Methodological Pluralism: Wishful Thoughts and Personal Tendencies

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Abstract: Economists failed badly both to predict and solve the Great Global Recession of 2008-2010 for two interconnected reasons. The first is that economics has moved too far away from its social foundations. The second reason is that the positivist economic methodology that economics follows has produced both benefits and costs, perhaps even costs than benefits, one may argue. This paper looks at the available evidence (not exhaustively) to argue for a Caldwellian methodological pluralism. It illustrates the advantages of such a methodological approach as well as the disadvantages of its alternatives, especially monolithic positivism.

Keywords: Economic methodology, methodological pluralism, philosophy of economic science, economic research methods, explanation versus prediction. **JEL Code**: B14, B50, C81, C82, Y80

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1. Introduction

In a piece for the *New York Times* Professor Paul Krugman (2009) asked a seemingly innocuous question about the Great Global Recession (GGR) of 2008-2010: "How did economists get it so wrong?" His brief answer was that, in the USA at least, current policy has been taken over by the 'vulgar economics' preached by free marketeers ('freshwater economists') who not only assume the omnipotency of market efficiency, but also put down 'saltwater economists' and everyone else. My own answer is that the failure in this regard stems from the fact that economics itself has moved too far away from its social foundations. The shift exposes analytical holes in the current economic methodology, which means there is a need for economists to return to their genealogy in the social sciences. As James Buchanan (1979) has put it:

> Modern economics, as practiced by professional scholars, embodies confusions that are fundamentally methodological. These have their historical foundations in the failure of economists to establish an effective synthesis between the objective and the subjective theory of value. The issues did not emerge with clarity, however, until efforts were made to extend the applicability of economic theory beyond its traditional limits. So long as the task of theory remained that of 'explaining' the functioning of the market system, objective and subjective element could exist side by side without open contradiction. During the past half-century, however, theory has been called upon to do much more than this. It has been employed to derive norms for policy aimed at making allocation more 'efficient.' Economists have, in other words, proceeded as if theirs were a 'science of choice' (pp. 62-63).

In other words, economists over bit and they remain both unable and unwilling to adjust their overbite. To help encourage the return to the basics, I compiled *Selected Readings on the Anthropological Bases of Economic Behavior, Organization, and Control*, whose objective I summarize in Amavilah (2010).[1] These efforts outline pieces of work by Frank Knight (1951), Joan Robinson (1970), Bronislaw Malinowski (1944), Melville Herskovits (1952), W. Arthur Lewis (1965), and J. Kornai and B. Martos (1981). Both *The Readings* and the summary conclude that real life is neither as simple nor economic as economic theory sometimes suggests. In fact, and for much of human history, non-economic factors and forces have driven economic activities and reinforced economic principles. Detached from its social foundations, economics is boring fiction. As serious non-fiction, economics cannot successfully divorce itself from its elemental foundations in the social sciences. Any divorce means loss of both economic spill-over and "spill-in" effects on neighboring disciplines as James Buchanan (1979, Chapter 6) put it. For instance, to advance its theory in the past, economics needed to borrow its formal language from mathematics and its methodology from physics – both forms of spill-ins. Farmer, Shubik, and Smith (2005), Shubik and Smith (2006), and Camerer and Fehr (2006) all agree that economics has something important to learn from other disciplines. However, to remain policy-relevant economics cannot wish away the very social bases upon which it is founded; without its social foundations economics has a limited useful future.

It is clear that the methodological problem is in the black and white distinction which economists seek to draw between positive economics (superior and desirable) and normative economics (inferior and undesirable). Superficially, the theoretical distinction between positive and normative economics is easy to understand. The objective of a positivistic research philosophy and its methodology is limited to the first-order cause-effect relationships among economic phenomena. It deals with what and how economic relations are – the "what is" statements. Normative economics on the other hand goes beyond positive economics to make value judgments about feasible objectives over which discrete choices may be exercised. Thus, my general impression is that it is impossible to determine the deep causes of economic phenomena while remaining religiously wed to the current

[1] The manuscript is ready, but I do not yet have all the rights to publicly reproduce selected materials.

methodology of economics, because in practice the distinction often melts away like wax exposed to heat. Economic policies (norms), for example, intervene in what and how economic affairs are – an intervention that even Friedman (1953) would accept. The interaction makes choice among competing methodologies difficult, especially in view of well-documented weaknesses of, and the absence of better, replacements. Choice of either a positive or normative methodology is self-limiting because researchers do not decide what problems will befall their clientele in the future, or even whether there will be a felt need to solve them. Economic problems are often spontaneous and exogenous, and the existence of markets alone, no matter how free, does not guarantee solutions. To the contrary, research sanctions (e.g., availability or lack of research funds) are semi-exogenous, even assuming unrealistically that researchers have some influence on the research problems they end up working on.

Regarding the normative versus positive specialization among economists a similar problem pertaining to the developing countries is that there the number of trained economists is smaller. Hence, specializing as either a positivist or normativist reduces the impact that economists as a small group can have on policy issues, which dismalizes economics further. A positivist research stance may, for example, preclude some researchers from participating actively in research on topics deemed normative, and vice versa. Therefore, whereas the theoretical distinction between positive and normative approaches is useful, requiring researchers to subscribe to either of the two positions before specific research problems are even identified is not helpful to both policy and further research.

The preceding statements summarize my personal frustrations with disciplinary requirements to follow distinctive research philosophies and methodologies. In the next sections I pay attention to what I have dubbed Caldwellian methodological pluralism (CMP) and outline my wishful thoughts and personal tendencies. Specifically, I describe my understanding of the role of economic theory in economic research, the purpose of empirical work, and finally the problems of, and solutions to, data use in economic research. I base the discussion mainly on selected works by Bruce Caldwell and Mark Blaug, especially *Beyond Positivism: Economic Methodology in the Twentieth Century* (1982) and *The Methodology of Economics, or How Economists Explain* (1980), respectively, and a few other selections as well.

2. Some Clarifications

To appreciate where I am coming from and clarify my preference, I discuss my research interests briefly below. Let me say that two major reasons explain why I prefer the CMP to either a purely normativist or purely positivist research philosophy and methodology. The first reason stems from my interests in both descriptive and prescriptive economics. Consequently, a "conditionally normative" approach appeals to me as the most efficient way to pursue a positive research objective (Castle, 1983, 1980, 1968, 1969). A conditionally normative position, unlike its purely positivist or purely normativist cousins, considers value judgments á lá Gunnar Myrdal (1969). The knowledge of value is important because it tells us something about the nature of the facts at hand. For example, in studying poverty in Apartheid South Africa, raw facts in themselves were less informative, perhaps even useless, unless the political and ideological values implicit in the collection, analysis, and interpretation of such facts were recognized. Hence, the quantitative measures of poverty based on raw data invariably gave the perception that all South Africans were equally well-off. However, a little acquaintance with the political values of the Apartheid regime clearly showed that economic measures at the time exaggerated the living standard of an average black South African, and underestimated that of an average white South African. This example is consistent with J.S. Mill's (1961) positive view of the world and the role of people's beliefs (call them utility) on that view. It indicates that one cannot deduce who values what from facts alone without preconceived ideas about the value of what is valued by whom. In short, the cliché that "data/facts speak for themselves" is just that, a cliché.

A descriptive-prescriptive research methodology and its underlying philosophy starts with real life problems. Like its positivist counterpart, it uses logical reasoning to draw conclusions. Unlike its counterpart, it does not try to dig for problems. Real-life problems are already on the surface affecting real-situations; they need buried, not dug up; it is solutions that need searched for high and low. In that sense a descriptive-prescriptive methodology prefers simplicity to complexity and clear thinking to deep and straight thinking. It recognizes that values and the research activities are inseparable. Hence, it makes value judgments explicit and treats them objectively á lá Gunnar Myrdal (1969). Moreover, the interdisciplinary nature of the methodology enables one to work with value-loaded problems like economic growth and development, income (re)distribution, poverty, hunger, education, and discrimination, social justice, fairness, to mention a few examples. These important matters are now frequently avoided,or they are simply glossed over by those who strive to be positivist scientists rather than "negativist" non-scientists, the connotation commonly associated with normative economics.

The CMP is also suitable for people with interest in a variety of subjects within economics. As Bruce Herrick and Charles Kindleberger point out in their classic textbook, *Economic Development* (1983), people study economics for different reasons, indicating the interdisciplinary nature of the research activity, and therefore the suitability of CMP to problems affecting developing countries. Of course, it does not mean that disciplinary research is no good for these countries. Disciplinary research has made important contributions to the development of economic theory, and hence to the search for solutions to problems facing all countries. However, a good theory that is largely inapplicable, or one which fails to solve the problem it sets out to solve, is just good fiction (Buchanan, 1979). This take explains why it is not uncommon to find studies cloaked as positive economics which end up making normative policy recommendations. I am sorry to say that a good number of the World Bank's research on African countries is of this type.[2]

3. The CMP: Wishful Thoughts and Personal Tendencies

According to Caldwell, generations of philosophers specializing in the growth of knowledge have made tremendous effort to locate and confirm a "prescriptive, universal, and scientific methodology." However, such effort generated many competing theories instead. Confirmationists did not succeed to produce a systematic formula for choosing among competing theories. Instrumentalism as advocated by Milton Friedman (1953) also has had limited practical use because it is good mainly for research situations in which prediction is the only goal. The falsification approach to research, in the pure Popperian sense, only seeks to minimize error. This all means that the rigor and objectivity of research activities and the techniques they employ are not unchallengeable. For varied discussions of these issues see Frank Hahn and Martin Hollis's *Philosophy and Economic Theory* (1979).

It has been long understood that progress for "normal science", to use Thomas Kuhn's (1970) terminology, is cumulative; revolutions in science happen, but infrequently. Success is often independent of strict and unbreachable methodologies. Bruce Caldwell put this cogently by saying that "the story of science involves both constancy and flux, both bold conjectures and rigorous criticism, both normal science and revolutionary crisis. The positivist fixation on the objective side of science missed half of a beautiful and complex tale" (p. 244-245). Thus, the failure of early philosophies to find one "God-Methodology" that sets the criteria for scientific investigations indicates that a different approach is badly needed. In saying this I appeal to Caldwell again that "other significant tasks can be attempted: to foster an understanding of the scientific process among members of the [economics] profession; to systematize jargon; to rationally reconstruct the methodological content of various research programs; [and] to promote an environment in which both novelty and criticism can operate freely" (p. 245, []

[2] I personally find it paradoxical that a good number of Keynes critics, work for the very institutions which Keynes helped to create.

added). Too much effort has gone unnecessarily into the search for a single research methodology; the black cat may not be in the dark room, and it is time for a new and more comprehensive research methodology – the CMP may be just it.

The fundamental problems that befell all attempts at finding *the methodology* are the problems of choosing among competing theories and theory appraisals. Again, logical empiricists like Karl Popper (1961, 1962, 1964, 1965, 1972) concentrated on confirmation, arguing for the testing of the whole theory rather than its parts by comparing its predictions to data (cf. Wold, 1989). Comparable theories can then be ranked according to their relative quantitative and qualitative strengths, as well as 'temporal acceptability'. However, since highly confirmed theories did not have to be true, the choice among competing theories was ultimately not made on the basis of the strength of confirmation. Additional criteria such as logical consistency, elegance, simplicity, familiarity, generality, and extendability were invoked. But these criteria were themselves unfalsifiable.

These problems are not entirely new. Karl Popper devised his fundamental asymmetry, which required that all propositions based on universal laws be disprovable, to deal with some of these issues. However, the requirement simply recognized the problem of induction and did not touch the difficulty of choosing among competing theories. It was only Feyerabend's (1965, 1975, 1978) position on theoretical pluralism that did not have to worry about the problem of choosing among competing theories. Unfortunately, Feyerabend did not provide any criteria for theory appraisal either (see Blaug, 1980, pp. 43-44).

Mark Blaug (1980) returns to falsification as an alternative to positivism. Unfortunate falsification seems unpracticable in economics for the following five reasons: (1) There are infinite initial conditions to falsify; (2) even when presumed finite, some initial conditions may be highly variable and uncontrollable; (3) many general laws (e.g., the rationality postulate) are unobservable and unfalsifiable; (4) model falsification is not the same thing as theory falsification; and (5) empirical data, particularly in the developing countries, is often inaccurate, incorrectly interpreted, unreliable and aggregative. Caldwell (pp. 238-242) has articulated these reasons well, but I return to the fifth reason in the penultimate section of this paper.

In sum: The search for the God -Methodology has failed to produced criteria for theory choice. Hutchison (1938, 1956, 1960, 1977, 1978) , one of the strong proponents of falsificationism, has argued for theories to be tested and for their implications to be compared against data. Realizing how difficult that would be, Paul Samuelson (1955, 1963, 1964, 1983) has softened Hutchison a little by requiring that hypotheses need only be conceivably falsifiable and that theories need only make reference to observable facts (cf. Wong, 1973, Boland, 1989). This reinterpretation of Hutchison has brought charges that Samuelson is recommending lip service to serious problems. The charges are in addition to Blaug's call for falsification as a substitute for positivism, although Blaug himself suspects that few economists would provide theories that would yield refutable implications. The reluctance of economists to subject their theories to facts disturbs Blaug, making it difficult to understand why he continued to prescribe methodological monocracy, despite his argument that empirical evidence is useless when research programs are divided.

I think Blaug's error is his unwarranted insistence (assumption) that neoclassical economic theory is the vanguard economic theory. From that viewpoint it is easy to see why he would argue that neoclassical theory has had a powerful predictive record regardless of its hierarchical and 'absolutist' nature. He further alleges that neoclassical theory can provide the criteria for accepting or rejecting research programs, and it can also set the standards for separating "wheat" from "chaff" – poor rhetoric on his part since it presumes wheat is necessarily good and chaff is always bad. There is a need for a pluralist methodological approach, and CMP is it.

How can CMP work where others have failed? How can it be achieved? Is CMP an invitation to methodological'barbarism'? Well, the CMP is a reasonable alternative, because its major advantage is that it uses Popper's method of rational reconstruction (cf. Wong in Caldwell pp. 195-199, Wong, 1973). This method enables its users to belong to designated research programs, but not to designated research philosophies and methodologies. To understand a theory all that a researcher needs to do, first, is to reconstruct the hypothetical problem-situation, which the theory was developed to tackle. See, e.g., Maki's (1986, 1988, 1995, 2008, cf. Klamer and Colande, 1990, Klamer, 1987, Klamer, 2004) reconstruction of D. McCloskey (1976, 1983, 1985) for an excellent illustration. Reconstruction in this way facilitates further explanations as to why the theorist believes that his/ her theory solves the problem, and does so more effectively than alternatives. Doing so requires proper documentation of the objectives (theoretical aims) as well as metaphysical, methodological and epistemological orientations (situational constraints). Secondly, the researcher assesses the strength of a theory by criticizing it externally and internally. External criticism seeks to establish whether or not the theorist reached his/her stated goals. Internal criticism considers the logical structure and consistency of the theory, its goals, whether the problems it deals with are solvable, whether theoretical aims and situational constraints are well understood, whether moral considerations are accounted for, and, finally, whether the theorist's knowledge of the general problem-situation approximates what is already known and what is knowable. Compare this material to A. Phillips Griffiths' *Knowledge and Belief* (1967).

The last two points are important features of the CMP. Accounting for moral considerations means accepting the inseparability of research from value judgments. Studying for and writing *The Theory of Moral Sentiments (1759)* made Adam Smith a better economist than he would have been because the experience provided the ethical, psychological, philosophical, and methodological bases for *The Wealth of Nations* (WON, 1776). The illustration is good to point out because 'positive economists' adore *WON* even though only few today under the age of 60 have read it in full – a ridiculous accusation on my part, but one I am prepared to defend. Thus, adopting CMP means being willing to deal with values objectively. The requirement for adequate knowledge of the general problem-situation serves as a check on theorists of economic development, for instance, who like to theorize about specific problems of developing countries without sufficient knowledge of the general problem scenarios in those countries. Africa is a classic example; Africa suffers an *Africa-itis* by which there is a standard way of writing about Africa if any one wants his/her research to be published (see Binyavinga Wainaina's 'How to write about Africa' (2005).

The CMP is also attractive because it allows further assessment of the methodological content deemed essential to the nature of a theory. Such an assessment compares the merits and demerits of the theory, bearing in mind that the task is not to optimize a single methodology, but rather a whole range of, perhaps not compatible but complementary, methodologies. This approach may be useful to the developing countries because it allows economists to examine methodological contents of existing theories to determine whether or not the theories themselves can be applied to specific problems facing different economies.

Again, the assessment of methodological strengths and weaknesses stimulates discussions about the advantages and disadvantages of "the rationally reconstructed methodological positions under examination" (Caldwell, p. 247). Some theories which have passed scrupulous empirical tests in the industrialized economies invariably fail similar tests in developing economies. This may be a result of bad theories themselves, but more likely of bad methodologies. The concern this outcome suggests is that there has not been much discussion going on in both developed and developing countries about methodological failures because current methodologies are inflexible or they are not flexible enough. The problem then becomes that of methodological dogma rather than of institutions. The solution is a pluralist methodology like CMP, which permits discussion on these issues.

A pluralist methodology also creates a conducive atmosphere in which methodological evaluation of alternative research programs can be carried out. The rational reconstruction method permits criticism. Pluralism sparks and fuels healthy debates. These are important features of the CMP, which will become even more important if H.A. Simon (1979) is correct that in some future time economics will shift "from substantative to procedural rationality". Some of the existing research programs are descriptive while others are prescriptive. What CMP offers are firm grounds for the combination of description and prescription, if the policy implications of research are to be useful. The description-prescription combination enables a clear understanding of the process of scientific growth and ultimately the effects of scientific progress on economic performance, technological change, and welfare.

Although Caldwell himself has not elevated the CMP to the status of *the methodology*, some objections to it can legitimately be anticipated. One could argue that a better methodology already exists. A reasonable question would be: What is it? Friedman's instrumentalism, which has the largest following in the USA (Paul Krugman's freshwater economists), has not been able to resolve the issues of theory choice and theory appraisal (Lakatos, 1976, 1978, Mayer, 1975, 1980). It holds that prediction is the primary goal of economic theory and that the realism of assumptions does not matter. Yet, it goes on to say that the choice between competing theories requires such criteria as simplicity and fruitfulness. The argument is logical, but not convincing, and hence it does not pose an insurmountable challenge to CMP. When economic prediction cannot be explained, it becomes inexplicable from astrological and religious prediction. The CMP is an all-inclusive general case of which instrumentalism is one of many special cases; it does not condemn to 'hell' those researches who, and research programs, which subscribe to the "methodology of positive economics" (Friedman, 1953). It welcomes as equals even the competition.

The second possible objection is that CMP "undermines substantative work in economic science" (Caldwell, p. 250). This objection is not substantive itself. The opposite seems true, and CMP may in fact increase substantative work in economics because competition enforces efficiency, which in turn increases productivity. A true "scientific method" would emerge to replace the dogmatic "Scientific Method" (cf. Green, 1977, Hansen, 1996, Hempel, 1965).

The third likely objection is that CMP may lead to methodological anarchism. This objection is simply nonsensical. The term anarchism has been used in attempts to ridicule and silence D. McCloskey's "economic rhetoric" and before that Feyerabend's (1965, 1975, 1978, 1981) theoretical pluralism. The term is used in vindictive, even invective, ways. All it really shows is intellectual intolerance and the extent to which some people are willing to go to defend their positions rather than accept change – a methodological dictatorship (autocracy) of a kind – for why would a synthetic methodology should necessarily lead to anarchy more than the alternative?

The CMP accommodates all research programs; it does not require strict membership to a particular research ideology or philosophy because it embraces both positive and normative analysis. It exposes the naivete of the positivist method that emphasizes high R-squared and large (statistically significant) t-values, while denying the role of values in economic research – a very strange denial since economics is really the study of the value of scarce resources (Darnell and Evans, 1990, Harrod, 1938, 1968, Machlup, 1955, 1963, 1978, Mayer, 1975, 1980). The CMP does not deny the existence of positivist research programs; nor does it exclude the possibility for the emergence of alternative methodologies. What it appreciates best is that science goes beyond the high R-squared. The study of economic science is ultimately the study of values in use and exchange. Those researchers who appreciate the importance of values must see comfort in the CMP, and when they do, it is not a matter of intellectual inferiority, but rather a matter of intellectual responsibility to the growth of scientific knowledge. Accepting CMP is choosing to gain freedom to learn from other people. Jack Hirshleifer (1977, 1985) and more recently Ben Fine (undated), are both correct that Economics has contagious imperialist effects on other disciplines, but economists cannot possibly know everything (Rosenberg, 1979) so that the nature of the effects economics can have are both spillovers and "spill-ins" (Buchanan, 1979). The 'unrealist' positivist remains split about this. Rejected by the "hard sciences" he/she is trying to mimic, he/she then arrogates himself/ herself to the position of the King of all social sciences within which he/she remains anti-social and afraid of losing his/her status, prestige, and power. Pluralism means stiffer competition, but CMP also creates opportunities by inviting the participation of young brains, new thoughts and ideas, understanding well that competition lowers monopoly rents accruing when artificial barriers to entry are high. However, for CMP followers the opportunity cost of decreased salaries is worth the benefit of unmasking academic protectionism and pseudoscience in the name of positivism. The benefit aids the growth of science and pushes the economics discipline to elevated heights.

4. The Role of Economic Theory in Economic Research

Economic theory plays three equally important roles in economic research. First, it explains the nature of economic activities to enable an understanding of the economic circumstances surrounding economic decision units, and the way these units interact and intra-act in the production, exchange, and distribution of economic goods and services (Robbins, 1935, 1979, Emmer, 1967, Eagly, 1974). Theory helps us understand scarcity, choice among alternatives, and the role of markets and prices (Arrow, 1963, cf. Bromley, 1997). A clear understanding of economic phenomena enlightens us about available opportunities and the best ways of achieving or maximizing them. Whether or not explanation itself exists in economics is a dividing question; however, a considerable number of economic schools of thought tend to agree that explanation is an important goal of economic theory, albeit not the only goal. Human beings, including researchers, want to know and understand the present and how to anticipate the future (explanation).

The second role that economic theory plays in economic research is prediction. Predictions help us understand what will happen to the key variables that affect our well-being. Having an idea about the impact of key variables on our lives in the future puts us in a position to fix disturbances around these variables. This is probably what Karl Popper (1964, 1965, 1961) had in mind in suggesting that the value of scientific research is to minimize random errors. However, how useful can a small error be to policy and further research if we cannot explain what it is, and how we can minimize it?

Since prediction is the major preoccupation for many economists, Krugman's piece for the *New York Times* referred to above is correct to ask: "How can economists fail their major preoccupation so badly." In fact, Krugman is gentle in his criticism; economists come out worse if they cannot explain 'how they got their key preoccupation so wrong'. For most economists explanation takes the form of rhetoric, which they deny they are using, as McCloskey (1985) tells us. This makes the fascination with prediction at the expense of explanation even more puzzling.

Some have argued that economic theories do not predict well because the real world is dynamic, and because human behavior is similarly dynamic and unpredictable. This argument is true, but not strong. Economic models abstract from the real world (Tinbergen, 1969, Harrod, 1968, Samuelson, 1948[1983])). According to Jan Tinbergen's (1969) Nobel Prize Lecture, for example, a good model does three "essentials": (a) it draws up a list of variables to be considered; (b) it draws up a list of the equations or relations governing the variables listed; and © it tests the validity of the equations (cf. Harrod, 1968). Thus, models approximate at least some aspects of reality. Yes, the world is not static; yes, people are both rational and irrational creatures, making prediction seem like shooting at a moving target in the darkness (Ariely, 2008, 2012, Mill, 1961). The fact that some (perhaps many) shots miss the target, suggests a need for an explanation as to how and why that is happening; is the problem with the target, the shooter, the rifle, the initial conditions and situational constraints, or some combination of all these? Since economists in general are aware of the predictive weaknesses of economic research, which is one reason why many use prediction and estimation interchangeably, it does make good sense to think of explanation as no lesser a research goal.

Thirdly, economic theory enables researchers to abstract from the real and complex world and to draw conclusions from simplified sets of initial conditions – conclusions which approximate how the real world works. This does not mean that theories do not have problems. In the preceding section some problems relating to the role of economic theory in economic research were outlined. More importantly, the problem of theory choice is still with us regardless of the tentative solution being offered by the CMP. Indeed, research with out a theory is like an observatory without a light. It is also important to understand that theories are statements of inclination. They provide generalizations about how economic phenomena are (explanation), or what will happen (prediction) if in practice initial conditions hold. The practical relevance of economic theories depend upon the construction of assumptions. For the theory to play both its predictive and explanatory functions, its assumptions need to be correct and complete (and preferably realistic, not just testable). If the assumptions are incorrect and incomplete, then there will be a huge gap between theoretical and practical considerations of the theory (Samuelson, 1964, 1963, 1955). Two quick examples illustrate this point.

First, in the 1950s and 1960s a popular hypothesis held that African peasant farmers and workers did not respond correctly to commodity and factor prices. The assumption was that African peasant farmers and workers preferred leisure to work. In the labor market the observation was that peasant workers allocated fewer and fewer man-hours of labor as wage rates increased – plotting the now familiar backward-bending individual supply curve. In product markets farmers produced less and less at higher product prices – which sounded like disobeying the law of supply. To visiting agricultural experts the observation meant that these people were lazy, or they were ignorant of their market values. Not only was the hypothesis incorrect, as La-Anyane (1985) later showed that during that same time period the cocoa industry in Ghana responded correctly to market prices, it was also incomplete because it lacked full understanding that all rational workers allocate their time between work and non-work (leisure) activities. Given limited time, the opportunity cost increases as more and more time is spent doing one activity and giving up the other(s) – rational laziness.

The formulators of the 'laziness' hypothesis did not ask whether the increase in prices or income was large enough to induce a corresponding response in inputs and outputs. Historical studies are now showing that wage rate increases were not large enough to offset the implicit costs of poor working conditions and inhuman treatment of African workers by their [colonial] employers. Similarly, in the commodity markets farmers understood the small size of their monetized market, and the observed low production levels were meant to avoid excess supply. This is partially confirmed by the fact that the price and income elasticities of traded and non-traded commodities.

The second example has to do with the connection between employment and the industrialization of nations. Theory assumed that the marginal productivity of rural labor in developing countries was very low because of the "unlimited supplies of labor" there (Lewis, 1954, 1965). Since it was low labor productivity in rural areas that constrained economic growth and therefore economic development, Lewis saw rural-to-urban migration as a potential source of growth and development. But as in the previous case, studies are now showing that marginal productivity of rural labor was never as low as theories predicted then. Depending on the nature of commodity production, the marginal productivity may have been low when product demand was low. At wages below subsistence level the marginal productivity was never zero because even when unemployed some labor is spent on other economic activities.

In both cases above, researchers understated the fact that theories are only statements of tendencies (cf. Hayek, 1945, 1974). Consequently, they failed to construct careful hypotheses. Their assumptions were testable, but also grossly unrealistic, and the results clearly illustrate the debatability of Friedman's assertion that the realism of assumption does not matter. Even Machlup's (1955, 1963, 1978) ideal type construct appears to be disconfirmed, because one cannot argue that African workers and farmers behaved "as if" they were maximizing leisure (laziness). They were not! An extra man-hour of unpleasant working conditions was simply not worth an extra dollar increase in the wage rate, perhaps and extra man-hour of production would mean oversupplying an already small market and thereby lowering prices.

Economic theory plays a huge role in economic research. But if hypotheses and their underlying assumptions are not carefully constructed, then economic theories lose their utility, which makes attractive Paul Feyerabend's (1981, Part 2) stance that the applicability and acceptability of a theory should not be judged on the basis of its age and familiarity (see Caldwell, ib., pp 76-92, 224-225 , Blaug, ib., pp. 43-44). Unlike wine which "sweetens" with age, theories are the world they represent; they age and die naturally with it. Methodological falsification of theoretical constructs (*truth realism*) that approximates reality (*world realism*) must recognize that theories exist in a large and dynamic universe within which, to survive, they too must undergo modifications, and hypotheses and assumptions underlying them must reflect such change (italics in parentheses are Maki's, also see Maki, 1988). For this reason empirical work is desirable, if not necessary, to validate and substantiate theoretical claims, otherwise economics is simply bad fiction, a rhetoric worse than that depicted by McCloskey (1983, 1985). The purpose of empirical work in economic research is next.

5. The Purpose of Empirical Work in Economic Research

Unlike Lakatos (1976, 1978) who argued that the scientific nature of a theory lies in its theoretical progressiveness, and that empirical investigations are pseudo-scientific, I think that the scientific nature of a theory finds is completeness and validity in its empirical implications. Using only mathematical analysis of spatial observations, astronomers of the past came very close to what we now know using the technologically advanced tools today. Still it would be unreasonable to argue for the cancellation of all space missions because we can figure out everything mathematically. Theories and empirical studies based on them are complements rather than substitutes. Economic theory guides empirical studies, but the latter tests the assumptions and conclusions deriving from the former. Even when one likes only one side of a coin, the other side still exists to complete the coin. Without empirical investigations there is no way to compare the assumptions and conclusions of a theory with observed facts. A science that does not enable technology and engineering ultimately advances human welfare only as bad sci-fi, even though scary fiction has its user value, too – well, I am being ridiculous for stress here.

A key basis for Lionel Robbins' (1932) assertion that empirical studies are useful only in the short term, that in the long term they do not lead to the discovery of empirical laws, is not clear [to me]. What is clear is that economic principles (laws) are hypotheses which have withstood empirical testing and earned acceptance on the grounds of the strengths of the empirical regularities they exhibit. Empirical testing does not necessarily have to be statistical; it may just be overriding evidence. Evidence is often colored by value judgments, but it is evidence, nonetheless. An illustration of a historical nature that comes to mind is Max Weber's (1998) hypothesis presented in *The Protestant Ethic and the Spirit of Capitalism*. Weber did not use modern statistical techniques to test that Protestants more than other people do indeed promote the growth of capitalism solely because of their religious ethic. However, his story is reasonable because he provided overriding evidence for it. In other words, he did not estimate the marginal contribution of a protestant to find high R-squared and significant t-values. In fact, the correlation between religion and economic progress continues to be bafflingly unclear economists to date.

However, Robbins hits the nail on the head by asserting that empirical investigations check the applicability of theoretical constructs to particular concrete situations. The applicability suggests subsidiary postulates to be used with fundamental generalizations. The results identify the components of pure theory that require reformulation and extension. In that sense Robbins was far ahead of Kuhn (1970) because for him the growth of scientific theory propagates itself through continuous empirical self-investigations and cross-investigations. For Kuhn scientific growth comes from the accumulation of internal anomalies as prime movers, along with intermittent revolutionary crises, depending on the seriousness of anomalies, and hence his formula that:

$$\textit{normal science} \mapsto \textit{internal anomalies} \ (\textit{occasional revolutionary crises}) \mapsto \textit{normal science}.$$

Advocating for empirical research is not ignoring its problems. For one thing, hypotheses may have philosophical and political undertones that bias economic researchers towards one position or the other. But, if those ideologies are treated objectively, Gunnar Myrdal would say, they must, empirical work can still be useful.

Blaug has said that questionable empirical testing cannot falsify a theory or hypothesis, and he is correct. The downside of Blaug's view though is that empirical studies that disconfirm well-accepted propositions are either not published or they are branded rebellious nonsense to be discredited. Putting down unfamiliar research results does not prove that empirical research is useless; all it indicates clearly is that the present methodological approaches need to change as they are oblivious to the old maxim that "all truth passes through three stages: First, it is ridiculed; Second, it is violently opposed; and Third, it is accepted as self-evident" (Arthur Schopenhauer, 1910).

Citing Thomas Mayer's (1975, 1980) seven criteria for a rigorous testing procedure, Blaug also points out researchers' unwillingness to be objective in falsifying their research implications. They do so by throwing out variables for which estimated coefficients have incorrect signs or are statistically insignificant (Darnell and Evans, 1990, Leamer, 1978, 1983, Zellner, 1996, Phillips, 1988). Emery N. Castle (1968) discusses some of the "threats to objectivity" in economic research, including the following five: (1) desire for approval; (2) advocacy for a particular public policy; (3) vested interests in a particular hypothesis, theory, or approach; (4) desire to avoid controversial problems; and (5) desire for personal financial gains (also cf Castle, 1980, 1983). Castle has returned to some of these issues with a 20/20 rearview in his *Reflections of a Pragmatic Economist: My Intellectual Journey* (2012). My only concern, now as then, is that Dr Castle seems to imply that individual researchers choose, or even prefer, to behave in these subjective ways, that there are no external pressures that affect the behavior of economic researchers. However, my concerns are relieved by additional questions Castle poses on (I) whether "economics as a discipline ... makes scientific inquiry possible," (ii) whether the construction of "refutable hypotheses is possible, given empirical observations," and (iii) "what the normative questions facing [researchers individually or collectively]" are, and whether what researchers find important to address is what society at large also values.. These questions indicate that Dr. Castle is aware of the impact of external forces on the behavior of individual researchers and their communities. In fact, he is line with Daniel W. Bromley (1997) who in "Rethinking Markets" questions the validity of Kenneth Arrow's (1963) assertion that the market mechanism is used 'to make economic decisions ...' (p. 1). Bromley shows how markets have had difficulty dealing with the environment, education, freedom, and even characterizing socialism and capitalism, and concludes that "markets ... simply reflect many individual choices which, once aggregated, hold social implications" (p. 1). However, the idea "that markets are a means for making social choices ... rests upon value judgments" (pp. 1-2)..

Empirical investigations present another problem, stemming from the distinction Kuhn (1970) calls "exemplars" and "disciplinary matrices". Symbolic generalizations (disciplinary matrices) control exemplars, well, at least influence them a lot. For example, it is plausible, perhaps even probable, to find graduate students (dealers in exemplars) pre-falsify their hypotheses. This happens because graduate students do not have control over their research projects both in terms of disciplinary authority and funding, and therefore it is quite possible for them to give their major professors what the professors want or expect, i.e., high R-squared, significant t-values, this about multicollinearity, that about auto-correlation and heteroskedasticity, goodness-of-fit, downward sloping demand curve, upward sloping supply curve, and so on – expected received stuff. Quite frankly, the work like that done by Dan Ariely (2008, 2012) could simply not be done in a regular economics program. Blame is not the issue; the issue is that empirical work is useful. However, if the research atmosphere does not permit and reward honest empirical work, then it is perfectly legitimate either not to do it, to be "honestly dishonest", or "dishonestly honest" (Ariely, 2012)..

The last of the problems afflicting empirical work that I can mention here is that some economists use *ad hoc* theorizing and adopt "immunizing stratagems" (Blaug's phrase) to do it. Marxist economists seeking to defend the tenets of Marxism are prime victims of this problem. There is an important relation between this problem and the problem of data mining and data use in economic research to which I turn to next.

6. Issues Relating to Data Mining and Data Use in Economic Research

On average economists have paid little attention to the processes for data generation until recently. This is in part because the positive methodology that dominates economic research today derives from the "first-principles models" common to the non-social sciences and engineering. However, without experimental data most economic relationships are difficult to model mathematically, while in many areas of science "there is currently a paradigm shift from classical modeling and analyses based on first principles to developing models and the corresponding analyses directly from data" – data mining (Kantardzic, 2003, pp. 1-2). To paraphrase, data mining seeks to advance prediction and description for the purposes of classifying, running regressions, categorizing (clustering), summarizing, assessing local dependencies, and/or detecting change and deviation in, the data.(Kantardzic, same pages). Data mining is not only relatively new to economics, it is also theory driven, especially in its imitation of physics (see Leamer, 1978, Lovell, 1983; cf. Jeans, 1981). This is a serious drawback, because "in order to give empirical content to economic models, data is required, [and] in economics almost all available data are of an observational nature; the data was not obtained by performing controlled experiments, but by passively observing economic reality. One of the consequences of limited possibility to experiment in economics is a gap between theory ... and the available data" (Feelders, 2002, pp. 1 and 3). The drawback has led to specifications of the kind Leamer (1978, 1983) and Spanos (1986) articulate, and some of these specifications are like the tail that wags the dog in that it is both common and tacitly accepted for "researchers to start from their favored theoretical model and 'patch' the model, for example by including additional variables, if the data do not agree (e.g., if a parameter estimate had the 'wrong' sign" (Feelders, p. 6). As Feelders (see, p. 9) suggests "using the data at hand for finding a good model and testing that model using the same data" has created what Hoover and Perez (2000) refer to as the "three attitudes towards data mining." The first attitude recommends avoiding data mining altogether, or to at least adjusting the data for errors. The second attitude holds that data mining is inevitable, but its dangers can be averted by use of the best available specifications, such as Leamer's (1978, 1983) extreme-bounds analysis. This attitude seems to indicate that model estimation and data mining are the same thing. The last attitude says that appropriately used data mining makes it possible to draw conclusions about the specific from the general; i.e., there is not one "true" theory; a good theory may be a result of many case studies. The truthfulness of these attitudes is not as important as the recognition that data matters enormously to economic research. Moreover, secondary data, which is most common to economics, may be incompatible with the economic theory being used. In such cases the results may not approximate economic reality even as they may demonstrate statistical reality, leaving both policy and further research not well-served. Also secondary data nearly always contain the value judgments of whoever collected them, how, and for what and whom. White, Wong, Whistler, and Haun (1990, p. 39) have the following insightful quote in their *Shazam Econometrics Computer Program User's Reference Manual*:

> The government are very keen on amassing statistics. They collect them, add them, raise them to the nth power, take the cube root and prepare wonderful diagrams. But you must never forget that everyone of these figures comes in the instance from the village watchman, who just puts down what he damn well pleases.

The quotation is dramatic, but its essence is enlightening. On the one hand it can be understood both as implying that theory is better for scientific growth than empirics. On the other hand it raises the question of why data collection takes place. Therefore to lower value judgments as barriers to entry in exclusive research enclaves, the use of primary data is necessary to filter the systematic bias that infects the results. Obviously, primary data alone will not eliminate values, but research will at least be aware of their presence. Moreover, the use of primary data will increase data banks, especially in developing countries where data is currently limited, and where good policy and research stand the best chances of improving the living conditions of billions of people.

Again data collection procedures may be inappropriate or even inaccurate, leading to measurement errors. Measurement errors influence the functional forms of economic models. Incorrect and misspecified forms give skewed results. Skewed results are not good for policy in addition to that measurement errors may also introduce other problems, such as serial correlation in time-series data. Both cases complicate correct inferences and accurate explanations and predictions.

Because the data economists normally use are weighted averages and raw aggregates, one cannot be sure whether or not economic explanations and/or predictions based on them are reflective of the real world, which in turn questions the validity of underlying theories. However, even if pure data were available, data is just data – massageable. This is the biggest and most common problem relating to data use in economic research. When the purpose of economic research is to defend a particular position, or to produce estimates known *a priori*, then data mining and data use are simply 'immunizing stratagems.' In this case Mayer's (1980, 2000) suggestion that researchers avoid over-specification searches (the 'con' that Leamer wants taken out of econometrics) by saving some of their data to enable retesting and replication is on target.

Utilizing various testing techniques is one solution to some of these problems, but this is often possible mainly for quantitative data. Qualitative data, especially when represented by dummy variables, leads to the well-known 'dummy variable problem'. In developing countries much of the economic research focuses on qualitative variables like education, so that there remains a need for techniques capable of handling such data (cf. Greene, 1990, Train, 1986).

Ideally data mining and data use should not present a problem to scientific progress as pure theory can advance science without data – 'theory without measuring' as opposed to 'measuring without theory'. For theory to work, what is needed are correct specifications of the initial conditions. But correct specifications can only be made if researchers spend more time with their own theories to understand them well. Predictions and explanations in this situation are still possible, but they might be too general without empirical content. It may be useful to gain correct general knowledge than incorrect specific knowledge (Hayek, 1945, 1974). The former contributes little to the fund of knowledge; the latter adds nothing, and may even stupify.

7. The CMP and Economics Teaching

In this penultimate section I comment briefly and haphazardly on the implications of the CMP for teaching economics. The CMP informs all academic endeavors because for many practitioners teaching and research methodologies are one integral 'philosophy of life'. Yet, to-date there are still few truly 'heterodox economics' graduate programs in the world. A Google search brings up only 38 first- and second-tier graduate economics programs, and a good many of these offer only a masters degree in economics. Bruce Caldwell (2011) has argued or "Why Economics Needs the History of Thought" as a way of reminding economists that the ideas they take for granted have a history – a history not quite scientific in the usual sense. Uskali Maki (see, e.g., 2008), who has perhaps thought and written about these things more than most, notices a significant improvement in the area even compared to the 1970s alone. Gaps remain, but economic ideas are more illustrative when one reads Robbins (1935), Eagly (1974), and so on than when one reads a bunch of beautiful reduced form equations.

I wish I could attribute to a personal name a kernel of wisdom one of my doctoral professors said to me having noticed how frustrated I was with the progress on my dissertation. He urged me to understand that at the time I had already completed the "doctor" in the "Ph.D.", but that the "Philosophy" also has to be earned. Obviously, it was not the answer I was hoping for, but partly because of it I never stopped wondering why research and teaching, methodologies and the philosophy of economic science are not required courses for a Ph.D. degree. The moral of this little personal anecdote is that it gives me an opportunity to end this essay like I started it. In its fervent attempt to imitate The Scientific Method, Economics has forgotten its social genealogy (cf. Camerer and Fehr, 2006, Shubik and Smith, 2006, Farmer, Shubik, and Smith, 2005). Both George Stigler (1984) and Ervin Laszlo (1972) have argue for the connectedness of disciplines along with specialization. In Chapter 2: "Specialism: A Dissenting Opinion" of his small book *The Intellectual and the Marketplace*, Stigler advices that "Let us all specialize a little ... Then we shall have a common meeting ground on which to have pleasant and profitable jousts of knowledge and wit, and on which we can form some estimate of the quality of a man"(p. 18). Both "pleasant and profitable jousts of knowledge and wit," as well as communication will not had, or will happen slowly and reluctantly, if one man knows everything there is know.

Stigler was talking to his fellow economists, but Laszlo was kindly brave to leave the following for all sciences:

> ... ideal specialization ... has led to the great advances in the sciences and technologies that now affect the lives of us all. ... The unfortunate consequence of such speciality barriers is that knowledge, instead of being pursued in depth and integrated in breadth, is pursued in depth in relative isolation. Instead of getting a continuous and coherent picture, we are getting fragments – remarkably detailed but isolated patterns. We are drilling holes in the wall of mystery that we call nature and reality on many locations, and we carry out delicate analyses on each of the sites. But it is only now that we are beginning to realize the need for connecting the probes with one another and gaining some coherent insight into what is there (pp. 3-4).

View ed from this point, it is questionable whether or not the gains in finesse are sufficient to justify the loss in moral imperatives. The net result leaves the policy implications of economic research weak, and demonstrates a double personality split within the economics profession. Half of the split is that all first-tier economics programs neither do research in nor teach economic methodologies and philosophy of science. The other split is that only a few senior economists openly indulge in normative economics, but all economists somehow draw normative conclusions from their positive hypotheses. Young economists continue to do either theory without measurement, or measurement without theory – much of the World Bank's work, especially with respect to Africa, is of this kind.

Again, Economics is in a tough spot because while the twentieth century has witnessed unprecedented improvements in the material conditions of nations worldwide, only a few economies continue to reap huge benefits, while the vast majority of the world population languishes in poverty. This need not be the case as economists' teaching and research interests, and the academic and personal pursuits (agenda) they suggest, derive from a strong philosophical conviction that everyone can live a decent life. Economics in general offers powerful tools for policy and research, that hold essential keys to human progress. Not only does knowledge of economics allow effective exploration and discovery of new sources of economic resources, it also enables efficient decision-making about the development, use, and management of resources. It is not an exaggeration to say that the gap in economic performance across economies is often the difference in the quality of decision-making, and the difference partly explains why some resource-poor nations (Finland or Japan) have fared better over history than resource-abundant countries (Democratic Republic of Congo). Still there is no escaping the fact the monocratic dominance of positivism is not entirely good for the profession.

I think of economists as *knowledge peddlers*, and teaching is one of the essential contents of their trade kit. The conventional view holds that a teacher is a transmitter of knowledge to the learner, and that it is up to the learner to understand the lessons assuming a threshold of normal capability (intelligence). However, teaching provides an excellent way to study, digest, and through repeated explanations to students, appreciate, the meaning and importance of ideas. Teaching yields new perspectives on old ideas, and a comprehensive knowledge of the field of one's specialization. The standard economic methodology just isn't conducive for this perspective on economics teachings.

However, within the CMP teaching emphasizes the underlying principles of economics rather than the simple mechanics necessary to pass an exam. Many bright students come to economics well-informed that it is one of the most quantitative of the social sciences. An equal or higher number of students come misinformed that all economists do is calculate, without much critical thinking for that matter. Students from the mathematical sciences are often surprised that they are unable to do economics math; students from the non-sciences are often shocked to find that economics is not a cold and heartless study. This situation affords the CMP teacher to give higher credit for a principle understood and well-stated than for a beautiful graph or memorized equation that lacks economic content, and is mathematically shallow and unproven. By making students responsible for their work, the CMP teacher encourages their intellectual confidence and independence. Thus, the teaching part of a CMP 'philosophy of life' emphasizes the understanding of knowledge in contrast to the simple supply of knowledge. Understanding knowledge fosters creativity, and knowledge accumulation is an inevitable by-product of creativity. As Albert Einstein said, "imagination is more important than knowledge," because "any fool can know; the point is to understand." In that respect critical thinking and problem-solving skills as well as sound application of knowledge are elemental. The job of a scientist is to simplify in order to clarify the complexity of real life. Thus, teaching is a rigorous, precise and honest exchange of ideas and the value of those ideas to both the narrow academic community that develops them and to the broad society that ultimately uses them. Hence, one-on-one conversations with students are an essential part of successful teaching and learning. It builds confidence in both the teacher and the student by taking the hard edge off the traditional lecture trap, which remains a good method for organizing instruction to large groups, but perhaps not so good for helping students understand what they know. Emphasis: The CMP teacher loves to help students understand and use knowledge, and he or she values pure economic theory just as much as its history, philosophy, and methodology, for theory is to practice what the map is to a traveler. Practice is alos indispensable to theory, for as Arthur Schopenhauer (1910) put it "Thoughts put on paper are nothing more than footprints in the sand; you can see the way the man has gone, but to know what he saw on his walk, you want his eyes."

8. Concluding Remark

The inability to reverse the direct effects of the GGR, and their negative externalities, on the global economy made economics and economists appear weak – dismal, again. The failure to predict the GGR as Krugman and a few others have suggested shows that the economics profession is in deep troubles. Part of the source of this trouble is the vulgar and cultish assumption of the long-run efficient market hypothesis. The assumption is incorrigible as it does not seems to take any counsel from Keynes's (1923) admonition that "... the long-run is a misleading guide to current affairs. In the long-run we are all dead. Economists set themselves too easy, too useless a task if in tempestuous seasons they can only tell us that when the storm is long past the ocean is flat again" (p.).

Two related problem sources are apparent. First is that economics has chosen to divorce its methodology from its foundations in the social sciences. Second is that the conventional methodology of economics is too rigid with respect to research and nearly absence in teaching. This essay describes both problems and the prospects for the CMP alternative. The problems are real; the prospects for CMP may well be only wishful thoughts and personal tendencies. Even so, both problems and prospects represent a fruitful area for further research.

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