



## Methods in Economic History

# The domestication of the economic mind: A response to the critics

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## ABSTRACT

The paper takes issue with the views expressed by Luis Bértola, Christopher Lloyd and Deirdre McCloskey in their commentaries. Many of the points raised are relevant and worth consideration. However, McCloskey's ill-informed critique is rejected along with her binary opposition between economy and society. There are no laws in economics. Faith in the inborn nature of the market economy is misplaced and due to cognitive distortions.

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## La domesticación del pensamiento económico: respuesta a las críticas

## RESUMEN

En este trabajo se discrepa de las opiniones expresadas por Luis Bértola, Christopher Lloyd y Deirdre McCloskey en sus comentarios. Muchas de las cuestiones planteadas son importantes y merecen atención. No obstante, se rechaza la crítica mal fundamentada de McCloskey, así como su contraposición binaria entre economía y sociedad. En economía no hay leyes. La fe en la naturaleza innata de la economía de mercado no tiene justificación y es fruto de distorsiones cognitivas.

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*The Poverty of Clio* is clearly not a politically correct book. But it could have been worse. One should always look at the positive side of things. For example, I could have said that cliometrics is *Geschichtenscheissenschlopf*, as E.P. Thompson once observed, because it is part of my NATs (Negative Automatic Thoughts). But I did not. My penchant for understatement prevents me from being so direct.

What we have here is neither a treatise nor a comprehensive guide to recent economic historiography. I doubt I would have the patience to write such a book and, to be honest, I could think of better topics. It should be taken for what it is: a reaction to a state of things. A polemic has accomplished its mission when it makes people think. Therefore I welcome any comments, including those from

the cliometricians' side. No one should take offence if their work has not been covered. Europe is what I know best and the preindustrial period is the context in which the application of modern economic tools has been more harmful. I am currently involved in a global history project aimed at increasing the international impact of non-Western scholarship, but I still subscribe to the statement that 'the global and comparative perspective is... expressed not so much in the subject of study, which does not need to be exotic, as in the approach' (p. 155).

When I started writing the book I had three main goals: the first was to warn my European colleagues against the bad research habits that were spreading from across the Atlantic. Emphasising the Euro-American (or Asian-American) divide in this respect may be inelegant but it is a matter of fact that humanistic and social-scientific approaches to economic history have not yet been marginalised in the Old World and in many countries they still

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represent the orthodoxy. The second goal was to remind historians in the US about the importance of dealing with material life alongside other aspects of the past. ‘Don’t leave it to the neoclassical gang’ – that was my plea. Finally, I was deeply bothered by the frequent misuse of cultural and institutional explanations in this domain, often as a mimetic strategy aimed at incorporating the phenotype of colonised subject matters into the body of ahistorical reductionism.

I quite agree with Luis Bértola’s view that economic history (and history in general) should be relevant to the present. Let me say that history is *always* relevant to the present as long as it asks good questions. It is often the case that the present dictates the questions; it is left to the historian’s integrity not to let them condition our search for the answers. These belong to the past. However, a distinction has to be made between economic history, historical economics and historically informed policies. Economic history is the study of continuity and change in material life across time and it forms the subject of the book. By historical economics, a term much abused these days, I mean a form of inductive economics that takes advantage of the findings of historical research to make cautious generalisations. Both can also offer policy makers operational tools and, hopefully, food for thought. But these applications, although fruitful, do not fall within the domain of economic history as such.

Chris Lloyd’s discussion of ‘synthetic-structural history’ from the perspective of heterodox economics and broader intellectual approaches (such as critical realism) makes an important contribution. On the other hand, I do not believe that replacing the rational choice or new institutional framework with a different theory of economic history would change things for the better. I am somewhat sceptical about comprehensive theories of economic history. Marxism is one of them. It certainly offers many valuable insights into capitalist economies and societies but it does not explain everything. Class and class struggle are modern concepts as much as utility and growth. Moreover, I am not sure as to what extent it can be helpful to treat such driving forces of human behaviour as religion and mentalities as mere superstructures. These considerations also apply to world-systems theory, despite its merits in accounting for the role of power and power asymmetries in economic life. If neither specific theories nor comprehensive theories can provide salvation, we are left with metatheories.

Lloyd writes that ‘one person’s ideology is usually another person’s common sense’, hinting at the fact that metatheories are, after all, ideologies. Bértola makes a similar point in his paper. I regret that, in referring to metatheories as *Weltanschauungen*, I might have caused this misunderstanding. Let me elaborate a bit more on this concept. I do not wish to be shackled by words and I am sure there are other and perhaps better ways to express an idea that seems quite intuitive to me. In fact, what I called ‘metatheory’ is neither ideology nor common sense. It is the product of a dialectical process involving both the historian’s critical reception of the available body of theories and his own past findings. And all this only serves as a first-reference guide when approaching a question, as it needs to be continually tested against the evidence. The sources play an active role in this process, not a passive one. As Jürgen Kocka (and his Bielefeld colleague Hans-Ulrich Wehler, 1980) put it almost thirty years ago:

How do theories originate? Where does the historian get his or her theories from? Usually the neighboring social sciences are helpful in this respect, but I think that the historian has to modify them and to combine them with other theories and his or her own source-informed notions. *The historian has to reconstruct them for the purpose of using them for his or her specific tasks* (Kocka, 1984, p. 173, emphasis added).

Another point I want to make clear is that cliometrics should not be confused with quantitative history. I have much respect

for quantitative historians, especially when they work with real figures or reliable estimates, which is what for instance Paul Bairoch and Charles Feinstein did very well with national accounts, population trends and international trade. But most of what is currently produced under the label of the ‘new economic history’ has little to do with this and I would not call it quantitative, unless one regards as statisticians those medieval monks who sought to measure the distance from the earth to Purgatory. Reliance on bad economic theory, its methods and assumptions on human behaviour, makes the difference. The literary or numerical qualities of the prose are a non-issue.

Last but not least, I have never suggested that each place should be studied ‘as an island of data’. This is Deirdre McCloskey’s rather stereotypical view of historicism, a view that is interestingly shared by those writing from a neo-Marxist perspective (Sewell, 2012). When I say that ‘each particular economic system needs to be understood on its own’ (p. 87). I do not mean that mutual interactions, external influences and global forces should not be taken into account, nor I am suggesting that the nation state, or a region, should be assumed as the only legitimate unit of analysis. The message I intend to convey is that economies past and present reflect cultural specificities that must be fully grasped before their possible connections can be appreciated. Connections (if there are any) are to be found *ex post* rather than just taken for granted.

McCloskey should not dismiss the achievements of the German Historical School, and for two good reasons. One is that this tradition, along with Durkheim’s sociology, forms the backbone of Western social science. Another reason is that her latest book *Bourgeois Dignity* (McCloskey, 2010) is basically a restatement of Sombart’s thesis of the rise of ‘modern economic spirit’, as put forward in *Der Bourgeois* (Sombart, 1913) – a work which is virtually unknown in the United States. What is new is the author’s insistence that bourgeois values can be found everywhere across cultures and societies (maybe, if you use such term in a very loose sense, as Jack Goody does) and their rise goes hand in hand with the universal development of capitalism (which is wrong, as China will tell us in 20 years). After all, Sombart disliked the *Geist* of the *moderne Wirtschaftsmensch*; McCloskey is fond of it.

What divides McCloskey not only from the German masters, from Polanyi, Sahlins (and myself for that matter), but also from Keynes, Schumpeter, Myrdal, Perroux, Galbraith the Elder, Hirschman, Sen and many others, is her belief that natural laws do exist in economics. In this respect, she reminds me of Pareto (1906) who proposed an ingenious classification of human actions into logical and non-logical, the former being the subject of neoclassical economics, the latter of sociology. Logical actions are ‘prudent’ actions leading to wealth maximisation, non-logical actions lead to anything else. Pareto’s construction falls apart as soon as one realises that value judgements are implicit in the definition of what is economic, or logical, or rational, and what is not. In the age of Positivism this way of reasoning was all too common, and such a ‘bipolar’ model of human behaviour was an obvious counterpart to the phrenological understanding of the brain. Today it is no longer acceptable. Therefore, it is entirely misleading to represent neoclassical economics as if it was just about ‘counting’. The logic of utilitarian rational choice embodies a belief system. There is nothing objective about it. It is also unacceptable to label as ‘anti-market’ any analysis that contradicts neoclassical principles and their proponents as ‘anti-economists’. Was Keynes an anti-economist?

The best way of challenging disfunctional beliefs, according to most clinical psychologists, is to evaluate them against the available evidence. For example, McCloskey thinks that markets arose ‘in the 700th century BCE outside the caves of people in southeastern

Africa speaking full language.' Is this belief based on her own archaeological expertise or perhaps that of sociologist Jean Baechler? To my knowledge, there is no evidence supporting this claim. More importantly, the existence of markets (almost always in combination with other 'forms of integration', as Polanyi put it) tells us little about the nature of an economic system. It is always a matter of proportions. As I made clear, paraphrasing Finley: 'Markets did... exist [in classical Antiquity], just as they did in early modern Europe. But there was no *market economy*, nor could one have developed.... [T]he market economy is the typical allocation system of capitalism, and the ancient economy was not a capitalist system' (p. 32).

In fact, there is more heat than light in McCloskey's counter-critique. She says that I am as good at reading as Joel Mokyr's graduate students. Good for Mokyr, and for his students. She blames my sneering habits. Mixed in are three or four commercials for her books (I highly recommend them – they are a great value for the price) and a funny remake of Ricardo's theory of comparative advantage according to which Italians should specialise in Italian history, literature and poetry leaving economics and the hard sciences 'to the Yanks, or Yankee-oriented Italians [so] that the intellectual or practical benefits from such sciences spill over into other countries.' Come on, Deirdre, don't try and divert the attention away from my arguments. And please get rid of all the batteries and plastic items in your office, home or car. Neither Alessandro Volta nor Giulio Natta were particularly Yankee-oriented.

This said, I have a high opinion of McCloskey the scholar. She is unusually clever and well-read. The reason why I made an exception for her when it came to organise the big book-burn is that she seems to have undertaken an intellectual conversion over the years. She has freed herself from a materialist standpoint and now believes in the power of ideas. She has become an advocate of humanistic education. That is a great leap forward. Whether this is enough to make somebody a historian, however, is doubtful. I rather tend to see her as a literate economist who writes about the past in a teleological way. Her world is still one of individual achievements under the divine force of markets. It comes with no surprise that she has a hard time understanding how the environment, even in a physical sense, can condition and shape individual behaviour. This is precisely the message of Braudel's *Mediterranean*, which she labels as an example of 'scissors-and-paste history' – something that would sound ridiculous even to Braudel's historical opponents (see Hexter, 1972).

While I have a high opinion of McCloskey the scholar, McCloskey the performer has the bad habit of misrepresenting what she dislikes. In so doing she sometimes makes gross mistakes. I will give just two examples from her review.

McCloskey claims that I do not understand modern economics and calls my 'attempt at supply-and-demand analysis on p. 94 ... embarrassingly incompetent, confusing backward-bending supply curves with shifting ones.' No, Deirdre, I won't let you play Douglass North's trick on me. I will be charitable and assume that you were just having a bad day when you read the section on Poland. There is no need of your *Applied Theory of Price* to show that I am right and you would have done a better job at being more careful. Please go and take another look at the Samuelson and Nordhaus (2005, p. 53): 'When changes in factors other than a good's own price affect the quantity supplied, we call these changes shifts in supply.' As I wrote, 'the volume of agricultural products exchanged via the market was exceedingly unstable and depended exclusively on ... fluctuations in the harvest ... over which there was little control' (*Poverty of Clio*, p. 92), and again: 'a rise in prices [was] typically due to a poor harvest' (p. 95). The same could be said of an increase in demand (cf. p. 100). Poor harvests make supply curves *shift* backwards (p. 94, Fig. 4.1a) as the available quantities are independent of prices. On the contrary, a backward-bending curve would indicate a causal

relation between quantities and prices. It may be hard to believe for a former 'Chicago boy' but frosts (and exogenous shocks in general) do not happen in response to price signals.

Yet another example of McCloskey's peculiar reading habits: 'Boldizzoni is saying that output per man "makes no sense" if the man is a slave instead of a wage worker.' Very theatrical – but not what I said. I rather argued that Total Factor Productivity makes no sense – at least, if you intend to measure economic performance in preindustrial times (p. 84). TFP is not 'output per man' but only a way of calculating it. McCloskey should know the difference between the measure and the measured.

In Philip Hoffman's (1996) work on early modern France, TFP growth rate ( $\hat{g}$ ) is obtained by using the so-called 'dual method', as share-weighted growth in factor prices:

$$\hat{g} = s_K \cdot \dot{r}/r + s_L \cdot \dot{w}/w + s_T \cdot \dot{l}/l$$

where  $K, L, T$  are units of capital, labour and land;  $r, w, l$  are the rates of profit, wage and land rent, and  $s_K, s_L$  and  $s_T$  are the factor income shares (e.g.  $s_K = rK/Y$ ).

This all comes from a very simple statement of the equality between output ( $Y$ ) and factor incomes:

$$Y = rK + wL + lT.$$

Differentiation of both sides of the equation with respect to time leads straightforwardly to the formula for  $\hat{g}$ . What is wrong with it? Aside from the kind of objections that excited the Cambridge Keynesians in the 1960s (how do we measure capital? Can it be spread over time like butter or jelly? Is this way of reasoning scientific?), let us see under which conditions the result can be meaningful – not accurate, just meaningful. The basic assumption behind this equation is that we are dealing with a market economy, one where the inputs are systematically allocated via the market in a competitive way and the output commercialised. This is something that Hoffman (and McCloskey) should *prove*, not simply assume.

The evidence for early modern France, from Goubert (1960) to Grenier (1996), points to the contrary. Only a fraction of the agricultural output was commercialised. There is no trace of 'impersonal' labour markets and it was not uncommon for 'wages' to be paid in kind. Capital – even if we mean the modern concept rather than Quesnay's *avances* – was mostly self-produced. Land did certainly yield rents to the aristocracy but the point is how these rents were fixed: was scarcity the main determinant or should we look at other variables such as status and power? Moreover landowners, be they wealthy northerners or *vignerons du Midi*, would not sell their land unless overburdened by debt.

Mainstream economists and their historical followers share the not-so-conscious belief that the past can be treated as a giant Wal-Mart. This belief is unfounded, and not because human life involves more than material needs and wants. What they fail to acknowledge is that the Wal-Mart Economy is not the only form of *economic* life found in history, nor does it represent the final and supreme stage of an evolutionary process. Deduction from a set of axiomatic principles only reinforces the error. Why so many people who are certainly not 'dopes' (*sic!*) are trapped into this vicious cycle of seeing in the past what they want to see? Are they blinded by ideology? Do they lack the historical sensibility and training? Is it 'the neighbours'? Is it the *Zeitgeist*? There is no single answer. One thing is certain: either they break the vicious cycle so that their mind is liberated or any discussion of method will necessarily result in a dialogue of the deaf.

GALILEO (at the telescope): As your Highness doubtless knows, for some time past we astronomers have been in great difficulties with our calculations. For these we use a very old system which appears to coincide with philosophy, but not, alas, with facts.... Would the

gentlemen care to begin with an observation of the satellites of Jupiter, the Medicean stars?

ANDREA (pointing to the stool in front of the telescope): Please sit here.

THE PHILOSOPHER: Thank you, my child. I fear that things are not quite as simple as all that. Signor Galilei, before we apply ourselves to your famous instrument we would like to have the pleasure of a disputation. The theme: Can such planets exist?

THE MATHEMATICIAN: A formal disputation.

GALILEO: I thought you could simply look through the telescope and convince yourselves.

....

THE PHILOSOPHER: Your Highness, ladies and gentlemen, I am just asking myself where all this may lead.

GALILEO: I would suggest that as scientists it is not for us to ask where the truth may lead us.

THE PHILOSOPHER (furiously): Signor Galilei, the truth may lead us to absolutely anything. Bertolt Brecht, *The Life of Galileo*, IV (trans. Desmond Vesey).

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