Four Species of Reflexivity and History of Economics in Economic Policy Science[1]

This paper argues that history of economics has a fruitful, underappreciated role to play in the development of economics, especially when understood as a *policy science*. This goes against the grain of the last half century during which economics, which has undergone a formal revolution, has distanced itself from (what within the discipline was once known as) its 'literary' past and practices precisely with the aim to be a more successful policy science. Judged by the success of economics degree-holders to secure employment throughout government and finance this strategy has been unqualified winner.[2]

It is, however, less clear that during this period there has been stable/secure empirical knowledge of the major parameters relevant to policy.[3] For example, in September 2008 the Dutch Central plan agency predicted an economic growth of 1.25% for 2009 for the Dutch economy. In fact, the economy dropped by about 4%. (The miss is extraordinary because much of the Dutch economy is on automatic pilot; it suggests that the variance in the non-controlled part of the economy – dependent on world trade and international finance -- was enormous; it turns out that world trade dropped by 20% during the final months of 2008 and the start of 2009.) Most strikingly, the worst case scenario presented at a press conference, December 8, 2008 turns out to have been more optimistic than reality. It took the plan agency about six month to catch up with reality.[4]

The paper will motivate the thesis by identifying and distinguishing four kinds of reflexivity in economics. The main thesis of this paper is that because these forms of reflexivity are not eliminable, the history of economics must play a constitutive role in economics (and graduate education within economics). An assumption that I clarify in this paper is that the history of economics ought to be part of the subject matter studied by economics when they are interested in policy science. Even if one does not accept the conclusion, the fourfold classification of reflexivity might hold independent interest.

The paper is divided in two parts. First, I offer a stylized historical introduction to and conceptualization of the themes of this paper.[5] In particular, I identify various historically influential arguments and strategies that reduced the role of history of economics within the economics discipline. In it I also canvass six arguments that try to capture the cost to economics (understood as a science) for sidelining the history of economics from within the discipline. A sub-text of the introduction is that for contingent reasons, post World War II

1 This paper is the product of a long-standing on-going conversation with the economists M. Ali Khan, David M. Levy, and Sandra J. Peart. In addition this paper has benefitted from comments by and discussion with Robert Goldfarb, Michel Heijdra, Floris Heukelom, Maarten Schinkel. Finally, I thank Paul Roth for his encouragement and suggestions. The author is solely responsible for any errors.
2 We forget easily that economics is not the only contender: law, history, sociology, political science, and even organizational psychology are active competitors.
3 "Robert S, Goldfarb, 1997. "Now You See It, Now You Don't: Emerging Contrary Results in Economics," Journal of Economic Methodology, vol. 4(2): 221-44 ; see also Robert S. Goldfarb and H.O. Stekler (2000) "Why Do Empirical Results Change? Forecasts as Tests of Rational Expectations, " *History of Political Economy.*2000; 32: 95-116.
4 All these numbers are taken from a remarkable self-study : http://www.cpb.nl/sites/default/files/publicaties/download/voorspellen-crisistijd-de-cpb-ramingen-tijdens-de-grote-recessie.pdf, accessed on May 17, 2011. This study has received no attention outside the Dutch planning agency.
5 By 'stylized' I mean the simplification of a complex empirical fact; since Kaldor introduced the term into economics in the early 1960s, it has become a popular locution in that field: http://en.wikipedia.org/wiki/Stylized_fact

economics evolved into a policy science. Second, I distinguish between four species of reflexivity. These are used to then strengthen the argument for the constitutive role of the history of economics within the economics profession.

One final clarification/disclaimer: in what follows I focus only on history of economics not on economic history.6 While their fates in the discipline are often correlated, it seems that, especially in the wake of the most recent experience of bubble and financial crisis, there is some renewed interest in economic history within the economics profession. Having said that most of my arguments below in favor of the significance of the history of economics can be carried over to economic history.

I. Stylized Historical Introduction

While most practicing historians of economics are employed in economics departments, the profession has lost interest in maintaining what we may call a property right in its own history.7 It has removed history of economics from graduate training; its leading journals refuse to publish much work in the history of economics; specialist journals in the history of economics have reduced status and are in danger of being removed from all important indexing services. My generalizations cover the English speaking world, but as indexing tools become more widespread in bureaucratic evaluations of 'quality,' the serious study history of economics might – despite robust presence in teaching curriculum of some undergraduate institutions -- go extinct within economics more broadly. In terms of the intellectual division of labor, the dominant attitude of the economics profession to the history of economics is that the history of economics is suitable labor for some other discipline with which there is no need to trade. This attitude is not due to special insularity of the discipline; Economics does trade with mathematic, statistics, and increasingly computer science, and has, in turn, been a major exporter of intellectual content to other social sciences and humanities (known as economic imperialism).

The major justification of the economics discipline's collective attitude toward its history might be summed up by the view that the market in economic ideas is reasonably efficient in the long run; the institutional memory of economics – as represented by the content and techniques in graduate textbooks – has assimilated all the true or at least useful ideas from the past. This idea was propagated by the influential Chicago-economist (and Nobel prize-winner) George Stigler, who, paradoxically, was one of the last major economists who combined active research in contemporary economics and history of economics.8 From the appearance of Kuhn's *Structure* onward, Stigler aggressively used Kuhn's and Kuhnian ideas to propagate the claim that economics as a science with a paradigm has only limited use for its own history.9 So, just as Kuhn's critics in the 1960s feared (Feyerabend, Popper, etc), the

6 Having said that, my arguments about the history of economics can be extended to economic history. See, for example, Eric Schliesser (2009 "Prophecy, eclipses and whole-sale markets in Babylon: a case study on why data driven economic history requires history of economics, a philosopher's reflection," *Jarhrbuch für Wirthschaftsgeschichte*. Edited by Bertram Schefold.
7 Ivan Moscati has documented some recent trends on the disciplinary orentation of active historians of economics "More Economics, Please: We're Historians of Economics," *Journal of the History of Economic Thought* (2008), 30: 85-92, DOI: 10.1017/S1042771608000070.
8 "One need not read in the history of economics – that is, past economics – to master present economics," GJ Stigler (1969) "Does Economics Have a Useful Past?" History of Political Economy, 1(2): 217. The whole paper is worth quoting. .
9 I have documented this claim Eric Schliesser "Inventing Paradigms, Monopoly and Methodology at 'Chicago' Economics: Nutter and Stigler", (under review).

removal of history coincided with a tendency toward *theoretical-conceptual* uniformity. (This can be so even in the context of political pluralism within the economics discipline.) Of course, economics could be a-historical and still allow for a 'portfolio' of conflicting theoretical-conceptual approaches to the economy.

Stigler's position dovetailed with the view of his ideological and methodological opponent, fellow Nobel prize winner, Paul Samuelson (who also combined active research in contemporary economics and history of economics), the main architect of the formal revolution that has transformed professional economics since World War II (together with Arrow and Debreu) and no advocate of efficient markets (except, perhaps, in finance); Samuelson thought that much prior economics was worthless due to ambiguity of language and thought. He dismissively called it "literary." On this view the introduction of mathematics as the language of economics made much of the prior history of economics unnecessary and progress *possible*.10 Samuelson's view need not be associated with an efficient market hypothesis; all it requires is the sense that economics prior to the formal revolution was (a few exceptions permitted) a source of massive conceptual confusion. But it also requires the view that once a new foundation was built (his *Foundations of Economic Analysis* appeared in 1947), the discipline could operate without attending to its own history beyond the (potentially mythic) useful pedagogical tropes to be found in textbooks (the most important one, *Economics: An Introductory Analysis* (first published in 1948), written by Samuelson!).11 Of course, in practice Samuelson often returned to what he embraced as the Whig history of economic *analysis* in order to articulate or to put theorems of the past to test.12 But such attention to the past is nothing more than a privilege, if not duty, on the part of great masters as opposed to the "mere mortals," who should devote themselves to the more mundane task of problem solving.13

Even if one were to reject an efficient market hypothesis for ideas, one might strengthen Stigler's position by allowing that the market in economic ideas is full of 'failures,' but that the opportunity costs in devoting scare resources (graduate education, ongoing research, refereeing etc) to history of economics are larger than the benefits. Any old ideas that might be worth re-discovering at some time will be discovered by present and future economists.

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10 This attitude – without mathematics there is really just confusion; with mathematics progress is possible -- is very nicely revealed in Paul A. Samuelson's (1946) intellectual obituary of Keynes, "Lord Keynes and the General Theory," (*Econometrica*, 14(3): 187-200: "until the appearance of the mathematical models of Meade, Lange, Hicks, and Harrod there is reason to believe that *Keynes himself did not truly understand his own analysis* [in *General Theory*—ES]…the realization grew that the new analysis of effective demand associated with *General Theory* was not to prove such a passing fad, that here indeed was part of "the wave of the future,"" (188; emphasis added).

11 For a critical study, David M Levy & Sandra Peart (2011) «Soviet growth and American textbooks: an endogenous past» *Journal of Economic Behavior and* Organization 78: 110-28

12 See Paul A. Samuelson (1987) "Out of the Closet: A Program for the Whig History of Economic Science," Keynote Address at History of Economics Society, " 1987. *History of Economics Society, Bullet*in 9(1): 51-60. For very effective criticism, see Charles Kurdas (1988) "The "Whig Historian" on Adam Smith: Paul Samuelson's Canonical Classical Model, HES Bulletin 10(1): 13-24; I thank Nicholas Theocarakis for calling my attention to it. For a balanced survey of Samuelson's activity as a historian, see Steven G. Medema & Anthony M.C Waterman. (2010) "PAUL ANTHONY SAMUELSON: HISTORIAN OF ECONOMIC THOUGHT" *History of Economic Ideas* 18(3): 67-86

13 This last statement is not so easy to prove, but see Paul A. Samuelson (1972) "Jacob Viner, 1892-1970" *The Journal of Political Economy,* 80(1): 5-11; there Viner is elevated above the "mere mortals" and his historical mastery of economics is indulged (even praised). For a subtly different understanding of Samuelson's position, see Peter J. Boetkke, Peter Leeson, and Christopher Coyne "Contra-Whig History ofEconomic Ideas and the Problem of the Endogenous Past," http://ssrn.com/abstract=1686134.

This argument is sharpened by the reflection that right now there are probably more professionally trained economists employed in some research capacity than the whole number of economists (broadly understood to mean Men of Letters – and a few women -- who have reflected on political economy in some sense) throughout recorded history combined. The refined position can be summed up with a slogan: if there are ideas that are *worth* discovering at some time then some entrepreneurial economists will do so! On this view an economist may read a few old texts or books (online) in the course of her research or may use her a-historical training to re-discover the economic wheel.

Now, the *costs* of giving up property right in history of economics may after a while become largely invisible to economists. (For the moment I am leaving aside the possible costs to society of having economists as policy experts that may be ignorant of their own disciplinary history.) First, one minor cost may be that the history of economics is henceforth studied by folk without much training and acuity in economics. A second, more serious cost may be that once the history of economics is abandoned to other disciplines than economics, these may engage in revisionary and debunking stories about economics. (Something like this happened during the so-called 'science wars' of the 1980 and 1990s in heated debates over various histories of the medical and exact sciences.) If one gives up property-rights, one also gives up (quality) control. There is little evidence that economists find this a high price.

Moreover, once the history of economics becomes properly historical, it (the history of economics) makes the present safe from the past. That is to say, such a "properly historical" history of economics, however revisionary, is in no sense action-guiding. This relies on the – contingent -- way professional historians have come to conceive the nature and purposes of their discipline, of course, in the wake of the so-called linguistic turn. Within the history of economics community there is a very prominent group that aims to embed the study of the history of economics within science studies more broadly.14 Even history that 'problematizes' or 'unmasks' (say inspired by Foucault) often does not link up to ongoing research or fails to promote new lines of inquiry to the *professional economist.* So, a disciplinary division of labor in which history of economics is farmed out to the historians (and some other disciplines – sociology, political science, history, and philosophy -- that have overlapping histories with economics and may not have outsourced their histories entirely) can maintain the status quo quite nicely and thus is quite advantageous to those within the economics profession. Occasional adverse negative publicity is no fun, of course, but it does not change the more important incentives that structure the way economics is organized.

Two further costs may be more problematic for economics: third, without history of economics one might more easily mistake the current ruling paradigm as truth. This claim is supported by behavioral literature that reveals overconfidence among experts.15 To put the insight in modal terms: psychologically experts view the world – or the major commitments of the model -- as necessary rather than as possible. But as hinted above, by itself this is no argument for history of economics: it is just an argument for a portfolio approach to theories. Such a portfolio approach to theories is probably unlikely to occur (given current institutional regime and incentives), but is by no means foreign to economics as practiced today: economists are trained in and deploy wide number of techniques – game theory,

14 See, for example, the papers collected in Roy E. Weintraub (2002). Editor, *The Future of the History of Economics*, Annual Supplement to Volume 34 *History of Political Economy*, Durham: Duke University Press.
15 See for references to the behavioral literature and a nice case-study of economic-expert overconfidence, Erik Angner (2006) "Economists as Experts: Overconfidence in theory and practice," *Journal of Economic Methodology* 13(1): 1-24.

econometrics, computer simulation, experiments, etc – and a number of mathematical languages.

Fourth, unless one assumes that contemporary theories are (approximately) true, economists are in no position to understand why previous approaches (models/theories) were abandoned. This is due to the fact that much graduate training is now focused on keeping economists at the edge of the so-called research front, which is thought to make all research older than two years superfluous.16 Sadly, textbooks cannot be relied on to offer accurate historical account of changes in the disciplinary understanding. As we learned from Kuhn, their function is something different.17 Even if one is confident that the present family of economic theories/models is superior to those of the past, it is unlikely that our contemporary economic theories have secure knowledge of the major parameters that govern economic life (recall my comments above). In fact, for all kinds of reasons (that need not detain us here, but that are intimately connected to the reliance on statistical techniques) these days it is far too easy to 'test' and 'confirm' empirically within economics. Thus, there are few very stable empirical results—too much economic research is sensitive either to changes in our understanding of the relevant data-set and 'improved' statistical technique or changes in social circumstances that have made old claims outdated.

That is to say, while there is no doubt that empirical evidence often played a significant role in the displacement of discarded theories and models, it is by no means obvious that the new theories subsumed all the valuable insights of the discarded approaches.18 I am alluding here to two important features: i) what in the philosophy of science is known as a 'Kuhn-loss,' that is, insights of past theories that cannot be articulated or recognized by the new theory;19 ii) the fact that few empirical economic models ensure that they can recover the surpassed model as limit-case. I should note, as an aside, that my generalization does not hold for the development of (for lack of a better term) high-theory (that is, economics at its most mathematical). There great attention is paid to introducing mathematical structures with greater generality and expressive power, but that can recover the results of the earlier theory.20 Yet, in so far as high-theory has been relatively autonomous from empirical

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16 The *Locus Classicus* is Derek J. de Solla Price (1965) "Networks of Scientific Papers", in *Science* **149** (3683):510-515.

17 This is not an innocent observation : Kuhn's description of the practice in physics became a prescriptive norm in economics. For an important recent study of how economics textbooks (including Samuelson's) deal with historical, cultural, institutional variance, see David M. Levy & Sandra J. Peart (2011) "Soviet growth and American textbooks: An endogenous past," ***Journal of Economic Behavior & Organization*** 78(1-2) : 110-125

18 In 1946 Keynes' theory was understood as recovering the "classical system as a special case" (Samuelson 1946: 190). It is not clear if that judgment still holds. Later debate, for example, centred on whether to assume wage stickiness was the special case (used by critics of Keynes) or whether perfect wage flexibility was the special case (used by Keynesians who wanted to argue in terms of special cases);as Roger Backhouse, pointed out to me even this way of putting it is oversimplification and the discipline lost interest in proving which theory was the special case.

19 I started reflecting on the significance of Kuhn-losses by Hasok Chang (2004) *Inventing Temperature: Measurement and Scientific Progress* Oxford: Oxford University Press; in Eric Schliesser (2008) "Philosophy and a Scientific Future of the History of Economics" *Journal of the History of Economic Thought*, March 30: 105-116, DOI: 10.1017/S1042771608000094), I used the possibility of Kuhn-loss as an argument for the proper role of history of economics (and economic history in Schliesser 2009 cited above) in developing economic science. According to I. Votsis "Structural Realism: Continuity and its Limits" chapter 6 in P. Bokulich and A. Bokulich (eds) Scientific Structuralism, Springer, DOI: 10.1007/978-90-481-9597-8_6, the term "Kuhn-loss" seems to be coined by Heinz Post " Correspondence, Invariance and Heuristics" *Studies in History and Philosophy of Science*, 2:213-255.

20 For a stunning, profound analysis (it may be the only use of Derrida within the history of science that illuminates its subject) that deploys Critical Theory to illuminate the development of (high theory) within

evidence, the exception proves the rule. In IID below, I develop the significance of a so-called "Kuhn-loss" further.21

Now in two earlier publications, I articulated fairly limited further arguments why economics need to keep history within the discipline.22 Fifth, in so far as economics is an empirical science and its data are historical (i.e., derived from historical time-series/sources and deployed in particular conceptual-theoretical contexts) and economists wish to continue study, rely on, re-examine old-data sets they will need to understand the historical-conceptual framework in which that data is generated. This requires non trivial ongoing attention to history of economics (and economic history broadly understood). Sixth, and building on the first, if one wants to explore foundational issues in economics (e.g., about the relationship among institutions, major empirical parameters, social norms, and economic phenomena, etc) then it is useful to have access to and ability to work with data-sets that are furthest removed (spatially, conceptually) from our scholarly present. One way to achieve distance is historical. That way, one can also control for ideological and intellectual fashion. This is why I promote work on the economic history and conceptual views of civilizations that are antecedent to Classical economics.

These six ideas all present ways in which discarding the history of economics counts as a cost to economics as a discipline rather than costs to individual economists and society at large. In what follows I ignore the costs to individual economists.23 I am very interested in the costs to society of conceiving of economics in an a-historical fashion. But as my own fifth and sixth reasons above suggest, I approach these by way of an analysis of how to make economics a better policy science. (So, I may well ignore costs to society that are unrelated to my aim of furthering economics as policy science.) The way these matters are linked is fairly straightforward. From the Second World War onward, economics and econometrics have been self-consciously designed as policy sciences. For Tjalling Koopmans (Nobel 1975) econometric technique was to be justified in its ability to deliver policy advice.24

One might think that Milton Friedman's very influential account of economics as a "positive" science was precisely intended to keep economics 'pure.' To quote from the second page of his famous 1953 essay, "The Methodology of Positive Economics, might suggest this reading: "Positive economics is in principle independent of any particular ethical position or normative judgments. As Keynes says, it deals with "what is," not with "what ought to

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mathematical economics, See M. Ali Khan, (1993) "The Irony in/of Economic Theory" *MLN* 108(4), (French Issue): 759-803; DOI: 10.1353/mln.1993.0004.

21 A striking example of a Kuhn Loss within economics is provided by David Levy and Sandra Peart in their recovery of Melchior Palyi's now prophetic work on rating agencies, "Prudence with Biased Experts: Ratings Agencies & Regulators » http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1681609. Palyi, who was Max Weber's last student and taught at The University of Chicago in the 1930s (where he was the teacher of Rose Director, later Rose Friedman). Palyi also warned that financial instruments needed to be treated as near-money—a very prescient warning. Palyi (who became associated with crank ideas about Gold standard) and his insights (and the whole connection between Chicago economics and Max Weber) completely disappeared from disciplinary memory.

22 Schliesser 2008 and 2009 quoted above.

23 I do not mean to be facetious about this. The loss of training in reading books anchored in history and philosophy (as the Classical economists were) with complex arguments and rhetorical structure may well represent a psychological cost to some professional economists (who are trained on a diet of short journal articles).

24 See, for example, Koopmans' side in the celebrated (1947-1949) Koopmans-Vining debate; the papers are nicely available here: http://cowles.econ.yale.edu/P/cp/p00a/p0029.pdf, accessed on May 16, 2011.

be."”25 One might think that Milton Friedman is committed to the following analogy: positive economics should be more like pure astronomy and less like (social) engineering. Even so, Friedman’s essay does not hide the fact that *even* positive economics is a policy science. For the very next sentence following the two just quoted reads, “Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances.” With the rise of the modern Military-Industrial-Welfare state, one of the crucial change in circumstances is, in fact, changes in policy.26 Even the suddenness of the financial crisis is treated by the modelers as a consequence of policy.27

In what follows I draw on ideas of Kenneth Boulding, a once very prominent American economist (he is a past president of the AEA), to introduce a more thorough account of why history of economics matters to economics. In particular, because economics is a reflexive discipline in multiple ways, history of economics (and with it, its permanent link to philosophy) ought to remain a constitutive part of it. The argument that follows is meant to supplement and make more compelling, not supplant, the reasons mentioned above. I do so by way of distinguishing four kinds of reflexivity.

II: Four Kinds of Reflexivity in Economics and Econometrics28

In this section I distinguish four kinds of reflexivity. By ‘reflexivity’ I mean the way policy economists and the systems analyzed by their theories and models are coupled. This characterization is meant to capture the ways in which reflexivity has become an issue in recent discussions of the discipline without pretending to more precision than the topic can handle.29 For analytic purposes I pretend that these four kinds are entirely distinct, although in practice they can be blended and often are mutually reinforcing. I do not mean to imply these four exhaust the ways in which economics is reflexive; I focus on these four because they figure in my argument for why economics as a profession and as a policy science incurs a significant cost in ignoring its own history.

IIA: Economics as a policy science

The first way in which economics is reflexive is that the incentives of the economists qua economists as policy advisors deploying economic tools can be studied from within economics. In Book I of *Wealth of Nations*, Adam Smith already noted this feature of economics prominently in summarizing his historical survey (in Book IV) of economic theories prior to his own. (Smith was not innocent about this; his history of the discipline is polemical.) In more recent times the noted public choice economist, Gordon Tullock,

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25Friedman is referring to John Neville Keynes, the father of his ideological foe, John Maynard Keynes.
26 This is not the place to recount how Friedman defends a certain type of social engineering. But see Eric Schliesser (2010) “Milton Friedman, Positive Economics, and the Chilean ‘Chicago Boys,” *The Companion to Chicago Economics*, edited by Ross Emmett, Elgar, 175-195.
27 The fall of Lehman Brothers is taken to be a policy decision in http://www.cpb.nl/sites/default/files/publicaties/download/voorspellen-crisistijd-de-cpb-ramingen-tijdens-de-grote-recessie.pdf , accessed on May 17, 2011.
28 I focus on economics for the purpose of this paper, but these points can generalize to other social and human sciences.
29 John B. Davis and Matthias Klaes (2003) “Reflexivity: Curse or Cure?” *Journal of Economic Methodology* 10(3): 329-352, identify three kinds of reflexivity: immanent, epistemic, and transcendent. All four of the kinds of reflexivity I identify incorporate aspects of their three kinds.

stimulated research in the economics of economics. He points out that even if individual economists are truth-seekers, there are lots of other incentives that favor less noble aims among policy scientists (see, especially, chapter VII of *The Organization of Enquiry*).30 In particular, it is by no means obvious that the 'function' of policy science is to speak truth to power rather than, for example, facilitate bargaining among elite policy makers (by providing a shared framework) or providing rhetorical cover for, say, pre-existing policy ends or disarming complaints against the (potential) effects of policy. (These are not mutually exclusive, of course.)31

To give a stylized, but very real example: many countries have a bureau of economic planning or a budget agency that offer detailed policy guidance to the executive and legislative branches. These often do so by providing a menu of options accompanied with probability or uncertainty ranges. These agencies do so while being fully aware that policy makers will focus on a single number from the whole menu offered to them. In response, some agencies have started to supply a single number with a confidence interval, which, of course, also gets ignored by political elites (and the media).32 Yet, in their modeling they pretend as if they do not know of these practices!

Now this is not by itself an argument for the history of economics. But if one wants to understand and, thus, improve upon the *policy impact of economic theory* then one must study simultaneously, the economic theories available and used by policy-experts and makers, the policy changes these facilitated (directly or indirectly), and the consequences of policy changes.33 This process could be studied both i) comparatively between countries at a time (which might provide many natural experiments), as well as ii) historically within a (relatively) stable institutional framework. (These are not mutually exclusive, of course.) The second of these presupposes history of economics. The history of economics then becomes, in terms familiar to economists, endogenous. Such study would be a prerequisite if one were genuinely interested in building to echo Milton Friedman, "a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances."

IIB: Economics as a Culture

The second way in which economics is reflexive is in the formation of tastes. In a much cited

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30 "Not all of the advocates of tariffs, of course, are hired by "the interests." But the existence of people whose living does depend on finding arguments for tariffs and the further existence of another group who think that maybe, sometime in the future, they might need the assistance of either someone who believes in tariffs or an economist who is in this racket makes it possible for them to continue to publish, even in quite respectable journals. Thus a dispute which intellectually was settled over a century ago still continues." The point generalizes. Gordon Tullock, *The Selected Works of Gordon Tullock, vol. 3 The Organization of Inquiry*, ed. and with an Introduction by Charles K. Rowley (Indianapolis: Liberty Fund, 2005). Chapter: *CHAPTER VII: THE BACKWARDNESS OF THE SOCIAL SCIENCES*
Accessed from http://oll.libertyfund.org/title/1555/136952 on 2011-05-17

31 Roger Koppl (ms) "Experts and Information Choice" has coined the term "Information choice theory" to describe the burgeoning field that studies and models the relationship between incentives and experts.

32 I have shared this example with Dutch journalists and bureaucrats at the finance ministery and none objected to this characterization ; they did help me rewrite it to make it more precise.

33 The disarmingly frank self-assessment of the Dutch planning agency, http://www.cpb.nl/sites/default/files/publicaties/download/voorspellen-crisistijd-de-cpb-ramingen-tijdens-de-grote-recessie.pdf, accessed May 17, does not study the impact of the model on outcomes, nor does it study how the evolution of the model has impacted policy and outcomes. It does identify major lacunae in the model (in the Dutch case trade impacts).

(1968), but rarely read,34 presidential address, “Economics as a Moral Science,” Kenneth Boulding attacks the very idea that there is fundamental value unanimity, which, surprisingly enough, is one of the most deeply entrenched commitments of the new welfare economics and, thus, indispensable to the way policy science is practiced by economists since World War II.35 In the lecture, Boulding identifies a “heroic” ethic (and three kinds of it: “the military, the religious, and the sporting” (Boulding, 9) as a major alternative to the “economic (or “cost-benefit”) ethic. We do not need to accept his taxonomy, to appreciate his insight that the absence of disagreement over values is not a given for Boulding, but evidence of some conflict resolution mechanism (sometimes peaceful, sometimes violent). When economist optimize under constraints they abstract away from the (sometimes tacit) working of some such conflict-suppressing mechanism.

We can recognize what Boulding is after when we reflect on the significance of why he calls , the “Immaculate Conception of the Indifference Curve.” That is, the idea that taste (or some other shared value) is simply treated as given (2) For Boulding is deeply attuned to the idea that economics (in the guise of economic theory and economic policy) can shape the formation of tastes. He goes so far as to consider the possibility that economics can destabilize the values that are necessary for the very survival of a society in which it is possible!

The core idea is a simple one: ideas can have an impact. More controversially, core ways in how ‘we’ see ourselves (as consumers, members of households, firms, etc) and others (education as a form of human capital, culture as pleasing, etc) are shaped (sometimes mediated by state power) by economic theory in non trivial ways. Foucault’s famous lectures on neo-Liberalism emphasize this point in particular.36 Now the take-home message for most readers of Foucault is that economic objects are not ‘natural kinds’ but require a lot of construction.

My appeal to Boulding is meant to emphasize an alternative point. For while Boulding was an early, even incisive critic of what we may term, hyper-formalization of economics,37 he did embrace the idea that economics should be scientific enterprise (embedded in a larger extremely general approach: general systems theory).38 This point is that if economics wants to be a science that can track the shaping of values – and if Boulding is correct this must enter into the way it treats major parameters -- it must find a way to evaluate its own impact on the societies it studies. In order to accomplish this it must understand its own history and its complex relationship with the objects it studies.

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34 For those in the grip of a supposed contrast between economics and ethics, the title of Boulding’s piece is irrestible. But Boulding is echoing an older use of ‘moral science’ familiar to those with knowledge of the classics of the discipline and the history of philosophy.

35 The point was debated with ferocity by Stigler and Samuelson in 1943 in the pages the *American Economic Review.* I have traced its disastrous impact on the so-called Chicago Boys (in Schliesser 2010) and described its provenance to the neo-Weberianism of Talcott Parsons and Frank Knight. See Eric Schliesser (2011) “The Surprising Weberian Roots to Milton Friedman’s Methodology”, *Explanation, Prediction and Confirmation. New Trends and Old Ones Reconsidered*. Ed. by Dennis Dieks et al, Dordrecht: Springer.

36 Michel Foucault (2008) *The birth of biopolitics: lectures at the Collège de France, 1978-79,* London : Palgrave Macmillan

37 Kenneth E. Boulding (1948) “Samuelson's Foundations: The Role of Mathematics in Economics,”*The Journal of Political Economy* 56(3): 187-199. I thank M. Ali Khan for caling my attention to it.

38 Kenneth E. Boulding (1956) “General Systems Theory-The Skeleton of Science *Management Science,”* 2(3): 197-208.

IIC: Economics as Prophecy

The third way in which economics is reflexive is, in turn, divided in three kinds. Economics offers self-fulfilling prophecies, self-refuting predictions, and what I call philosophic prophecies.39 They are structurally akin, but describe slightly different aspects. All three are meant to capture the kind of priestly role of contemporary policy economics: public pronouncements by economists – preferably brandishing mathematical formulas that are almost impossible to understand for lay-people – play a kind of oracular function in contemporary social life.

A. "The self-fulfilling prophecy is," according to sociologist of science, Robert K. Merton, "in the beginning, a *false* definition of the situation evoking a new behaviour which makes the original false conception come true. This specious validity of the self-fulfilling prophecy perpetuates a reign of error. For the prophet will cite the actual course of events as proof that he was right from the very beginning."40 Economic phenomena prominently influenced the development of Merton's ideas (in context he is describing bank runs, among others). One can think of it as a kind of placebo effect on large-scale social phenomena. Among philosophers, Popper, especially, was fascinated by this aspect of theory.
   In recent years, the sociologist of knowledge, Donald MacKenzie, has investigated and made popular what he calls (in a self-conscious nod to J.L. Austin) "the performative" aspects of finance theory (in particular the rational option's pricing theory co-developed by Merton's son, the so-called Black- Scholes-Merton formula). In accord with the principles of the Edinburgh school in sociology, MacKenzie does not like appealing to truth and falsity, but prefers the language of construction.41

B. Self-refuting predictions build on and then reverse Merton's self-fulfilling prophecy: a true description of the situation evokes behavior which makes the originally true conception eventually come out false. An admirer of Popper, George Soros, has made this insight a core feature of understanding financial crises very popular in recent times. (He has also made the term, 'reflexivity' very prominent.) "Reflexivity asserts that prices…influence the fundamentals and that these newly-influenced set of fundamentals then proceed to change expectations, thus influencing prices; the process continues in a self-reinforcing pattern. Because the pattern is self-reinforcing, markets tend towards disequilibrium. Sooner or later they reach a point where the sentiment is reversed and negative expectations become self-reinforcing in the downward direction, thereby explaining the familiar pattern of boom and bust cycles."42

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39 This is not to deny the possibility of successful predictions (that don't meaningfully change the path of the underlying system). We can understand the important paper by E. Grunberg & F. Modigliani (1954) "The Predictability of Social Events," *Journal of Political Economy* 62: 465-78, and J.F. Muth's (1961) "Rational Expectations and the Theory of Price Movements," *Econometrica*, 29: 315-35, as characterizing precise (and rather infrequent) circumstances in which this can take place. As Wade Hands points out, Herbert A. Simon (1954) "Bandwagon and Underdog Effects and the Possibility of Election Predictions" *Public Opinion Quarterly* 18: 245-253 does something similar. See W. Hands (1990) "Grunberg and Modigliani, Public Predictions and the New Classical Macroeconomics" *Research in the History of Economic Thought and Methodology*, 7: 207-223. For discussion.

40 Robert K. Merton (1996, reprint) *On social structure and science* Chicago : The University of Chicago Press, 185

41 Donald A. MacKenzie (2006) *An engine, not a camera: how financial models shape markets* Cambridge, MA : MIT Press

42 Quoted from http://en.wikipedia.org/wiki/Reflexivity_(social_theory). For a nice summary of his views by Soros, see: http://www.sharpeinvesting.com/2007/08/george-soros-theory-of-reflexivity-mit-speech.html

C. Philosophic prophecies are structurally related to a self-fulfilling prophecy except that the outcome is a contingent fact, i.e., their existence is necessary, but not sufficient for the outcome. I distinguish nine features in it: A) It is 'secular' prophecy'. In particular, B) it is not about offering predictions (although these can enter into it), but about intending to help create a possible future. C) It is, thus, a necessary (but not sufficient condition) for the prophesied future (and, thus, resolutely teleological). D) It appeals to the imagination; often by way of narrative, history, or mythic history. E) It often consist of claims that are beyond knowable at the time of articulation, but that avoid obvious falsehoods. F) It is, thus, (ultimately) accepted on faith; G) it relies on the obvious idea that texts and ideas can have an impact ; H) our present, once unforeseen actions can be the intended outcome of past design. I) One sign that one is dealing with an author that engages this genre, is if s/he articulates a dialectic between "true vs false" philosophers (or experts).

For analytic purposes, I have pretended these three kinds of 'prophetic' reflexivity are distinct, but these can be blended, too. One can imagine that an economist makes a self-refuting prediction in order to evoke a contrary response. For example. Hayek's famous slippery slope argument in *The Road to Serfdom* may well have been proposed in order to promote a contrary reaction.43 It mixes philosophic prophecy (including a characterization of the false prophets of socialism, a historical narrative, etc) with a self-fulfilling predictions.

The existence of these three kinds of prophetic roles for economics in public policy produces a distinct argument for the role of history of economics that is very similar to a point (inspired by Boulding) made above. If economics wants to be a science that can track the major determinants that shape economic behavior it must find a way to evaluate its own impact on the societies and economic phenomena it studies. In order to accomplish this it must understand its own history and its complex relationship with the objects it studies.

IID: Economics in the Labyrinth

The fourth and final form of reflexivity that I wish to discuss involves a peculiar form of reflexivity. I call it "being in the labyrinth" by which I intend to capture conditions of Knightian uncertainty, that is, in conditions in which risk cannot be measured or estimated.44 That is one faces an environment in which features of reality that fall outside the model (or theory); there are in Donald Rumsfeld's fine phrase, unknown unknowns.

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43 A more complex example is Adam Smith's claim that "To expect, indeed, that the freedom of trade should ever be entirely restored in Great Britain is as absurd as to expect that an Oceana or Utopia should ever be established in it. Not only the prejudices of the public, but what is much more unconquerable, the private interests of many individuals, irresistibly oppose it." (WN 4.2XX ?) While this can be taken as a mere statement of fact, it is more likely intended to rouse (part of) the public to overcome the private interests of some. One reason to doubt that Smith is making a purely factual claim is that it is unlikely that he thought that there ever was an entire freedom of trade in Great Britain (he never makes mention of it in WN) ! I thank David Levy for these examples and discussion.

44 The *Locus Classicus* is F.H. Knight (1921) *Risk, Uncertainty, and Profit.* Boston, MA: Hart, Schaffner & Marx; Houghton Mifflin Company. Many more recent discussion start from Daniel Ellsberg (1961). "Risk, Ambiguity, and the Savage Axioms". *Quarterly Journal of Economics* **75** (4): 643–669. doi:10.2307/1884324. As Ellsberg documents, modern economists, most famously Arrow and Samuelson, have tried to displace Knightian risk by treating it is as a species of randomness. Even Nassim Nicholas Taleb (2011, 2nd edition) *The Black Swan: The Impact of the Highly Improbable* New York : Random House, which captures the phenomenon very nicely, generally treats events that fall outside the model as "highly improbably" (or as "fat tails," etc) rather than as simply beyond the concept of measurement altogether.

Recall that a ‘Kuhn-loss,’ is the phenomenon that some insights of past theories cannot be articulated or recognized by a new theory. These endangered insights may be nurtured by so-called heterodox approaches in a discipline or completely forgotten.45 By itself Kuhn-loss,’ is no argument for the history of economics. For, one can imagine an economist adopting what I have called a portfolio (or tool-box) approach to economic policy making. In some circumstances one adopts one model and in other circumstances one adopts another. But that assumes the discipline keeps track of various models and their performance under different circumstances. So, only if you combine ‘Kuhn-loss’ with a lack of knowledge of the past then one has a potential argument against the Stiglerian efficiency claim.

And at this point, reflexivity enters with a special vengeance. Because one must be able to locate oneself in the face of uncertainty over which model to apply and, then, be in a position to decide both the relevant historical analogue as well as the relevant, potentially discarded part of the portfolio of economic models. Now I do not deny that there are techniques that are supposed to guide model selection under conditions of uncertainty. But most of these (Bayesian, econometric) techniques insist to the best of my knowledge that uncertainty just is randomness and that begs the question against Knightian uncertainty. What these do techniques fail to do is teach policy advisors and makers to treat the models with less certainty.46

If, however, we allow the existence of real (so-called Knightian) uncertainty, then what is required, is not the application of a decision algorithm, but good judgment and something like the precautionary principle (i.e., “it is the responsibility of an activity proponent to establish that the proposed activity will not (or is very unlikely to) result in significant harm”).47 It exceeds my brief here to argue for the claim that history can teach good judgment,48 but it can be trusted to offer some of the best anti-dote(s) against ruling dogma and expert-overconfidence.

Brief Conclusion

The guiding assumption of this paper is that the history of economics can aid economics to become a wiser and even more scientific policy science. My approach to this has been an immanent one; this is why I have adopted an economic terminology. In this paper, I have canvassed a number of existing arguments for the claim that history of economics can improve economics; I have also offered a new, cumulative argument for this conclusion by diagnosing four kinds of reflexivity and their significance to economics as policy science. I do not deny that there may be opportunity costs to the discipline to make history of economics an essential part of training and research. But I have presented considerable evidence that after half a century of neglect of the history of economics, the forecasting

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45 The creative philosopher, Hasok Chang (op cit.) has even advocated a new role for philosophy and history of science to explore such Kuhn-loses in what he calls “complementary science.”

46 Charles Manski, an economist at Northwestern associated with the prestigious NBER, has a working paper, POLICY ANALYSIS WITH INCREDIBLE CERTITUDE:
http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1648007
It explores an important topic, namely the tendency of policy sciences "to regularly express certitude about the consequences of alternative policy choices." In the paper Manski offers a typology of variants of this problem. Manski promotes what in the body of the text I have called a portfolio approach to model and policy.

47 http://en.wikipedia.org/wiki/Precautionary_principle

48 But recall the closing lines of Q. Skinner (1969) “Meaning and Understanding in the History of Ideas,” *History and Theory*, 8(1): 3-53.

ability of policy economists is not impressive. So, it is not clear how much would be lost if some of the existing core requirements in the training of economists would be sacrificed.

In particular, I have avoided arguments that appeal to the private and social costs and benefits of the a-historical character of economics. In particular, I have not addressed the private costs to, say, the aspiring economist who (for the sake of argument) accepts my diagnosis. It is true that my argument to the aspiring policy scientists offers a risky bit of advice: "if you incorporate history into your scientific practice you will be a better and, perhaps, more marketable scientist." This has not been proven. But I hope I have made it *possibly* true.

Finally, given the significance of so-called economic thinking in the modern, bureaucratic industrial-military-welfare state it follows from my argument that there are significant social costs, too. But to articulate one most drop the immanent approach.

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