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Economic History and the Rebirth of Respectable Characters

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How long can irony and cynicism sustain the economics profession? When will we see the rebirth of the Intellectual, the Social Activist, and the Teacher as respectable characters in the world of economics? (Arjo Klamer, 1990)

I've been asked to name some contributions that economic history can make to the Post-Autistic Economics Movement. The occasion made me think of the questions Arjo Klamer asked in *The Making of an Economist* (Westview Press, 1990: 185), his study with David Colander of graduate students at the universities of Chicago, Columbia, Harvard, MIT, Stanford, and Yale.

I thought of Klamer's question - - how long till the rebirth? - - because in America, the study of economic history was killed off with the Intellectual, the Social Activist, and the Teacher. The timing was ironic. I am not referring to the literal killings in Paris or Budapest or Mississippi (though the connection is worth exploring).

The irony is that when Harvard cut a full-year of history from the core of its graduate program in the 1960s (a fashion that was completed at most schools, including Chicago, by the mid-1970s), economic history was simultaneously and radically transforming.¹ Historians at Harvard and Purdue were its prime movers. It was a fantastic re-invention of the field, and brought - - as such things go in the human sciences - - a new methodology, a

change of guard at the journals, and a large increase in output, prestige, and resources. In 1994 two inventors of "the new economic history," Robert Fogel and Douglass North, were awarded the Nobel Prize. Tragically, many economists could not say why.

Economic history, then, is in one story a victim and a failure. As Deirdre McCloskey put it, the new economic historians had spent most of their energy explaining to departments of history the "wonderful usefulness" of economics.² But they forgot to sell their wares to their own hiring and curriculum committees--in economics. (McCloskey's article was published in 1976 in the *Journal of Economic Literature*. She tried to stop George Stigler from taking history out of Chicago's curriculum.) Economic historians continue to speak in the wonderfully useful language of statistics and constrained maximization. But to the Samuelsonians of the Seventies who crafted the curriculum of micro-macro-and metrics while fetching money to mathematize economics, the numerate historians' talk of politics, religion, institutions, open fields, lacks of freedom, legacies of slavery, narrative voice, contested meanings, census manuscripts, personal diaries, and plantation account books was, to use a technical term from the sociology of science, "humanities crap." A real economist was a Problem Solver, a calculus wonk.

Economic history, then, like foreign languages and the history of thought, was killed when the Problem Solvers killed the Intellectual, the Activist, and the Teacher--"the respectable characters." It's difficult to imagine a re-valuation and legitimization of these social roles - - so central to a post-autistic economics - - without a simultaneous revival of historical inquiry.

Now it's true that some people, loyal to the new Chicago School, have called the ahistorical Problem Solver, Robert Lucas, an intellectual. If you begin with standard Samuelsonian assumptions, then yes, Lucas is. If an intellectual is someone fastened to the belief that there is one way of doing "operational" economics "consistent with" real "science," if an intellectual (in published work) has read mainly in some corners of engineering mathematics and rational choice, free market economics, if an intellectual is a person who does not value social or cultural history as a mode of economic understanding, if an intellectual is someone unwilling to argue in his own seminar his privileging of a simple utilitarian social welfare function (for example, in Iowa City, Iowa, in 1994), then yes, Lucas is definitely an intellectual. Similarly, under Samuelsonian assumptions, Robert Barro is a Teacher, George W. Bush is an Orator, and Gary Becker is a Social Activist (in Tantric healing).

In other words, one contribution of history to post-autistic economics is this: valuing economic history for the serious economics it is (while retaining what it is now perceived to be: serious history) will hasten to economics the return of the lost and wandering tribes of respectable characters. (One example of the potential gain can be found in Nicholas Dawidoff's *The Fly Swatter* [New York: Random House, 2002]. It's an amazing and sad story of the great economic historian, Alexander Gerschenkron, who was twice forced to wander.) A group of French students in the post-autistic movement have suggested a new curriculum, setting theirs against present-day Chicago (PAE Review 4, 29 Jan 2000). They propose to put economic history back in to the core curriculum.

Why should a post-autistic economist study history? McCloskey's rubrics from 1976 provide some of the answers:

History has more facts. When today's economists begin a paper on America's Welfare Reform Act of 1996 they of course introduce the subject historically. But because they do not collect facts from history they get the facts wrong. The

Institute for Research on Poverty (IRP), which is meaningful to me for many reasons, and partly because my brother is a research affiliate with them, is unfortunately a good example. The IRP believes that poverty and the collective strategies to eradicate it began with President Johnson's War on Poverty. (To be fair, some will refer to the Great Depression. But their data sets still begin in the 1960s.) America has had poverty and public assistance since the Elizabethan Poor Law of 1601.³ In the late nineteenth century the largest cities abolished "public outdoor relief"--the tax-financed subsidies in cash and in kind. Abolition was part of the charity organization movement, a British import that attempted to privatize, moralize, scientize, localize, and personalize poverty and charity. It's an open secret that the 19th century experiment inspired today's Republicans to abolish entitlements. Data on the failed movement are voluminous and contain evidence relevant to the Act of 1996.⁴

History has better facts. Economic historians - - for example, Simon Kuznets, Eli Heckscher, John R. Commons, Lance Davis, Stanley Engerman, Jeffrey Williamson, Susan Carter, Richard Sutch, Roger Ransom, William Sundstrom, Gavin Wright, Warren Whatley, Claudia Goldin, Robert Margo, Emily Mechner, Elyce Rotella, Lee Craig, Ann Carlos, Dora Costa, Fernand Braudel, Joel Mokyr, Yasukichi Yasuba, Jean-Laurent Rosenthal, Pierre Bourdieu, Paul Baran, Paul Sweezy, Nancy Folbre, Kyle Kaufmann, Thomas Weiss, Sam Williamson, Jeremy Atack, Rick Steckel, John Murray, Joerg Baten, William Collins, George Selgin, Robert Higgs, Price Fishback, Shawn Kantor, Hugh Rockhoff, Peter Lindert, Avner Greif, Joan Hannon, Robert Humphreys, George Boyer, Mary MacKinnon, Timothy Hatton, Cormac O'Grada, Richard Easterlin, Gus Giebelhaus, Metin Cosgel, Mary Beth Combs, and Santhi Hejeebu - - collect their own facts. You can see that it's the industry standard. Laboring in the archives yields an intimate knowledge of the scope and limitations of facts collected. Downloading a *.gif file from www.economagic.com does not. In our second look at significance testing in the *American Economic Review* (this time we examined the 1990s) McCloskey and I found that among all the subfields of economics, the historians and labor economists pay most attention to the economic significance of their estimates.

History yields better theory. What caused the Great Depression? A Problem Solver in the mid-1990s put to paper one answer, and gave it to me: "a technology shock in a real business cycle model." There is in truth little consensus. But Milton Friedman, Anna J. Schwartz, John Kenneth Galbraith, Peter Temin, Charles P. Kindleberger, Barry Eichengreen, and many other historians have advanced the theoretical conversation by insisting their theory connect with actual world events.

History makes better policy. The history of welfare is a case in point: "time limits" do not produce self-sufficient wages. But then, *most* economic policy is a case in point. "The competitive supply of professional services in the nineteenth century, it is said, grievously injured consumers, justifying official cartels of doctors and undertakers." So midwifery and home birth have been virtually outlawed in the United States. "If marijuana were legally and competitively supplied there would be a huge increase in demand for it." Hey, I mean, look at that Robitussin go! "The United States will not lift the embargo on Cuba," President Bush told a crowd in Miami in Spring 2002, "because that would make Castro rich and therefore more difficult to remove." *I just got here*, you can almost hear him dreaming. *C'mon, America, give embargoes a chance.*

The rhetoric of Problem Solving needs revising. What is the point of emphasizing the size of the *t*-test, or formalizing the set of pooling equilibria, if history shows that you are solving

the wrong problem? A non-experimental science ought to look at real world experiments when nature coughs them up. For example, economists have much to learn about policy from East and West Germany by looking at them when they were together, then separate, then together again. Likewise in South Africa and in Palestine. The labor economists David Card and Alan Krueger were not the first to see 'natural experiments' in the adoption of minimum wage legislation; the method is old and historically sound. Still, the laboratory of history is strangely under-utilized by the Problem Solver.

History makes better economists. Arjo Klamer and David Colander asked their sample of graduate students to name the "most respected economists" (p. 42). At every school except Chicago, at least half the heroes listed (there are no women on the lists) did their significant work in historical economics: they are Smith, Marx, Veblen, Keynes, Hicks (part-time), Schumpeter, Myrdal, Polanyi, John Kenneth Galbraith, and Friedman. And still others on the list, such as Boulding, Sen, and Stigler, were deeply historical in the way they conceived of economic problems. Economic history is apparently a major field of inquiry for the world's most respected economists.

Economic history is their major field because history offers more facts, better facts, better theory, and better policy. But the reasons for bringing history back in exceed those that McCloskey raised against Stigler. Since that time a small but growing band of economists and historians have allowed discourse, feminism, postmodernism, and classical rhetoric to affect their work. Like feminist economics and economic methodology, the conversations of economic history are now more open and pluralistic. (Easy does it, Clio: it's not like trade theory but we have a long way to go.) History provides the alternative stories that give meaning to timeless models and "obvious" nulls. History exposes the contested meanings of utility, labor, freedom, and justice. History keeps us honest in our assumptions. History connects teachers of economics to the concerns of the humanities. History connects teachers to the concerns of minority and international students, and it connects students to the assumptions and the graphs. For example, when I introduce undergraduate students to externalities with Upton Sinclair's *The Jungle* (1905), or to comparative advantage with Steinbeck's *The Grapes of Wrath* (1939), or to labor economics with *The Philadelphia Negro* (1899), by W.E.B. Du Bois, it is no surprise that women and students of color become differently engaged.

How can we bring history back in? First: realize that Chicago and Columbia do not set world prices for economic education. But when others think they do, distortion--autistic economics--emerges. In fact, in another story, economic history is not dead; it's *already* back in. Today's core curriculum at Harvard, MIT, Stanford, Berkeley, and Northwestern University requires from Ph.D. students a satisfactory grade in a course on economic history during the first or second year of study. It's not like Gerschenkron's Harvard--a full-year of history--and it's a lot of micro, macro, and metrics; but clearly, in many of America's elite programs, economic history is still inside the core. What can we do? History stays out of the curriculum when the Problem Solvers say "that's not what MIT does." Show the problem solvers that they are wrong. Let it be known, moreover, that economic historians are the department chairs at Harvard (Jeffrey Williamson, 1997-2000), Stanford (Gavin Wright, currently, and for a second time), MIT (Peter Temin, 1994-1997), Northwestern (Joel Mokyr, 1999-2002), Arizona (Price Fishback, currently), and elsewhere. These Department Chairs are on your side.

Second. I say we call out the Chicago-Columbia-Harvard-MIT-Stanford-and Yale Ph.D.s who as students had spoken honestly with Klamer and Colander and are now writing and

teaching as junior or tenured professors. It's time they speak up and say who they are.

Of the 212 respondents to Klamer's and Colander's survey in the late 1980s, 98% said that a study of history was at least "moderately important" and 68% said that history was of highest importance (Klamer and Colander 1990, Table 2.1, p. 16). Let me make a plausible out-of-sample observation. Not long ago a knowledge of history was ranked by today's professors of economics as being of highest importance to the skills of a good economist, second in importance only to mathematics. (Seventy-three percent [73%] said that mathematics was of highest importance [p. 16].) If the respondents have changed their minds, if a knowledge of what Gerschenkron called "economic backwardness in historical perspective" is not useful, if a knowledge of the railroads, or the Poor Laws, or the Beveridge Plan, or the gold standard, or slavery, or Jim Crow, or the East India Company, or women's suffrage, or public education, or world war, or free immigration, or markets before central banking is, they believe, no longer important, they will of course agree to defend their change of mind at next year's ASSA meetings. These are nameable professors who could help to rescue the young from the autism of the middle and to repopulate the world of economics with respectable characters. These professors are acquiring the power to change the demand curve. They can refuse to vote for the pseudo-mathematician, famous for formalizing nothing of consequence. They can hire economists who care about the world and its many ways of knowing, and who show it in their teaching and their scholarship. They can fill the pages of economics with the image they had of themselves when they were happy.

But I am sorry to say with Nike that an important way to bring history back in lies solely within you--the obligation to just do it. There is a simple proposition that clarifies my point. If you are going to change the conversation, you have to change the conversation. Inspired by the critical pedagogy of Paulo Freire, the African American writer and English professor, bell hooks, has made a similar point in *Teaching to Transgress: Education as the Practice of Freedom* (1994). On the second day of classes I discuss with my students her Chapter 10, "Building a Teaching Community," which is a dialogue with a white male philosopher on the history and style of power and knowledge in the classroom and how to change them. Students find the dialogue to be inspiring (though sometimes unnerving) for claiming their own power, finding their own voice, in my classroom. Students for a post-autistic economics could take "education as the practice of freedom" as a second motto, a kind of just-do-it.

Education as the practice of freedom means taking graduate courses in history and other historical sciences, such as philosophy, biology, anthropology, or communication studies, and then putting your questions to your teacher, your dissertation, and your seminar speaker. It is simply not true what department chairs say, and repeat, with liturgical command, "that there is no time for those courses." Insisting to the young "that there is no time" is at best an example of blackboard economics (but the costs are of course higher than that). Ask, What did Alex do? Your courage to forge your own path will inspire others to do the same. Conventional teachers will be angered and embarrassed by their ignorance and by the fragility of their top-down and consumerist metaphors of power and knowledge. Who cares. Science is criticism. They should learn to take it. Your teacher of labor economics may bark you off the podium when you reveal to your classmates the private fantasies and the racist histories of black people and public assistance that you found in *The Bell Curve*. Big deal. How long should irony and cynicism rule the economics profession?

Notes

1. I thank Deirdre McCloskey and Jeffrey G. Williamson for helping me to put the economic history requirement in historical perspective. Errors are my own.
2. Deirdre N. McCloskey, "Does the Past Have a Useful Economics?" *Journal of Economic Literature* (June

1976), 434-61. Reprinted in R. Whaples and D.C. Betts, eds., *Historical Perspectives on the American Economy* (Cambridge University Press, 1995), Chapter 1, p. 31.

3. S.T. Ziliak and Joan Underhill Hannon, "Public Assistance: Colonial Times to the 1920s." Forthcoming in S. Carter, et. al., eds., *Historical Statistics of the United States: Colonial Times to the Present* (Cambridge University Press and U.S. Bureau of the Census).

4. S.T. Ziliak: "The End of Welfare and the Contradiction of Compassion," *The Independent Review* (Spring 1996); "Some Tendencies of Social Welfare and the Problem of Interpretation," *Cato Journal* (Winter 2002); "Pauper Fiction in Economic Science: 'Paupers in Almshouses' and the Odd Fit of *Oliver Twist*," *Review of Social Economy* (June 2002).

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Why Neoclassical Economics Explains Nothing At All¹

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Introduction

Critical realists (*c.f.* Lawson 1998; Fleetwood 1999a&b, 2001a&b) argue that neoclassical economics is rooted in the *deductivist method*.² Deductivism seeks to 'explain' something by deducing or predicting a statement about that something from a set of initial conditions, assumptions, axioms and a covering law and/or some other form of constant conjunction of events which drives the inferential machinery. These conjunctions, where for every event y there exists a set of events x_1, x_2, \dots, x_n such that y and x_1, x_2, \dots, x_n are regularly conjoined, only occur in, and are constitutive of, *closed systems*. There are, however, very few spontaneously occurring closed systems in the natural world, and virtually no non-trivial ones in the socio-economic world. Using deductivism, therefore, means engineering artificially closed systems by means of *known falsehoods*, reducing neoclassical economics to what Hodgson (1999: 11) calls 'the economics of nowhere'. What is not always appreciated, however, is that *the presence of known falsehoods removes all explanatory power from neoclassical economics*. This paper illustrates the point via a brief analysis of the theory of labour demand.

Closed system theorising: the example of the theory of labour demand

The law of labour demand, which states that the quantity of labour demanded varies indirectly with the (real) wage, is an example of one kind of constant conjunction of events - a Humean law. Incidentally, the significance of this apparently arcane law should not be underestimated: it underpins the entire neo-liberal project of making labour markets more flexible. This constant conjunction is artificially engineered via (at least) four assumptions known as the Marshall-Hicks conditions. Demand for labour is alleged to be more elastic if:

1. *The elasticity of substitution between labour and capital is high.* The demand for labour will be more responsive to a change in wages the more easily labour can be substituted for capital.
2. *The elasticity of demand for the final product is low.* The demand for labour

will be more responsive to a change in wages if the cost increases caused by wage increases can be passed directly to the consumer without a loss of revenue.

3. *The share of wages in the total cost of production is high.* The demand for labour will be more responsive to a change in wages if production is labour intensive because wages constitutes a relatively high proportion of overall costs.

4. *The elasticity of supply of other factors is high.* The demand for labour will be more responsive to a change in wages if, when capital is substituted for labour, the suppliers of this additional capital are able to increase their supply immediately and effortlessly.

The M-H conditions close the system: without them there would be no constant conjunctions between changes in labour demand and changes in the wage. Unfortunately, however, the M-H conditions also introduce *known* fictions into the theory. Let us consider the four M-H conditions as four *closure conditions*.

Intrinsic closure conditions (ICC)

To close the system, the *internal state* of individuals must be artificially engineered so that the individual (person, production system, firm or whatever) *always* responds in the same predictable way. The ICC is maintained, for example, by assuming ubiquitous substitution between labour and capital.³ Where production involves a relatively fixed crew of workers operating a relatively fixed set of machinery it is often impossible to substitute a worker for a machine. Where production requires human emotion such as a helpful attitude, a machine cannot be substituted for a human. In some cases substitution of labour for capital is not feasible: how does one substitute a machine for a nurse to carefully bath an elderly patient? Even where it is technically possible, substitution is often not socio-politically possible. If, however, the ubiquity of substitutability is *not* assumed, a change in relative factor prices cannot be said to cause the substitution of labour for capital, and the constant conjunctions of events that constitute the law of labour demand fail to emerge. Assuming ubiquitous substitution is a falsehood. Where non-substitutability *is* recognised it is treated as a special case. Knowingly false claims are, thereby, treated as the norm, and knowingly true claims (e.g. that firms may offset a legislated wage rise by introducing flexible working practices that raise efficiency and reduce costs) are relegated to an afterthought.

The extrinsic closure condition (ECC)

The ECC ensures that the system is completely isolated from any *external* influences that would violate closure. This occurs by assuming things like: (a) the suppliers of other factors of production can increase their supply immediately and effortlessly should need arise, which is unrealistic, especially when the economy is running near to capacity; and (b) the elasticity of demand for the final product is low so that firms can pass wage increases onto customers, which is a rather tenuous assumption in highly competitive global markets. Maintaining the ECC, then, often requires falsehoods.

The aggregational closure condition (ACC)

The ACC ensures that the response remains constant, irrespective of the level of aggregation. Hence, the need to assume that the share of wages is high no matter what the industry. If for example, the industry was, or became, highly capital intensive, an increase in wages might be lost in the overall costs and demand for labour might not fall following a wage increase. In capital-intensive industries, then, the ACC is a falsehood.

The reducibility closure (sub) condition (RCsC)

The RCsC requires the existence of assumptions whose *sole purpose* is to ensure mathematical tractability. These are merely technical assumptions used to ensure the relevant functions are well behaved, thereby preventing perverse outcomes. Even where substitution of capital for labour is possible, it is often not continuous or 'smooth' but 'lumpy'. Production functions would have 'lumps' in them and could not be differentiated.

If any of these four closure conditions are not met (and there are, of course, more ways of meeting them than mentioned here) constant conjunctions will not emerge. Incidentally, that there are *four* M-H and *four* closure conditions is merely coincidence. Moreover closure requires far more than the M-H conditions: the latter are merely those explicitly mentioned in the theory. Other assumptions lie buried within the *ceteris paribus* clause; are attached to sub-components of the theory, such as diminishing marginal returns; or are made by omission.

Neoclassical economists *know* perfectly well that they are using falsehoods (hence the reference to *known* falsehoods) but often ignore the *causes* and *consequences* of constraints on their freedom to choose the M-H conditions, or assumptions in general. They *cannot* choose assumptions on the grounds of their truth-likeness because the need to maintain systemic closure often overrides these (and other) considerations – such as descriptive adequacy. Faced with a decision between adopting an assumption that is known to be false yet closes the system, and one that is known to be true yet violates closure, the known falsehood *must* be chosen or the constant conjunctions will not emerge.

Removal of explanatory power

A damaging consequence of adopting *known* falsehoods is that their presence removes explanatory power, for (at least) three reasons - on explanation see Runde (1998).

Explanation is not merely efficient causality

Many critical realists share with Lipton (1993; 33) the thesis that to 'explain a phenomenon is to give information on the phenomenon's causal history'. The causal history of a phenomena is not merely (if at all) one couched in terms of the event(s) that precede the phenomena, but in terms of the underlying causal mechanisms. One does not, for example, adequately explain (the event of) a lamp becoming illuminated simply by pointing to the (event of) flicking of the switch that preceded it. One does not adequately explain an increase in the demand for labour by pointing to the fall in wages that (allegedly) preceded it. Yet this *form* of 'explanation' is all deductivism offers. The overriding necessity of closure requires the removal (theoretically of course) of all causal mechanisms that might violate the closure conditions. So, for example, the theory of labour demand omits reference to trade unions, the introduction or abolition of labour laws and responses to them, government policy, political ideology, management systems, different working practices and so on, mechanisms that have considerable causal impact on labour demand. But here is the rub: once removed from the theory these causal mechanisms cannot subsequently be recalled and offered as part of the causal explanation. Relevant causal mechanisms are either included in the theory, in which case they can contribute to the causal explanation, or they are excluded, in which case they cannot.

Explanation is not prediction

Prediction does not constitute explanation. The conflation of prediction and explanation is

referred to as the 'symmetry thesis' whereby the only difference between explanation and prediction relates to the direction of time (Caldwell 1991; 54). Explanation entails the deduction of an event after it has (or is known to have) occurred. Prediction entails the deduction of an event prior to (knowledge of) its occurrence. One can, however, predict *without explaining anything at all*. One can predict the onset of measles following the emergence of Koplic spots, but the latter does not explain measles. Even supposing an econometric model successfully predicted an event (and the predictive power of neoclassical economics is arguably weak), the regression might be grounded in no economic theory whatsoever, or be grounded in a theory that contains known falsehoods. In this case, even a successful prediction would not constitute an explanation.

Explanation does not allow known falsehoods

If, as part of a causal account, one includes a *known* falsehood, or, leaves out some important causal mechanism (falsehood by omission) then the 'explanation' can immediately be rejected as a *bone fide* explanation by pointing to this falsehood. Consider an analogy. In explaining how my rubbish bags got ripped during the night, I might hypothesise that it is the work of a fox or I might hypothesise that it is the work of a ghost. The explanation involving the fox is advanced because I believe it to be true. The 'explanation' involving the ghost is *known* to be false but is advanced for a pragmatic reason: I want to frighten my young nephew and stop him playing with the bin bags. Whilst the explanation involving the fox is valid (even if it turns out to be mistaken) the 'explanation' involving the ghost, pragmatically useful as it is, is invalid because it is *known* to be a falsehood. One only has to reflect upon this for a moment to see this conclusion is self-evidently correct: if known falsehoods are allowed to constitute 'explanations' imagine the bizarre explanations that could be advanced!

Counter arguments considered

Two counter-arguments are commonly deployed to legitimise the use of known falsehoods. The first runs as follows: 'all theory has to leave out the inessential, has to abstract from reality, has to make unrealistic assumptions, so all theory is inevitably false in the strict sense of the word'. Now whilst abstraction is legitimate, the process is complex and cannot be elaborated upon here (*c.f.* Sayer 1998). I defend my claim with the following observation. Theories like that of labour demand are replete with such obvious falsehoods that to suggest they are really (legitimate) abstractions is merely a rhetorical ploy to avoid methodological discussion. In any case, as noted above most neoclassical economists admit to knowingly using falsehoods. The second counter-argument invokes the 'method of successive approximation' (Sweezy 1968; 11) or the 'method of isolation' (Maki 1992, but see Pratten 1999), and runs as follows. 'The initial stages of theorisation use known falsehoods. Explanatory power is added in stages as realistic assumptions are successively substituted for false ones'. There are two objections to this.

1. The method of successive approximation or isolation *might* be appropriate when the successive analytical steps merely involve the *mechanical* addition of factors that were previously assumed away. This mechanical addition is, however, not appropriate for systems where the elements possess *emergent properties*. When, for example new technology is introduced to a workplace or a new management regime is installed, its behaviour often evolves, giving rise to properties that were not present before. Many theoretical propositions derived on the basis of pre-emergent properties provide no grounds for the analysis of post-emergent forms of behaviour.

2. Theory is still reliant on closed systems. All that has happened is that one closed system has been added to another (slightly larger) closed system. A succession of closed systems does not, however, add up to an open system. Consider the following example:

- Closed system₁ assumes demand for labour is determined *solely* by wages.
- Closed system₁ generates the deduction/prediction₁ that the introduction of a minimum wage will cause a fall in labour demand.
- Closed system₂ now allows labour demand to be determined by wages *and* (say) aggregate demand.
- System₂ is, however, still a closed system: it just contains more variables. Many of the previous (false) assumptions remain in place and, new (false) ones are added to ensure closure in this more complex system. Falsehood is then piled upon falsehood – and the dream of one day removing all false assumptions evaporates.

The method of successive approximations, or successive closures might, therefore, be more accurately termed the 'method of successive falsehoods' or the 'method of successive closed systems'. In short, the counter-arguments do not evade the critical realist critique.

Conclusion

To the extent that neoclassical economic theory is rooted in the deductivist method, constant conjunctions of events, artificially closed systems and *known* falsehoods, it explains nothing at all.

Notes

1. I wish to thank Paul Lewis for his careful comments.
2. Deductivism is also found in some heterodox (Austrian, Institutionalist, Marxist and Post-Keynesian) economics whereupon these perspectives also become vulnerable to the following critique.
3. Neoclassical theorists do, of course, recognise that substitution between labour and capital is not ubiquitous and attempt to deal with it via non-convex isoquants. 'L' shaped isoquants imply only one production technique based upon one capital-labour combination and allow no substitution. Isoquants with n 'flat' sections imply n-1 production techniques and allow limited substitution. But, where tangency between the isocost curve and the isoquants is at a corner, factor prices could change without 'causing' substitution. Where tangency occurs along the face of one of the 'flat' sections of the isoquant, then the choice of technique becomes indeterminate.

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Defining "Economics" Inclusively

Tony Aspromourgos (University of Sydney, Australia)

In following the contributions and debates in this *Review* it has struck me that there is a need for a *definition* of economics which is wider and more inclusive than the old Wicksteed-inspired formula articulated by Lionel Robbins, in terms of the allocation of scarce means to inexhaustible purposes. *By the very terms of its constitution*, this definition tends towards defining the discipline as marginalist. That is to say, it tends naturally to the normative inference that orthodox economics *is* (the only legitimate) economics. Any definition of more generality should be concise, constitutive and programmatic. I offer the following for consideration: In the most general terms, ***economics is the study of how societies organize the production and distribution of the means of human sustenance and larger consumption.*** This is constitutive and programmatic in the sense that it defines a proper domain for the discipline – and points to a desirable set of research programmes which *ought* to be undertaken.

Its inclusiveness – or, if one prefers, its expansiveness – should be evident. The classical-Marxian orientation towards material reproduction and distribution, with the price system as a conduit for the associated allocation of commodities, fits naturally into this definition. Keynesianism (and hence also the effective demand dimension of Kalecki) can be articulated within its domain – as a theory of how, in certain kinds of political societies, there may be no spontaneous mechanism to ensure an optimal level of resource utilization, and hence sub-optimal activity levels and consumption outcomes occur. The evolution of social organization and institutions also finds a natural place, as the study of how the politico-social mechanisms for effecting consumption have varied. Hence those associated with the Historical School, evolutionary economics and Institutionalism find a legitimate place.

My definition also avoids the asocial orientation of marginalism *by taking its point of departure from social organization*, rather than from independent individuals. Furthermore, it embraces the study of current, past, or even future (including 'ideal'), societies. Hence it does not discriminate against economic history – economic history is included in a natural way in the definition, as well as economic anthropology and economic sociology. (The discipline definitely would be better off if some of the resources currently devoted to theorizing were redeployed to historical studies. There is too much of the former and far too little of the latter.) The study of 'ideal' societies points to normative analysis – in orthodox language, welfare economics – though of course, non-orthodox welfare economics need not only proceed on such a grand scale.

If it is felt that nature and the natural environment should be more explicit in the definition, then 'extraction,' could be inserted before 'production' in the definition. In fact, the focus on 'sustenance' is suggestive of sustainability – and indeed, the classical focus on reproduction of social economies is, at core, a notion of sustainability (including scarcity of an objective kind, rather than the subjective marginalist form).

The definition offered here might also have the effect of orienting the various, current subdisciplines of economics more towards the final, human, material purpose of economic systems. This need not in all respects involve radical departure from conventional thinking. Hence, for example, the theory of finance has as its purpose the study of instruments and systems for enabling the intertemporal shifting of consumption. This is not a heterodox proposition, even if participants in financial analysis often lose sight of this ultimate (legitimate) social purpose of financial systems.

Consumption is not *the* ultimate human purpose of course; but beyond survival, the purposes consumption serves seem not to be something economics can say anything very significant about. Certainly orthodox economics has offered little beyond empirically empty nonsense-tautologies like 'utility' and 'preference'. (Why not just say 'people are what they are' and be done with it!) The referring of consumption demand back to deeper underlying characteristics of commodities *may* be a fruitful way towards saying more.

From the standpoint of my suggested definition of economics, the marginalist approach then appears as the study of the distribution or allocation of a given set of resources to the achievement of (some of) a given set of possible (ranked) uses. Hence my definition does not, and does not seek to, exclude orthodox economics – it rather locates it as one approach, to one particular kind of question, in a larger and more general context.

Or perhaps it does exclude one dimension of the marginalist intellectual project in its widest form: the idea that its method (constrained individual optimization, with or without strategic interaction) can be a general theory of human psychology and choice *as such* – hence the idea that it can explain getting married, having children, going to war, committing suicide, and so on. By orienting the subject matter towards 'economics' *in the common sense of that term*, my definition marginalizes (pun intended) these pretensions. Get back to the study of 'guns and butter' (or, in a classical vein, machine tools and corn) boys and girls! 'It's the economy, stupid' – indeed.

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Beautiful Mind, Ugly Deception: The Bank of Sweden Prize in Economics Science¹

Yves Gingras (Université du Québec à Montréal)

Much has been said about the Oscar-winning movie *A Beautiful Mind* and its hero, the mathematician John Nash. Just as spring is the time for Oscars, a new crop of Nobel prizes has accompanied the fall of autumn leaves every October since 1901. As Daniel Kahneman and Vernon L. Smith share an award this year, it's a good time to pose a question raised by a neglected aspect of the movie: what prize exactly did Nash really win?

The answer is not as obvious as it seems. When *A Beautiful Mind* hit our screens, one correspondent to an entertainment weekly pointed out that the 'Nobel Prize in Mathematics' he had read about did not actually exist. Many will recall the brief scene in the movie when the young Nash – suffering from lack of recognition of his true genius – remarks to his MIT colleagues that he has been robbed of the 'Fields Medal'. What is that? Ask any

mathematician, and he will tell you: 'this is the equivalent of the Nobel prize for mathematicians'. Established in 1936, it is given once every four years to no more than four exceptional mathematicians under 40 years of age.

The incident confirms that John Nash, in coveting this most prestigious prize in the mathematics community, was at that point still rooted in reality. In contrast, though the story of a man from Stockholm waiting for Nash after his class to share the good news that he had won a prize is confirmed, it is doubtful that the prize itself was real. Or so I will claim.

The currency is prestige

Which 'Nobel prize' was the man from Stockholm talking about? Most journalists (and every economist) will of course answer, the 'Nobel Prize in Economics' – even though it is never specified in the movie. Against this taken-for-granted 'fact', I am arguing here that this prize does not exist: and moreover, that this so-called 'Nobel prize' is an extraordinary case study in the successful transformation of economic capital into symbolic capital, a transformation which greatly inflates the symbolic power of the discipline of Economics in the public mind.

The confusion can be traced back to 1968 when the governor of the Central Bank of Sweden decided to mark the tercentenary of that institution by creating a new award. It could have been named after a well-known ancestral economist, such as Adam Smith, or more simply, though unimaginatively, 'The Bank of Sweden Prize in Economics'. After all, every discipline has its own 'prestigious' prize. Their number grows every year. However, the problem is that all these prizes, though well known within the microcosms of their discipline, have little public appeal. Only the Nobel prizes have a real public impact. But they are limited to five fields: physics, chemistry, physiology and medicine, literature and, finally, peace.

Moreover, the enormous symbolic capital of the very name 'Nobel prize' has been accumulated over the years by a careful selection of prizewinners. Like every new prize, by definition unknown, the Nobel faced the problem of what we can call (invoking Pierre Bourdieu's apt concept) the 'primitive accumulation of symbolic capital'. This obstacle was overcome by giving the prize early on to already renowned scientists who would bring the prize real credibility. The idea was that, over the years, this symbolic capital would surely accrue to such an extent that it could in turn bring recognition to the chosen winners.

The organisers, conscious of this conundrum and wishing to endow the discipline of economics with as much public credibility as possible, decided to call the prize: 'The Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel'. Curiously then, it was the memory of Nobel, not that of an economist, that was being recalled. This mystery can be explained if we unpack the process crystallised in that bizarre and awkward name.

First, despite the scepticism of some scientists towards the 'scientificity' of economics, the Bank managed to convince the Royal Swedish Academy of Sciences and the Nobel Foundation to administer their prize. Secondly, identical procedures for the selection and nomination of the prize were chosen to those of the real Nobel prizes. Of course, the prize money would come from the Bank of Sweden, not the Nobel Foundation, but all the rest would be done exactly as if it was in fact a Nobel prize, up to and including the ceremony of 10 December.

Thus, the inclusion of the term 'in Honor of Alfred Nobel' in the title created the necessary bridge to the Nobel prize, and by exactly mimicking the process, the Bank created all the

conditions enabling the association and even the identification of its prize with those established by Alfred Nobel at the turn of the century. Note that, for obvious reasons, it is much simpler to say 'Nobel Prize in Economics' than 'Bank of Sweden Prize in Economic Sciences in Honor of Alfred Nobel'! No surprise that, since 1969, all journalists and economists have commonly referred to the Bank of Sweden Prize as 'The Nobel Prize in Economics'. The strategy was a complete success.

The social alchemy of belief

Now that we understand why a bizarre name was chosen, transforming a peculiar social alchemy into a 'Nobel prize', let us look at the 'flow of capital' the whole process involved. The Bank started with economic capital and 'invested' it in the Nobel Foundation to transform it into symbolic capital as fast as possible. Even a very large amount of cash is not sufficient in itself to assure the prestige of a prize. The key point was to effect a complete transfer of the already accumulated symbolic capital of the Nobel prizes to the new Economic Prize instituted by the Bank. Any other strategy would have been more risky given the difficulty, uncertainty and time lag attending any primitive accumulation of symbolic capital. In other words, this history makes visible the well-managed transformation of economic into symbolic capital, thus confirming Bourdieu's theory of the convertibility of the basic kinds of capital (economic, social, cultural and symbolic).

Of course, many will say: 'We all know it is the Bank of Sweden Prize, but it is much simpler to say "Nobel Prize".' In point of fact, the Nobel website is careful to make the distinction, thus habitually announcing the '2002 Nobel Prizes and the Prize in Economic Sciences in Memory of Alfred Nobel'. But this argument is either naive or disingenuous. For the success of the strategy of creating a 'Nobel by association' has obvious social consequences.

As anyone knows, the attribution of a Nobel prize gives instant world fame to the winners, who become oracles commenting on anything journalists can fathom: war, peace, philosophy, environment, irrespective of their particular fields of expertise. Interestingly, there is a strong correlation between the dates of attribution of a Nobel prize and the subsequent publication of memoirs or opinionated books by Nobel Laureates. This is a socio-logical consequence of the fact that the legitimacy bestowed by the Nobel prize is rapidly put to use in the public space to voice ideas that the winner would not have dared to submit were he or she a 'simple scientist'.

Whereas the 'spontaneous' philosophy or sociology of scientists can be considered relatively harmless, the situation is quite different in economics. By its annual offer of a public image of 'hard science' through its association with the Nobel prizes, the Bank of Sweden Prize in Economic Sciences gives the discipline and its laureates the 'scientific' aura it lacked to put forward authoritative but often simplistic theories about the economy (or, worse, the whole society) conceived as a big 'market' where everything can be submitted to the so-called 'law of demand' – be it a house, a wedding, or even an idea.

What is even more fascinating is that the social alchemy which transmuted the Bank of Sweden prize into a Nobel prize, affected not only the general public (via its media coverage of course) but the members of the discipline and even the winners themselves, who are convinced they have won a real 'Nobel Prize in Economics'. Thus, James Buchanan (1986 prize) offers the readers his "Notes on Nobility". Before him, Paul Samuelson (1970 winner) wrote about his 'Nobel coronation' – not his 'Bank of Sweden Coronation' – and filled his talk with references to Einstein (4 times) Bohr (2 times) and eight other winners of the (real) physics Nobel prize (not to mention, of course, Newton)

plus a few other names as if he were part of this family. Curiously there is not a single economist named in this talk. A simple counterfactual *gedanken experiment* (as physicists like to call these thought experiments) makes it easy to understand that such a talk would have been impossible had the prize been called “The Adam Smith Prize of Economics” and accompanied with a Million dollar check.

As for the discipline – in a move typical of the pushy newcomer – it markets with ostentation its (false) membership in the Nobel club by publishing books, such as *Lives of the Laureates: Seven Nobel Economists* (1986 and carefully updated to ‘Ten’ in 1990), which promote the discipline by associating it with the Nobel prize, a practice not observed in the scientific fields covered in the will of Alfred Nobel.

It would seem that engineers, frustrated not to have a Nobel of their own, have also approached the Nobel Foundation to create one, only to be told that, in order not to dilute the prestige of the Nobel prize, there should not be any more. Though the effect of scarcity applies to the value of economic as well as symbolic capital, the credibility of the Foundation may already be affected by association with the Bank of Sweden and the economists. Having played an important role in lobbying the Swedish Academy of Sciences to accept the Bank’s offer and after having himself received the prize, Swedish economist Gunnar Myrdal changed his mind and became a fierce advocate of the abolition of the prize. More recently, a few days before the Nobel ceremony of the 2001 prizes, descendants of Alfred Nobel criticized the use of the term “Nobel prize” applied to economics. Peter Nobel, a great-grandnephew of Nobel told journalists that his family is “asking for a clear distinction between the original Nobel prizes and this (prize)”. True to economic “laws” (or maybe ironic...) he noted that the actual use of the name “is like an intrusion in the trademark”! (See *Chronicle of Higher Education*, December 7 2001).

Though this suggestion may be considered extreme by many (not me), it is clear that there are now many people coming to the conclusion that the institutions involved made a mistake in associating themselves with this symbolic coup d’Etat in the ‘Republic of Science’ – a move aimed at enforcing the dominant status of economics as a ‘hard’ science not only among the disciplines of the social sciences, but first and foremost in the mind of the public and its elected representatives.

In his classic book *How to Do Things with Words*, philosopher John Austin, explained that words not only describe the world but create it through their performative aspect. Those of us who want to resist the symbolic violence inherent in the usurpation of the “Nobel Prizes” by economists and *do* something against this annual propaganda can begin by calling the prize by its real name: The Bank of Sweden Prize in Economic Science”. They can also correct systematically those who still persist in talking about the “Nobel prize in Economics”. Where the mere repetition of words has contributed to the “reality” of that prize in the public mind, it is not impossible that a systematic counter-attack could deconstruct this chimera propagated by media and idolized by economists.

Note: An earlier version of this article appeared on www.opendemocracy.net.

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In Defence of Amartya Sen

Ingrid Robeyns (University of Amsterdam, Netherlands)

In Issue 15 of the *Post-Autistic Economics Review*, Emmanuelle Benicourt (2002) argues that Amartya Sen's capability approach remains "undeniably neoclassical", and is "just a variation of standard microeconomics". She also categorizes Sen as a traditional mainstream economist. I wish to explain why I believe that these views are fundamentally mistaken.

The capability approach reconsidered

Sen's capability approach has its roots both in welfare economics (Sen 1985, 1987), where it was the logical extension of his earlier work on the informational poverty of utilitarian calculus (e.g. Sen 1979), as well as in the philosophical literature on inequality (1980), where it was proposed as an alternative to both the utilitarian and the resourcist paradigms. The capability approach advocates that in making evaluations of well-being or policies, we focus on what people can do and be, instead of exclusively on their mental states (utilitarianism) or on the goods that they have at their disposal (resourcism). Over time, Sen and others have extended the scope of the capability approach to study such diverse issues as development and development ethics (Gasper 1997, Sen 1999), the evaluation of small-scale NGO-projects (Alkire 2002), eating disorders and famines (Lavaque-Manty 2001), unemployment and inactivity (Burchardt 2002), gender inequality in western societies (Robeyns 2002), to mention just a few. At this moment PhD students are using the capability framework to study topics such as well-being of disabled people, environmental law and climate change, and the impact of a financial crisis on people's well-being. The *Human Development Report*, which is currently (one of) the strongest alternative frameworks to the neoliberalist "Washington consensus", is largely based on the normative foundations of Sen's capability approach. In other words, the capability approach has gradually developed into a *paradigm*, which moves between and beyond existing disciplines, and which is applied in many more domains than only welfare economics or liberal philosophy.

Is the capability approach just mainstream economics?

Does the capability approach make a difference with a standard mainstream economic analysis of these issues? I think that the existing work in the capability paradigm strongly suggest that it *does*. Some examples can illustrate this.

Sabina Alkire (2002) showed, based on fieldwork in Pakistan, that a cost-benefit evaluation that only focuses on material (financial) change, will not capture the changes in a number of important capabilities, such as self-respect. NGO projects that are not viable from a narrow economic point of view may lead to many non-material beneficial changes in poor people's lives.

Tania Burchardt (2002) developed a method to measure a person's *capability* for employment, instead of their *achieved functioning* (thus their real opportunity to hold a job, instead of the job-holding itself). By applying that method to British panel data, she can empirically distinguish between those who do not hold a job because they do not have a real opportunity to hold one, and those who do not hold a job although they could have one if they wished so. As Burchardt concludes, measuring employment capability would be more adequate than relying upon standard unemployment statistics.

In my own PhD-dissertation (Robeyns 2002), I first theoretically analysed (and empirically illustrated) why mainstream economics is fundamentally unsuited to study over-all gender inequality in well-being. A capability perspective, in contrast, allows us to see ambiguities and complexities that a pure utility- or income based analysis cannot reveal. For example, while women in western societies are worse off than men in many dimensions, there are also strong suggestions that men fare worse with respect to interpersonal relations and social support. 'Emancipation' then becomes much less an issue of getting women into jobs, but more radically about abolishing gender as we know it.

Reinventing the wheel?

Of course, it is often argued that ultimately the capability approach is doing the work that sociologists and other social scientists have been doing for ages. I agree that much of the work that is done in other social sciences is very similar to analyses that are done in the capability framework. However, a crucial distinction is that the capability approach gives a consistent normative framework to place these scattered studies, thus providing a sort of theoretical umbrella for existing empirical work. Moreover, the capability approach makes it theoretically very clear how different dimensions, such as commodities, observable outcomes and unobservable opportunities are related. Empirical and theoretical work, or micro and macro work, thus become much more connected. In addition, because of its inter- or post-disciplinary character, the capability approach offers a framework in which scholars and policy makers from different disciplines can easily meet.

This inter- or post-disciplinary character of the capability approach is one of its most interesting aspects. In my opinion, most fields in economics are more connected to related fields in other social sciences or the humanities, than to other fields in economics. The capability approach offers a paradigm for those utopian idealists who are dreaming of breaking down the walls between the disciplines and to do research and teaching based on topics and links between fields, instead of disciplinary assumptions and methodologies.

Of course, all this does not imply that the capability approach cannot substantially be improved or refined, or that it is completely ready to deliver; therefore much more work needs to be done – work that is currently undertaken by scholars across the disciplines, including many economists.

Sen's support for economists outside the neoclassical mainstream

Amartya Sen's work is extremely wide-ranging. Some of his work might be labelled mainstream-like because of its highly mathematical character. But few of these articles model behaviour; instead, most are about measurement or social choice. I doubt that this work should even be labelled neoclassical, because Sen has criticised many core neoclassical assumptions, like exclusively self-interested behaviour or the dogma of optimisation. In addition, Sen has written scores of articles that are definitely non-mainstream. And although he has spoken of himself as a "mainstream" economist, he has added that for him that mainstream is economics in the tradition of Joan Robinson, Marx, Kaldor and so forth. Thus, when Sen calls himself a mainstream economist, he is trying to rescue economics from the narrow-minded, imperialist discipline that it has become.

I think we must make a firm distinction between an economist who *is* a traditional mainstream economist, and those who, from time to time, use neoclassical mainstream *tools*. Moreover, we should not fear or condemn economists who use mainstream tools (1) if they have a positive encouraging attitude towards non-neoclassical economists, and (2) if they do not try to dominate them, for example, by only giving jobs to mainstream

economists or by refusing on methodological grounds to publish articles of other persuasions. Sen cannot be accused of any of this. Sen has done much to make economics more inclusive for economists with non-traditional views, and has given much personal support to such economists and their organisations (see also Fine 2001). He is, for example, a patron of the *Cambridge Journal of Economics*, and has given much support to the *International Association for Feminist Economics* and its journal. On a personal note I want to add that when I was his PhD-student he actively encouraged me to do what I believed in, without being straightjacketed by disciplinary or methodological requirements – a situation that many contemporary economics PhD students can only dream of.

Using Sen's work to develop an alternative economics

In recent months, several authors in the *Post-Autistic Economics Review* have argued that we need to focus our attention on trying to develop an alternative economics. I believe that much of the constructive work that has to be done can potentially benefit from Sen's work. Or, to use Ben Fine's (2001: 12) words:

"[Sen] has not been captured by economics imperialism and, unlike its practitioners, he opens and is open to debate across key issues. The contrast with mainstream economics is sharp, where the language let alone the ideas necessary for a genuine political economy of capitalism are precluded by its reductionism. Ultimately, the nature and extent of Sen's lasting contribution will depend upon taking his work forward critically rather than allowing it to be captured and transformed by the dismal science. Political economy may not always be able to stand on Sen's shoulders in the coming period, but he certainly provides many weapons in addressing the social, the macro, the material, and the cultural in the intellectual battles that lie ahead in defining the "economic" for social science."

Indeed, it would be a capital mistake not to regard Sen and his work as an ally in our struggle to open up economics, even if Sen himself prefers not to jump on the barricades, but to provide us with some fundamental concepts and tools that can be used to provide the hard-needed alternative.

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Economics Outside the (Edgeworth) Box

Robert Scott Gassler (Vrije Universiteit Brussel, Belgium)

Away from Autism

A couple of years ago I went to a European conference on heterodox economics, and I made three mistakes. First, I wore a blue suit. Second, I admitted to the session chairperson that I was from Texas. Waco, even. Third, within the first two minutes I accidentally said something about neoclassical economics that fell short of a complete condemnation. The audience fried me. Next year I plan to go again. I'll wear a tweed jacket with no tie, I'll tell everyone I was born in Ohio (which is true), and I'll start by trashing neoclassical theory big time.

To avoid that problem here, I shall state my position as clearly as I know how. Pluralism is good; too much mathematics is bad. Any decent doctoral program should include methodology, history of economic thought, economic history, and plenty of heterodox theory. I was one of the first three signers of the PAE petition in Belgium.

What should we keep from neoclassical economics?

After twelve years as an American teaching in Belgium, I have developed a great appreciation not only of Belgian culture in particular and European culture in general, but also of the things about America that I think are worth emulating in the rest of the world. Interestingly, they are not at all the things that others might think. By the same token, I believe that there are certain things worth keeping from neoclassical economics, but they are not at all what others have listed in this journal.

The most important thing about neoclassical economics is that it has developed a relatively complete set of categories which can be used to classify concepts and facilitate communication with other disciplines. The fact that these particular concepts are undervalued by neoclassical economists whose communication with other disciplines is notorious for its one-sidedness is no reason to throw them out.

From Walras and others at the turn of the last century, under the influence of the logical positivists, is the threefold categorization of positive economics, normative economics, and applied economics. It is important to distinguish the first two precisely because they are so

easily conflated and virtually impossible to disentangle. Positive economics is invariably subjective (which is not the same as normative: astronomers of any value orientation can see other parts of the universe only from our location and their time). It is also infused with values from the time a researcher chooses what variables to measure to the time he or she draws “policy implications.” In order to make progress in determining what the universe is like, however, we must do what we can to avoid seeing only what we want to see due to our values.

The distinction between theory and “empirics” should be maintained for similar reasons, though pedagogically it may be better to mix them as they do in the natural sciences. We should perhaps avoid telling our students that an earlier meaning of the term “empirical” was “fraudulent”. We should however note that the most zealous guardians of the inner core of neoclassical theory (located in Chicago, Illinois, not Cambridge, Massachusetts) refer to themselves as “empirical economists”.

The important thing for both theory and empirics is to be pluralistic. For theory the meaning has been discussed extensively in this journal. For empirics it means taking an interest in techniques not taught in econometrics courses. For example, my old micro professor dismissed survey research with an apocryphal anecdote: someone once asked CEOs whether they maximize profits, and they said no, we also look to help the community. Then they asked whether the CEOs could be doing more to make money, and they said no, they were doing all they could. End of story. He neglected to mention that virtually all the statistics used for number-crunching are based on data gathered by surveys.

From the field of industrial economics, we should adopt the concepts of structure, conduct (or behavior), performance, and practice. The latter is called “policy” in the literature, but “practice” includes firms and other nongovernmental actors. Never mind the esoteric debate over whether causality runs from left to right or whether we can skip any of the steps in between: the universe is complex and causality is mutual and simultaneous. Get used to it.

From public economics, we should keep the distinction among the economic activities of government developed by Musgrave: allocation of resources (public goods, externalities, imperfect competition, social rates of discount, excessive risk), distribution of income, and macroeconomic stabilization. Do not yield to the temptation to use the word “function” instead of “activity” for fear of inducing unnecessary debate by sociologists and even biologists. To complete the list, we should add the two used in the underrated theory of economic systems: the natural and societal environment (taste, technology, resources) and the economic system itself (ownership, economic information, transactions).

Together these constitute as close to a mutually exclusive and exhaustive set of categories as anyone would want in as complex a subject as this. One wants an open-ended science, like biological taxonomy or library classification. Therefore too much rigidity is bad, as is the construction of a closed system, even if a taxonomic one. But so is too much confusion.

A Suggested Framework

The whole scheme would fit together in a systematic way, while leaving room for miscellany and growth in unexpected directions. (I elaborated on this in Gassler, 1998) It helps us if we were to draw analogies with the pure and applied biological sciences; these are given in parentheses. It also helps us make sense of the suggested curriculum for economics proposed here earlier (“Two Curricula,” 2000). These are indicated in quotation marks.

Structure (positive, analogous to anatomy) “Descriptive economics: history of economic and social phenomena, actors and institutions”

1. The natural and social environment: tastes, technology, and resources.
2. The economic system itself: the rules of ownership, the pattern of economic information, costs of different transactions.

Conduct or Behavior (positive, analogous to physiology)

3. The allocation of resources, including: public goods and externalities, coordination (through market or nonmarket means under perfect or imperfect competition), choice over time, risk and uncertainty.
4. The distribution of income: in cash or in kind.
5. Macroeconomic stabilization: employment, prices, growth.

Performance (positive in using performance measures; normative in choosing which ones to use; analogous to pathology when used to mean the study of disease) “Applied economics and quantitative methods; economic and social policies”

Criteria that correspond roughly to the categories given above: efficiency, equity, stability for the last three, things like “propriety” or “information symmetry” for the others.

Practice (applied, analogous to clinical medicine) “Theories and issues”

Corporate strategy, nonprofit strategy, government policy, etc. This is the purview of the professional schools: business, international affairs, law, library and information science, nonprofit administration, public administration, social work.

Arrow-Debreu and the fundamental theorems of welfare economics are a rather simple special case of positive and normative economics, a fact not lost on Arrow, Debreu, and others. Assume maximization of exogenous tastes, exogenous technology, fixed resources, private ownership, full information, costless transactions, no public goods or externalities, parametric prices (which some say could mean competitive equilibrium), no social discount rate, and no excessive risk, and the allocation of resources is efficient for whatever equitable or lopsided distribution your economic model started with. The Edgeworth boxes “prove” it. Stabilization is automatic, since everything runs smoothly. Performance is perfect. Application is impossible. This is worth a week or two in class sometime in an economic student’s career in order to show how neoclassical theory fits together and to show how silly it is for economists to be flippant about things like free trade.

To be sure, the neoclassical model has been amended and extended to relax some of the assumptions in each category; whole fields of economics have been started that way. But the fun part comes in when we look at the heterodox approaches. Most of them attack on all fronts, but as a first approximation some of their attacks are stronger in certain categories. For example, the assumptions about tastes have been attacked by several schools: feminist (attacking the assumptions of exogeneity and selfishness), behavioral (attacking maximization), and humanistic and socio-economics (attacking the assumption that all things enter preferences the same way). The Marxist and Post-Keynesian schools attack the assumptions underlying neoclassical theories of stabilization (macroeconomics). Of course Marxist, evolutionary, and institutional economics go way beyond the list, but that fact should be stressed in order to keep us from thinking the list is a closed system, not in

order to discard the list.

Conclusion

To restate my position: neoclassical economics, especially mathematical neoclassical economics, is okay if you do not take it too seriously. It should be only a small part of the economics of the twenty-first century. If you think that makes me anything other than a post-autistic economist, then I'll swat you with my tweed jacket.

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Need Efficiency – and Much More!

A response to Richard Wolff

Grischa Perino (University College London, UK)

Richard Wolff¹ attacks the concept of efficiency and the related tool of cost-benefit analysis, first for being based on false assumptions about the nature of the world and second as an instrument which enforces hegemonies of one social group over another. He doesn't clearly reveal what he wants to change. (Or maybe I just haven't got the point.) It is nevertheless obvious that he dislikes the status quo. However he doesn't say whether he wants to get rid of all cost-benefit analysis because it fails to incorporate the infinite interdependency of the world or whether he just wants to emphasise the subjectivity of this kind of analysis. This makes all the difference.

Before I explain why I think the first option is dangerous but the second very important, let me summarise his reasoning.

Richard Wolff says that from an "overdeterminist" view of the world, "any one act, event, or institution has an infinity of effects now and into the future" and vice versa that "each of such effects actually had an infinity of causative influences". He concludes that it is impossible to undertake a complete analysis of all positive and negative effects of any policy. Cost-benefit analyses are therefore by their nature selective. Different social groups struggle against each other to gain the power to define the set of effects (and therefore interests) included in the process of evaluating different policies. The group who wins this fight sets its own definition of efficiency as an absolute measure and imposes a hegemony over the rest of society.

I share his view that our world is much more complex than any kind of cost-benefit analysis can ever cover. But is this a legitimate basis of critique? I think it is not. From my point of view there is no concept or tool which could deal with the full richness of our world. Each and every attempt to invent such a tool has to fail. That is because all our thinking about

the world is by its nature incomplete: and so are all the models we create to explain what is going on around us and what will happen if we do or don't do anything. (Just to avoid misunderstandings, the term model covers much more than the funny mathematical things used in economics textbooks. Each and every kind of thought about the world builds up some kind of model which relates causes and effects in a more or less incomplete way.) This is a mess, but this is the only way we can deal with our imperfect human state situated in a mind-bogglingly complex environment. So we have to be selective when we evaluate policies. This is nobody's fault. It is a simple result of living in a constrained world.

As it is impossible to develop a tool which predicts the 'true' effects of any policy on the agenda, it is pointless to claim that a particular tool is inappropriate because it fails to do so. There may be a lot of reasons to criticise the concepts of efficiency and cost-benefit analysis, but being limited in scope and therefore selective isn't one of them.

It is not only pointless but dangerous to use this kind of criticism. As there is no instrument of policy evaluation which satisfies this condition, the call for tools which aren't selective is equivalent to saying that there should be no policy evaluation at all: but in my opinion there is nothing more dangerous than arbitrariness.

It is nevertheless most important to keep in mind that all evaluations of policies are limited and selective, because it follows that no single tool could claim to tell the truth. Each and every analysis ignores some causes and effects and therefore interests. Richard Wolff is right in concluding that the implementation of one single instrument leads to a systematic bias towards particular interests and the exclusion of others. But how can we avoid building a hegemony without falling back to arbitrariness? The solution relies on two features. We need rich and diverse branches of social sciences (among them economics) which offer many different instruments and apply them to evaluate policies. After an open discussion which should aim to reveal the different values behind the analyses, the decision on which policy is chosen should be up to a democratic process. In my opinion there is no better way to take into account both the limits of our ability and the necessity to evaluate policies.

The aim of the post-autistic movement, to demand diversity in economics teaching and research, is therefore the best thing I can think of.

Note

1. Richard Wolff, Efficiency: Whose Efficiency, *Post-Autistic Economics Review*, issue no. 16, September 16, 2002, article 3. Available at: http://www.btinternet.com/~pae_news/review/issue16.htm

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