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# Inequality, the financial crisis and stagnation: competing stories and why they matter<sup>1</sup>

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#### Abstract

This paper examines several mainstream explanations of the financial crisis and stagnation and the role they attribute to income inequality. Those explanations are contrasted with a structural Keynesian explanation. The role of income inequality differs substantially, giving rise to different policy recommendations. That highlights the critical importance of economic theory. Theory shapes the way we understand the world, thereby shaping how we respond to it. The theoretical narrative we adopt therefore implicitly shapes policy. That observation applies forcefully to the issue of income inequality, the financial crisis and stagnation, making it critical we get the story right.

**JEL codes** E00, E02, E10, E20, E24

Keywords Income inequality, financial crisis, stagnation, economic theory

#### 1. Introduction: competing stories about the role of inequality and why they matter

This paper explores competing stories about the role of income inequality in the financial crisis of 2008 and the ensuing stagnation. At one level, the paper is a purely analytical exercise. At another level, there is a deeper purpose regarding exposing the neoclassical monopoly in economics that has destroyed pluralism and distorted economic debate and policy making.

An open-minded pluralistic economics demands representation of all economic theories that provide a logically coherent explanation of the economy consistent with the facts as we know them. But that is not how economics is practiced owing to the neoclassical monopoly.

Pluralism is not just important as an intellectual aspiration. It is also important in practical terms for delivering sound economic policy. Theory shapes how we understand the world, which in turn influences how we respond to events. Theory is a form of story-telling, and the stories we tell shape our understanding of the economy and economic policy. That means the stories we tell are critical.

The paper examines several mainstream explanations of the financial crisis and stagnation and the role attributed to income inequality. Those explanations are then contrasted with a structural Keynesian explanation.<sup>2</sup> The role played by income inequality is substantially

<sup>&</sup>lt;sup>1</sup> This paper was originally presented at the *European Dialogue 2015* forum organized by the Hans Bőckler Foundation and held in Brussels, Belgium on April 16-17, 2015.

<sup>&</sup>lt;sup>2</sup> The financial crisis of 2008 and subsequent Great recession have triggered new interest in the effects of income inequality among mainstream economists. Non-mainstream economists focused on inequality for over three decades prior to the crisis. At the theoretical level, the Keynesian approach is based on the neo-Kaleckian growth model pioneered by Rowthorn (1982) and refined by Bhaduri and Marglin

different in each explanation, giving rise to different policy recommendations. That illustrates the importance of the theoretical stories we tell about the economy, making it critical we get the story right.

# 2. Fault Lines: Rajan (2010)

Rajan (2010) was an early contributor to the new wave of thinking attributing a role for inequality in the financial crisis. According to him, increased income inequality in the US prompted a populist political response focused on making homeownership more affordable. This involved government interventions in the housing finance market which encouraged homeownership beyond people's means and spurred a credit-driven house price bubble. When the bubble eventually burst in 2006, the supporting financial structure came crashing down.

There are three features to note about this story. First, Rajan's claim that the financial crisis of 2008 was caused by government intervention in the housing market is empirically implausible (Palley, 2012, chapter 6). These interventions had been in place for decades. The Community Reinvestment Act was passed in 1977, and the Federal National Mortgage Association (FNMA or Fannie Mae) was founded in 1938 as part of the New Deal. Sub-prime loans, which triggered the crisis, were originated by private lenders and Fannie Mae only started buying them and facilitating their issuance towards the very end of the bubble. Lastly, the price bubble impacted commercial real estate equally strongly but commercial real estate was not subject to any of these government interventions.

Second, according to Rajan the labor market was working efficiently and income distribution was neither a micro nor a macroeconomic problem. Instead, income inequality was economically justified by technological developments that had increased returns to skilled labor and lowered returns to unskilled labor, and it was only a problem because it spurred politically motivated flawed policy. Thus, though raising the issue of income inequality, Rajan departs fundamentally from reasoning that holds income inequality generates aggregate demand problems and is the result of unequal bargaining power in labor markets. Absent careful attention, it is very easy to misattribute this argument to Rajan, when it is in fact completely absent in his book.

Third, Rajan's book lacks any implications about stagnation. Recently, to explain stagnation, he has argued (Rajan and Ramcharan, 2015) that the after-effects of economic crises associated with high leverage are especially long. That puts him in the company of Reinhart

<sup>(1990)</sup> to include a distinction between wage-led and profit-led growth. Stockhammer (2011) provides a survey of this literature. Palley (2011) provides a policy framework for implementing wage-led growth. Non-mainstream empirical work documenting the rise of income inequality includes Galbraith (1998) and the biennial *The State of Working America* produced by Larry Mishel and his co-authors at the Economic Policy Institute in Washington DC since 1986. Non-mainstream analytical work regarding the economic impact of inequality and its tendency to create stagnation includes Peterson (1994), Palley (1998), Stanford (1999), Pollin (2003) and Glyn (2006). Mainstream academic interest was initially triggered by the empirical research of Piketty and Saez (2003) and Gordon and Dew-Becker (2008). That interest has gone viral following the publication of Piketty's (2014) book, *Capital in the Twenty-First century*. The new mainstream policy interest is evident in Federal Reserve Chairman Yellen's recent speech on income inequality (October 2014) and her call for more research into the effects of inequality (April 2015). It is also evident in recent highly profiled IMF research papers on growth, redistribution and inequality (Berg and Ostry, 2011; Ostry et al, 2014) and on unions and inequality (Jaumotte and Buitron, 2015).

and Rogoff (2009), but their empirical claim of lengthy recessions after financial crises has been challenged by Christina and David Romer (2015). The latter find that when financial distress is categorized on a relatively fine scale rather than being treated as a 0-1 variable, "output declines following financial crises in modern advanced countries are highly variable, on average only moderate, and often temporary."

# 3. Inequality, leverage and crises: Kumhof and Rancière (2010)

A second contribution to the debate about the role of income inequality in the crisis comes from Kumhof and Rancière (2010). Their explanation is a mix of Keynesian demand side theory and classical supply-side theory. The argument is worsening income distribution, caused by declining union bargaining power, led to a persistent surge in borrowing as workers tried to maintain their living standards. That rendered the economy financially fragile and vulnerable to another shock to worker bargaining power that further lowered worker income so that they could not pay back their loans.

However, closer inspection shows the story is much less Keynesian than it appears. First, the economy is a full employment economy both before and after the crisis so the distribution of income is not a concern for full employment.

Second, the role of income distribution is to drive borrowing that causes financial fragility. That means their explanation of the crisis is really one of financial market failure in the form of excessive lending that renders the economy vulnerable to shocks. Absent excessive lending, deteriorating income distribution is not a problem except for ethical reasons.

Third, according to the Kumhof and Rancière story the financial crisis was preceded by another adverse worker bargaining power shock that lowered workers' incomes so that they could not pay back their loans. However, there is no evidence of such a shock in 2006-7. Indeed, to the contrary, this was a period of relatively full employment that increased worker bargaining power, as evidenced by rising real wages.

Fourth, the model has difficulty explaining the size of output reduction caused by the financial crisis and why stagnation set in after the Great Recession. Kumhof and Rancière's explanation is to assume the financial crisis destroyed 10 percent of the capital stock, which is implausible.

# 4. "Debt, deleveraging, and the liquidity trap": Eggertsson & Krugman (2012)

A third account of stagnation is the set of explanations associated with the zero lower bound (ZLB) nominal interest rate trap. The originator of this frame of thinking is Paul Krugman (1998) who originally developed it to explain Japan's stagnation after the collapse of its asset price bubble in 1991. Now, Eggertsson and Krugman (2012) have elaborated the story to try and make it explain the stagnation that has followed the US financial crisis of 2008.

The precursor story to stagnation is that a financial bubble drove excessive borrowing and leverage in the US economy. When the bubble burst in 2007/8, the economy experienced a financial crisis and a deep recession. It also prompted a wave of deleveraging as borrowers

shifted to rebuilding their balance sheets. That deleveraging increased saving which the economy has been unable to absorb because of the ZLB. The resulting excess saving has reduced aggregate demand, thereby causing stagnation.

The Krugman-Eggertsson story of stagnation is described in Figure 1. The crux of the story is the claim that there exists an interest rate that yields full employment, and the needed interest rate is determined in the loanable funds market by the supply of saving and investment demand. The role of the interest rate is to balance full employment saving with full employment investment.<sup>3</sup> Deleveraging increases saving and causes an outward shift of the full employment saving supply schedule so that equalizing full employment saving and investment needs a negative real interest rate. However, owing to the ZLB the nominal interest rate cannot go negative. Consequently, there is an excess supply of saving which causes a contraction of income and employment.



**Figure 1** The Eggertsson – Krugman deleveraging explanation of stagnation

The policy solution is two-fold. First, run large budget deficits so that the public sector deficit absorbs the excess private sector saving. Second, encourage inflation expectations so that the expected real interest rate goes negative even if the market nominal interest rate is trapped at zero.

There are multiple features of the ZLB story that are problematic. At the most general level, the ZLB story of stagnation rests on a loanable funds theory of interest rates in which the interest rate is determined by the supply of saving and the demand for investment. That approach to the theory of interest rates was discredited long ago by Keynes (1936) in his *General Theory*.

Second, the ZLB story of stagnation attributes too much significance to interest rates as both the source of the problem and as a means of solving the employment and instability problems

<sup>&</sup>lt;sup>3</sup> In the Krugman – Eggertsson model the central bank achieves this full employment interest rate via its targeting of interest rates.

of a capitalist economy. The claim is a three percent negative real interest rate would increase AD so as to restore full employment. However, real interest rates were negative in the 1970s and that did not solve the employment problems of that era. Today, a three percent negative interest rate would likely trigger a renewed financial bubble that would crash even harder once real interest rates eventually started to reverse upwards. That inconsistency suggests that there is a deeper problem in the economy that the Eggertsson – Krugman (2012) ZLB story fails to identify.

Third, the deleveraging story of excess saving and demand shortage is unconvincing. In fact, as shown in Table 1, US non-financial business debt has been increasing quite fast since 2011. US household debt also shrank little during the Great recession and it too has been increasing since 2012. Furthermore, a significant part of the reduction in household debt likely came from default and debt write-offs, which likely increases aggregate demand and reduces saving by relieving debtors of their obligations.

#### Table 1. Growth of US household and non-financial business debt (%). Source: Financial Accounts of the United States, Federal Reserve, Fourth Quarter 2014.

	2008	2009	2010	2011	2012	2013	2014
Households	1.1	0.0	-1.1	-0.2	1.5	1.5	2.9
Business	5.8	-4.3	-0.9	3.0	4.8	5.1	5.9

Fourth, the Eggertsson – Krugman (2012) explanation of stagnation actually attributes no role for income inequality. Income distribution can be added to the story by assuming higher income households have a higher propensity to save.<sup>4</sup> In that case, a shift in income distribution toward higher income households would increase full employment saving. In terms of Figure 1, it would have an identical effect as deleveraging and would shift the full employment saving function right. However, even though this adds income distribution effects to the Eggertsson – Krugman model, it does not resolve the other criticisms of the model regarding the economic logic and significance of ZLB reasoning. There is need to add income distribution to explain stagnation, but it must be added to another story.

# 5. The economic significance of inequality for stagnation

In addition to introducing the ZLB as an explanation of stagnation, Krugman has persistently contested the economic significance of inequality for explaining stagnation:

<sup>&</sup>lt;sup>4</sup> Palley (2010) provides a comprehensive theoretical justification for differences in the propensity to consume by debtor and creditor households. The theory is consistent with all the established stylized facts of consumption spending including the findings that the long-run aggregate propensity to consume exceeds the short-run propensity (Kuznets, 1946); the cross-section observation that higher income households have a higher propensity to save (Carroll, 2000); and the cross-section observation that the variance of household income exceeds the variance of household consumption (Krueger and Perri, 2002).

"Joe Stiglitz has an Opinionator piece arguing that inequality is a big factor in our slow recovery. Joe is an insanely great economist, so everything he says should be taken seriously. And given my political views and general concerns about inequality, I'd like to agree. But – you knew there was a "but" coming – I've thought about these issues a lot, and haven't been able to persuade myself that this particular morality tale is true" (Krugman, 2013a).

The essence of Krugman's rejection of inequality's economic significance is the fact that US private saving as a share of GDP decreased in the years prior to the financial crisis despite the fact inequality was increasing. As shown in Figure 2, the saving rate declined significantly after 1980 through to 2000, which supposedly proves inequality does not decrease demand:

"So look at overall private saving as a share of GDP: the trend before the crisis was down, not up – and that surge with the crisis clearly wasn't driven by a surge in inequality. So am I saying that you can have full employment based on purchases of yachts, luxury cars, and the services of personal trainers and celebrity chefs? Well, yes. You don't have to like it, but economics is not a morality play... (Krugman, 2013a)."



Figure 2. US private sector saving as a share of GDP, 1947-2012.

What's wrong with this argument that a falling saving rate shows increased income inequality does not cause demand shortage? The problem is it takes no account of other developments that were counteracting and hiding the adverse demand effects of worsening income distribution. This is illustrated in Figure 3. The neoliberal era formally began with the inauguration of President Reagan (in reality, it was already underway with President Carter who initiated the deregulation movement and appointed Paul Volcker with a mandate to crush inflation with high interest rates). As argued in Palley (2012), the shift to neoliberal policy generated two fundamental changes. The first was an era of wage stagnation and widening

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income inequality. The second was an era of asset price inflation and a thirty year-long credit bubble which increased wealth, collateral, the quantity of credit, and ease of access to credit. Those financial developments fuelled spending that more than offset the negative impacts of wage stagnation, and they explain why the saving rate fell even as income inequality was rising. The credit bubble ended with the financial crisis, bringing to an end the era of outlandish borrowing. That caused the saving rate to rebound, causing demand shortage. This explanation fits the facts in both Table 1 and Figure 2, showing that increased saving caused by income inequality rather than deleveraging is responsible for stagnation.





Cynamon and Fazzari (2014) provide strong evidence supporting this pattern of events. They report that income growth of the bottom 95 percent of households stagnated pre-2006, but the debt-income ratio of those households rose to unsustainable levels. Since the Great Recession that debt-income ratio has come down to more sustainable levels via a process of debt-default, tightened credit access and recognition by households that future asset price inflation was not going to pay-off debts. Their findings fully support the hypothesis that borrowing covered up the adverse demand effects of inequality before the crisis, and the demand drag of inequality surfaced when the borrowing binge came to a close.

# 6. The structural Keynesian account of inequality and stagnation: Palley (2009, 2012)

The above argument shows that income distribution matters, but it must also be incorporated in a better macroeconomic story than that offered by ZLB proponents. This section presents a "structural Keynesian" account (Palley, 2009, 2012) of the financial crisis and stagnation – which was written long before stagnation was identified by mainstream economists like Larry Summers. That makes the structural Keynesian account rather unusual for economics as it correctly anticipated imminent developments.

The explanation runs as follows. Until the late 1970s developed country economies, including the US, could be described by a Keynesian virtuous circle growth model in which wages were

the engine of demand growth. The economic logic is illustrated in Figure 4. Productivity growth drove wage growth which fuelled demand growth. That promoted full employment which provided the incentive to invest, which drove further productivity growth. Within this system, finance was characterized by a public utility model based on New Deal regulation. Its role was to (a) provide business and entrepreneurs with finance for investment; (b) provide business and households with insurance services, and (c) provide households with means of saving for future needs.

Figure 4. The 1945 – 75 virtuous circle Keynesian growth model.



After 1980 the virtuous circle Keynesian growth model was replaced by a neoliberal growth model. The two key changes in the real economy were: 1) abandonment of the policy commitment to full employment which was replaced by a commitment to stable low inflation; and 2) severing of the link between wages and productivity growth. Additionally, there was change in the financial sector driven by the phenomenon of "financialization" which increased the presence and power of finance within the economy. Together, these changes created a new economic model. Before 1980, wages were the engine of demand growth: after 1980, debt and asset price inflation became the engines of demand growth.

As shown in Figure 5, the new economic model can be described as a "neoliberal policy box" that fences workers in and pressures them from all sides via (1) the corporate model of globalization; (2) the small government agenda that attacks regulation and public sector activity; (3) the labor market flexibility agenda that attacks unions, worker bargaining power and worker protections; and (4) the replacement of full employment macroeconomic policy with low inflation targeting policy. With regard to the financial system, the New Deal public utility model was gutted by deregulation and subsequent financial innovations were left largely unregulated. The result was a new system characterized by growing financial instability, wage stagnation and increased income inequality.

Figure 5 The neoliberal box



These wage and income developments created a growing structural demand shortage. The role of finance was to fill that gap. Financial deregulation, financial innovation, speculation, and old fashioned financial fraud enabled finance to fill the demand gap by lending to consumers and by inflating asset prices.

There are several features to note. First, having finance fill this "demand gap" was not part of a grand plan: it was an unintended consequence. Neoliberal economic policymakers did not realize they were a creating a demand gap, but their *laissez-faire* financial ideology unleashed developments that accidentally filled it. Second, the process was inevitably unstable and was always destined to implode. There are limits to borrowing and asset price inflation. Every Ponzi scheme comes apart eventually. The problem it is impossible to predict when it will end. Third, the process was of long duration. Consequently, the collapse was far deeper when it eventually happened. It also means escaping the after-effects is far more difficult because the economy is now burdened by debt and destroyed credit worthiness.

# 7. The structural Keynesian view of the role of inequality in the crisis and stagnation

The above structural Keynesian account of events is subtly different from popular accounts. Income inequality did not cause the financial crisis. The crisis was caused by the implosion of the asset price and credit bubbles which had been off-setting and obscuring the impact of inequality. However, once the financial bubble burst and financial markets ceased filling the demand gap created by income inequality, the demand effects of inequality came to the fore. Viewed in that light, stagnation is the joint-product of the long-running credit bubble, the financial crisis and income inequality. The credit bubble left behind a large debt over-hang; the financial crisis destroyed the credit-worthiness of millions; and income inequality has created a "structural" demand shortage.

This diagnosis also makes clear why the medium-term prognosis remains stagnation. That is because policy has not repaired these fundamental problems and they have actually worsened. First, the US still has a structural "demand gap" caused by deteriorated income distribution and income distribution has actually worsened since the crisis of 2008. Second, the credit bubble is over so that borrowing can no longer fill the "demand gap". Furthermore,

financial sector reforms have systemically tightened credit access. Third, the import and investment leakages associated with globalization remain unrepaired, while fiscal stimulus has turned to fiscal austerity. Consequently, despite the Federal Reserve's zero interest rate and quantitative easing (QE) policies, the economy is beset by slower growth and overall labor market slack stands to be permanently higher. Furthermore, there is a danger that having re-inflated asset prices, the QE experiment will backfire in the form of renewed financial market turmoil.

# 8. The story we accept matters

The previous sections have described four different stories regarding the role of income inequality in causing the financial crisis and stagnation. Which story we accept matters enormously because the way we explain the world affects how we understand it, which in turn has major political and policy consequences.

If Rajan's (2010) story is accepted income distribution is reduced to an issue of political and ethical concern, but it is not an issue of macroeconomic concern. Furthermore, since labor markets are working as they are supposed to, there is no justification for interventions in labor markets aimed at increasing the wage share or strengthening worker bargaining power. Rather than focusing on income inequality, the economic policy response should be to repeal government interventions in housing finance and return to more orthodox monetary policy to avoid possibilities of another asset price bubble. There may also be case for some after-tax income redistribution but that is a purely ethical and political matter.

If the Kumhof and Rancière (2010) story is accepted, the cause of the crisis is financial market failure that allowed excess borrowing by worker households whose income prospects had diminished. The policy response should be to tighten financial market regulation to prevent a repeat of an unsound lending bubble. However, once again, labor markets are actually working efficiently. That means the case for income redistribution aimed at increasing the wage share is again purely ethical and political.

If the Eggertsson - Krugman (2012) ZLB deleveraging story is accepted, income distribution is again reduced to a non-economic issue. Instead, the cause of stagnation is deleveraging which is a process to be worked through. However, during this period there is a case for large budget deficits to offset excess private saving caused by deleveraging, and thereby avoid any output and employment losses caused by the ZLB obstruction to full employment. Since the labor market is efficient and not the cause of the problem, it means income distribution is again a purely ethical and political matter and there is no economic case for interventions aimed at increasing wage share.

If the "structural Keynesian" story is accepted, income distribution is a central problem and the principal factor explaining the demand shortage that is the cause stagnation. The solution is to replace the neoliberal policy framework with a "structural Keynesianism" framework. Metaphorically speaking, policymakers needs to repack the box, take workers out, and put corporations and financial markets in. As illustrated in Figure 6, that requires replacing corporate globalization with managed globalization; restoring macroeconomic policy commitment to full employment; replacing the anti-government agenda with a social democratic agenda that supports and funds public investment, provision of public services

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and regulation (including financial markets); and replacing neoliberal labor market flexibility with solidarity based labor markets in which workers have greater bargaining power and receive an increased wage share.

#### Figure 6 Repack the box



However, there is an additional layer of complexity associated with financialization, which makes today's political economy different from the past. Repacking the economic policy box requires regaining control over financial interests which have played a critical role in creating and maintaining the new economic model. This role of finance is illustrated in Figure 7. First, finance used its political power to promote the policies on which the new model rests. Scratch any side of the neoliberal policy box and you find the influence of finance. Thus, finance lobbied for financial deregulation; it supported the shift of macroeconomic policy away from focusing on full employment to focusing on inflation; it supported corporate globalization and expanding international capital mobility; it supported the attack on unions and labor aimed at lowering wages.

Figure 7 The main conduits of financialization



Second, finance took control of business and compelled it to adopt financial sector behaviors and perspectives. The change was justified using the rationale of shareholder value maximization. The result was adoption of the leverage buyout model that loaded firms with debt; the adoption of a short-term business perspective; the adoption of excessively high required rates of return that undercut long-term investment; support for offshoring and abandonment of commitment to communities; and adoption of Wall Street-styled pay packages for directors and top management.

Third, deregulated financial markets and financial innovation provided the credit to finance leveraged buy-outs, takeovers, and stock buybacks. They also supported mortgage and consumer borrowing that inflated house prices and temporarily filled the "demand shortage" created by wage stagnation. Finance covered over the demand gap created by the neoliberal policy model, but it did so at the cost of creating an increasingly fragile financial structure that eventually imploded with the crisis of 2008.

The outline of a program to regain control of finance (Palley, 2014) might be as follows. Political and electoral reform that diminishes the role of private money; changing monetary policy so that it gives more weight to full employment relative to inflation; corporate governance reform that discourages management's short-term perspective and focus on maximizing share price; and financial regulatory reform that permits use of quantitative policy to manage the size and composition of financial firms' balance sheets.

# 9. Inequality and economic policy failure as the cause of stagnation?

Thus far, the focus has been on the economic role of inequality in generating stagnation. Political economy provides another channel of impact by having inequality affect economic policy. Indeed, Krugman (2013b) argues that political economy has been the main channel. His argument is increased inequality increased the political power of the wealthy who favored policies of fiscal austerity that caused stagnation:

"In my view, however, the really crucial role of inequality in economic calamity has been political. In the years before the crisis there was a remarkable bipartisan consensus in Washington in favor of financial deregulation – a consensus justified by neither theory nor history. When crisis struck, there was a rush to rescue the banks. But as soon as that was done, a new consensus emerged, one that involved turning away from job creation and focusing on the alleged threat from budget deficits... Surveys of the very wealthy have, however, shown that they – unlike the general public – consider budget deficits a crucial issue and favor big cuts in safety-net programs. And sure enough, those priorities took over our political discourse" (Krugman, 2013b).

According to Krugman, stagnation is the result of failure to use fiscal policy to offset deleveraging, and that policy failure can be attributed to the political effects of increased income inequality.

There are several important points to note. First, this political economy argument is fully consistent with the structural Keynesian hypothesis. Indeed, Palley (2012, p.205-7) explicitly argues that power and wealth have shaped economic ideas that have pushed neoliberal policy. Increased income inequality has only further strengthened that shaping.

Second, albeit unintentionally, Krugman's political economy argument gets to the heart of the economic debate. For Krugman, there is nothing "structurally" wrong with the economy. It is in a process of deleveraging that needs to be worked through, and fiscal stimulus can help work through that process faster and with less pain. In contrast, the structural Keynesian hypothesis roots stagnation in the flawed structure of the economy. The adoption of fiscal austerity has definitely aggravated stagnation, but it is not the deep cause.

Third, the idea that economic policy is the cause of stagnation is common to both Krugman's view and the structural Keynesian view. However, as with the debate over the economic impact of income inequality, it is important to get the story straight regarding the role of economic policy. For Krugman (2013b), the policy failure is the turn to fiscal austerity after 2009. That contrasts with the structural Keynesian hypothesis which traces the policy failure back to the late 1970s and the shift to neoliberal policies. That is a very different story with very different policy implications. It shows, once again, the importance of getting the story right.

### **10.** The resistance of mainstream economic theory to inequality

Rajan, Kumhof and Rancière, and Krugman are leading mainstream economists. Their associations include the University of Chicago, the IMF, and MIT. In terms of intellectual disposition, Rajan is identified with the hardcore neoliberalism of the Chicago school which views the economy as approximating the textbook model of perfect competition. Market failure is argued to be rare and relatively small. Furthermore, even if not small, government policy intervention to correct market failure produces even worse outcome because it is subject to government failure that is more costly than market failure.

Kumhoff, Rancière and Krugman are identified with the softcore neoliberalism of the MIT school. They believe in the same benchmark perfectly competitive model as hardcore neoliberals. However, market failures are argued to be pervasive and large, and government policy is claimed to do a good job remedying their effects.

The relation between hardcore and softcore neoliberalism is shown in Figure 8. The important point is that Rajan, Kumhof and Rancière, and Krugman all share a common mainstream theoretical view of the economy – though they differ on the extent of market failure and the effectiveness of corrective government policy intervention. That view contrasts significantly with the non-mainstream structural Keynesian view.



#### Figure 8 The intellectual structure of modern economics

This contrast is particularly sharp with regard to the issue of inequality. Mainstream economics has deep intellectual resistance to recognizing the efficiency impacts of inequality, possibly because inequality is the most politically contentious issue. Recognizing its efficiency impacts would provide compelling reason to remedy it, which would involve challenging the *status quo* and elite moneyed interests.

One source of resistance to recognizing the macroeconomic efficiency effects of inequality is the Arrow-Debreu (1954) competitive general equilibrium model that remains the analytic heart of mainstream theoretical economics. That model benchmarks an "ideal" economy and it generates the two famous welfare theorems. The first welfare theorem states that perfectly competitive economies, with no market or information failures, generate Pareto optimal equilibrium outcomes. Such economies are productively and allocatively efficient in the sense that no person can be made better-off without making another worse-off, and this result holds regardless of how equal or unequal is the initial distribution of wealth.

The second welfare theorem states that in an ideal economy the only way to redistribute wealth and income without generating productive or allocative inefficiencies is via lump-sum taxes. Since such taxes are impossible in the real world, that makes it impossible to redress inequality without incurring efficiency losses.

These two theorems only hold for an ideal economy, but they benchmark mainstream economists' thinking in a way that produces two biases. First, inequality does not matter for economic efficiency. Second, redressing inequality is likely to increase economic inefficiency.

A second source of intellectual resistance is neoclassical microeconomic behavior theory which imparts a favorable disposition toward inequality. That disposition is captured by Arthur Okun, a major liberal economist of the past, who wrote:

"The contrasts among American families in living standards and in material wealth reflect a system of rewards and penalties that is intended to encourage effort and channel it into socially productive activity. To the extent

the system succeeds, it generates an efficient economy. But that pursuit of efficiency necessarily creates inequalities. And hence society faces a tradeoff between equality and efficiency..." (Okun, 1975, p.1).

This incentive argument has seeped deeply into economics and societal thinking, both of which accept Okun's claim of a big trade-off between equality and efficiency.

A third source of intellectual resistance and indifference to inequality comes from macroeconomics and conventional theories of consumption. According to the permanent income hypothesis (Friedman, 1957) all individuals have the same marginal propensity to consume, rendering income distribution and inequality irrelevant for aggregate demand. According to life-cycle consumption theory (Modigliani and Brumberg, 1954), the propensity consume depends on an individual's age. The age distribution of society and the distribution of income across households of different ages is what matters for aggregate consumption, and not income distribution *per se*.

These combined arguments – Arrow-Debreu competitive general equilibrium theory, neoclassical microeconomic incentive theory, and macroeconomic consumption theory – have contributed to mainstream economists' indifference or even support for inequality. That helps explain why inequality is so absent in mainstream explanations of stagnation. In contrast, Keynesian economics has a very different perspective in which inequality can be a source of major macroeconomic inefficiency.

The Keynesian argument begins with dismissal of the Arrow-Debreu ideal economy and its claims to full employment. Instead, the real world economy is described as a monetary economy marked by fundamental uncertainty regarding the future, and in which aggregate demand can fall when people delay spending plans in response to uncertainty. Furthermore, a market system may be unable to restore a level of aggregate demand sufficient to ensure full employment because lower prices and deflation increase debt burdens, encourage people to further delay spending, and induced defaults may disrupt the banking system and upend financial markets.

According to Keynesian economics, aggregate demand is the decisive factor determining economic activity. Furthermore, consumption spending is affected by inequality (Palley, 2010) as richer households have a higher propensity to save than poorer households. Consequently, increased inequality can increase saving and lower aggregate demand, causing Keynesian unemployment that the market cannot remedy.

As regards microeconomic incentive theory, motivations for behavior are far more varied and malleable than suggested by Okun (1975, p.1). Okun's view reflects an American perspective. In a society where money is the dominant metric of individual self-worth and self-esteem, monetary incentives are likely to be much more powerful. However, it is also possible to have societies where other metrics of worth and esteem are prominent, and in these societies monetary incentives will be less powerful. The implication is what motivates us is socially constructed in important ways, which dramatically challenges the view of a hard and sharp trade-off between efficiency and inequality.

Furthermore, the nature of permitted incentive arrangements also matters enormously. Winner-take-all tournaments are a powerful form of motivation, especially in a society where money is the metric of worth and social protections are weak. However, they can be socially

sub-optimal in that the tournament rules are set by the owners who collect the tournament surplus, and worker participants may well prefer other forms of incentive arrangement. That is the lesson of the economics of the rat race which generates an ugly race-to-the bottom (Akerlof, 1976; Palley, 1998, p.9).

In sum, the structural Keynesian perspective on inequality is fundamentally different from the mainstream view. Inequality is a source of aggregate demand failure, and inequality driven incentive systems can be socially sub-optimal. However, the academic monopoly of Chicago – MIT neoliberal economics hinders that view from getting a hearing.

# 11. Conclusion: gattopardo economics again

There are three major conclusions. First, the four stories above have superficial similarities in their mention of either "income distribution" or "demand shortage", but they are actually fundamentally different. If readers do not have their wits about them, it is easy to miss those fundamental differences.

That potential for confusion is increased by the fact that different stories can lead to overlapping policy recommendations. For instance, Krugman's ZLB story recommends using fiscal stimulus, as does the structural Keynesian story. However, the two stories are fundamentally different in their explanation of the roots of the financial crisis and stagnation. That raises a critical issue. It is not enough to find points of policy agreement: there is also need to get the story about the economy right. A wrong story misleads policy makers and the public regarding how to think about the economy; encourages an incomplete policy response; and sets up future analytical and policy disagreements that are politically damaging.

Second, there is a great danger of "gattopardo economics" (Palley, 2013), which is change that leaves economics unchanged. For thirty years, progressive Keynesians have argued for the macroeconomic significance of income distribution. Now, mainstream economists are picking up on this issue. The gattopardo danger is that they will incorporate it into their stories in ways that strip income distribution of its critical significance for macroeconomic efficiency, thereby cannibalizing the case for policy interventions to reduce income inequality.

Third, the paper described four stories. Three of them are widely cited and known. They are taught in graduate schools and discussed by the IMF and central banks. The fourth (the structural Keynesian story) is consigned to a black hole. It is not because of lack of evidence or logic. In fact, its logic and evidence are superior. Instead, it is buried because of the "power of interests" that ensure only certain ideas make it into the classroom and on to the stage of public debate. Those interests include the wealthy, but they also include the economics profession which is structured like a club and only gives voice to the ideas of existing club members.

These conclusions carry an important practical implication. Given the vital significance of "getting the story right", progressive action aimed at policy change must be accompanied by vigorous efforts to challenge and replace the mainstream economic story. Changing the story is a two-part project. First, it requires disseminating the alternative structural Keynesian account of the crisis and stagnation. Second, it involves challenging mainstream economic theory that is the deep foundation of both hardcore and softcore neoliberalism. Absent a

change of economic story, progressives are unlikely to win the political debate about the policies and economic arrangements necessary for shared prosperity and the good society.

That failure is visible in political developments since the financial crisis of 2008. The failure to change the story has seen economic policy significantly revert to pre-crisis tropes, including fiscal austerity, labor market flexibility and more corporate globalization. Only monetary policy remains in a different mode, but it too threatens to revert to pre-crisis mode at the first whiff of inflation. As for electoral politics, in the US the Republican Party has made large political gains; in the UK the Conservative Party has trounced the Labor Party; and in Germany the conservative Christian Democrats have trounced the Social Democrats. In part, these political developments reflect the failure to get the story right and offer electorates a clearly defined alternative structural Keynesian narrative.

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# Deductivism – the fundamental flaw of mainstream economics

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"Confusion of sign and object is original sin coeval with the word" W. v. O. Quine

#### Introduction

In science one could argue that there basically are three kinds of argumentation patterns / schemes / methods / strategies available – deduction, induction and abduction.

In this paper it will be argued that the failings of the mainstream modeling strategy are related to how mainstream economics (mis)uses the first two of these three modes of inference and – with severe negative analytical consequences – to a large degree disregard the third one.

Fixation on constructing models showing the certainty of logical entailment – *realiter* simply collapsing the necessary ontological gap between model and reality – has been detrimental to the development of a relevant and realist economics. Insisting on formalistic (mathematical) modeling forces the economist to give upon on realism and substitute axiomatics for real world relevance. The price for rigour and precision is far too high for anyone who is ultimately interested in using economics to pose and (hopefully) answer real world questions and problems.

The deductivist orientation is the main reason behind the difficulty that mainstream economics has in terms of understanding, explaining and predicting what takes place in our societies. But it has also given mainstream economics much of its discursive power – at least as long as no one starts asking tough questions on the veracity of – and justification for – the assumptions on which the deductivist foundation is erected. Asking these questions is an important ingredient in a sustained critical effort at showing how nonsensical is the embellishing of a smorgasbord of models founded on wanting (often hidden) methodological foundations.

The mathematical-deductivist straitjacket used in mainstream economics presupposes atomistic closed-systems – i.e., something that we find very little of in the real world, a world significantly at odds with an (implicitly) assumed logic world where deductive entailment rules the roost. Ultimately then, the failings of modern mainstream economics has its root in a deficient ontology. The kind of formal-analytical and axiomatic-deductive mathematical modeling that makes up the core of mainstream economics is hard to make compatible with a real-world ontology. It is also the reason why so many critics find mainstream economic analysis patently and utterly unrealistic and irrelevant.

Although there has been a clearly discernible increase and focus on "empirical" economics in recent decades, the results in these research fields have not fundamentally challenged the main deductivist direction of mainstream economics. They are still mainly framed and

interpreted within the core "axiomatic" assumptions of individualism, instrumentalism and equilibrium (cf. Arnsperger and Varoufakis (2006)) that make up even the "new" mainstream economics. Although, perhaps, a sign of an increasing – but highly path-dependent – theoretical pluralism, mainstream economics is still, from a methodological point of view, mainly a deductive project erected on a foundation of empty formalism.

If we want theories and models to confront reality there are obvious limits to what can be said "rigorously" in economics. For although it is generally a good aspiration to search for scientific claims that are both rigorous and precise, we have to accept that the chosen level of precision and rigour must be relative to the subject matter studied. An economics that is relevant to the world in which we live can never achieve the same degree of rigour and precision as in logic, mathematics or the natural sciences. Collapsing the gap between model and reality in that way will never give anything else than empty formalist economics.

In mainstream economics, with its addiction to the deductivist approach of formalmathematical modeling, model consistency trumps coherence with the real world. That is sure getting the priorities wrong. Creating models for their own sake is not an acceptable scientific aspiration – impressive-looking formal-deductive (mathematical) models should never be mistaken for truth.

### Deduction

Premise 1: All Chicago economists believe in REH Premise 2: Robert Lucas is a Chicago economist

Conclusion: Robert Lucas believes in REH

Here we have an example of *a logically* valid deductive inference (and, following Quine, whenever logic is used in this essay, "logic" refers to deductive/analytical logic).

In a hypothetico-deductive reasoning – hypothetico-deductive *confirmation* in this case – we would use the conclusion to test the law-like hypothesis in premise 1 (according to the hypothetico-deductive model, a hypothesis is confirmed by evidence if the evidence is deducible from the hypothesis). If Robert Lucas does not believe in REH we have gained some warranted reason for non-acceptance of the hypothesis (an obvious shortcoming here being that further information beyond that given in the explicit premises might have given another conclusion).

The hypothetico-deductive method (in case we treat the hypothesis as absolutely sure/true, we rather talk of an *axiomatic-deductive* method) basically means that we

- Posit a hypothesis
- Infer empirically testable propositions (consequences) from it
- Test the propositions through observation or experiment
- Depending on the testing results either find the hypothesis corroborated or falsified.

However, in science we regularly use a kind of "practical" argumentation where there is little room for applying the restricted logical "formal transformations" view of validity and inference.

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Most people would probably accept the following argument as a "valid" reasoning even though it from a strictly logical point of view is non-valid:

Premise 1: Robert Lucas is a Chicago economist Premise 2: The recorded proportion of Keynesian Chicago economists is zero

Conclusion: So, certainly, Robert Lucas is not a Keynesian economist

How come? Well I guess one reason is that in science, contrary to what you find in most logic text-books, not very many argumentations are settled by showing that "All Xs are Ys". In scientific practice we instead present other-than-analytical explicit warrants and backings – data, experience, evidence, theories, models – for our inferences. As long as we can show that our "deductions" or "inferences" are justifiable and have well-backed warrants, other scientists will listen to us. That our scientific "deductions" or "inferences" are logical non-entailments simply is not a problem. To think otherwise is committing the fallacy of misapplying formal-analytical logic categories to areas where they are pretty much irrelevant or simply beside the point.

Scientific arguments are not analytical arguments, where validity is solely a question of formal properties. Scientific arguments are *substantial* arguments. If Robert Lucas is a Keynesian or not, is nothing we can decide on formal properties of statements/propositions. We have to check out what the guy has actually been writing and saying to check if the hypothesis that he is a Keynesian is true or not.

In a *deductive-nomological* explanation – also known as a *covering law explanation* – we would try to explain why Robert Lucas believes in REH with the help of the two premises (in this case actually giving an explanation with very little explanatory value). These kinds of explanations – both in their *deterministic* and *statistic/probabilistic* versions – rely heavily on deductive entailment from assumed to be true premises. But they have preciously little to say on where these assumed to be true premises come from.

Deductive logic of confirmation and explanation may work well – given that they are used in deterministic closed models! In mathematics, the deductive-axiomatic method has worked just fine. But science is not mathematics. Conflating those two domains of knowledge has been one of the most fundamental mistakes made in the science of economics. Applying the deductive-axiomatic method to real world systems, however, immediately proves it to be excessively narrow and hopelessly irrelevant. Both the confirmatory and explanatory ilk of hypothetico-deductive reasoning fails since there is no way you can relevantly analyze confirmation or explanation as a purely logical relation between hypothesis and evidence or between law-like rules and explananda. In science we argue and try to substantiate our beliefs and hypotheses with reliable evidence – propositional and predicate deductive logic, on the other hand, is not about *reliability*, but the *validity* of the conclusions *given* that the premises are true.

Deduction – and the inferences that go with it – is an example of "explicative reasoning", where the conclusions we make are already included in the premises. Deductive inferences are purely *analytical* and it is this truth-preserving nature of deduction that makes it different from all other kinds of reasoning. But it is also its limitation, since truth in the deductive context does not refer to a real world ontology (only relating propositions as true or false

within a formal-logic system) and as an argument scheme, deduction is totally non-ampliative – the output of the analysis is nothing else than the input.

Just to give an economics example, consider the following rather typical, but also uninformative and tautological, deductive inference:

Premise 1: The firm seeks to maximize its profits Premise 2: The firm maximizes its profits when marginal cost equals marginal income

Conclusion: The firm will operate its business at the equilibrium where marginal cost equals marginal income

This is as empty as deductive-nomological explanations of singular facts building on simple generalizations:

Premise 1: All humans are less than 20 feet tall Premise 2: Robert Lucas is a human

Conclusion: Robert Lucas is less than 20 feet tall

Although a logically valid inference, this is not much of an explanation (since we would still probably want to know why *all* humans are less than 20 feet tall).

Deductive-nomological explanations also often suffer from a kind of emptiness that emanates from a lack of real (causal) connection between premises and conclusions:

Premise 1: All humans that take birth control pills do not get pregnant Premise 2: Lars Syll took birth control pills

Conclusion: Lars Syll did not get pregnant

I guess most people would agree that this is not much of a real explanation.

Learning new things about reality demands something else than a reasoning where the knowledge is already embedded in the premises. These other kinds of reasoning may give *good* – but not *conclusive* – reasons. That is the price we have to pay if we want to have something substantial and interesting to say about the real world.

### Induction

Premise 1: This is a randomly selected large set of economists from Chicago Premise 2: These randomly selected economists all believe in REH

Conclusion: All Chicago economists believes in REH

In this inductive inference we have an example of a logically non-valid inference that we would have to supply with strong empirical evidence to really warrant. And that is no simple matter at all, as Keynes (1973 (1921): 468f) noticed:

"In my judgment, the practical usefulness of those modes of inference, here termed Universal and Statistical Induction, on the validity of which the boasted knowledge of modern science depends, can only exist—and I do not now pause to inquire again whether such an argument must be circular—if the universe of phenomena does in fact present those peculiar characteristics of atomism and limited variety which appear more and more clearly as the ultimate result to which material science is tending...

The physicists of the nineteenth century have reduced matter to the collisions and arrangements of particles, between which the ultimate qualitative differences are very few...

The validity of some current modes of inference may depend on the assumption that it is to material of this kind that we are applying them... Professors of probability have been often and justly derided for arguing as if nature were an urn containing black and white balls in fixed proportions. Quetelet once declared in so many words— 'l'urne que nous interrogeons, c'est la nature'. But again in the history of science the methods of astrology may prove useful to the astronomer; and it may turn out to be true—reversing Quetelet's expression—that 'La nature que nous interrogeons, c'est une urne'."

But even though induction is more demanding in terms of justification than deduction, we should not draw the conclusion that it is no inference at all:

"Now it might be charged that moving from such facts as that F's have always been followed by C's, to the claim that F's obtaining is a good reason for expecting C, – that this is not an inference at all; not when one's only defence consists in citing more facts, namely the specific meteorological, botanical, and biological data which support the general claim that F has regularly preceded C. Entailment it may not be, granted. But inference it certainly is, as must be every case of drawing reasonable conclusions from evidence." N. R. Hanson (1971:242)

Justified inductions presupposes a *resemblance* of sort between what we have experienced and know, and what we have not yet experienced and do not yet know. Just to exemplify this problem of induction let me take two examples.

Let's start with this one. Assume you're a Bayesian turkey and hold a nonzero probability belief in the hypothesis H that "people are nice vegetarians that do not eat turkeys and that every day I see the sun rise confirms my belief." For every day you survive, you update your belief according to Bayes' Rule

P(H|e) = [P(e|H)P(H)]/P(e),

where evidence e stands for "not being eaten" and P(e|H) = 1. Given that there do exist other hypotheses than H, P(e) is less than 1 and a fortiori P(H|e) is greater than P(H). Every day you survive increases your probability belief that you will not be eaten. This is totally rational according to the Bayesian definition of rationality. Unfortunately – as Bertrand Russell

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famously noticed – for every day that goes by, the traditional Christmas dinner also gets closer and closer...

Or take the case of macroeconomic forecasting, which perhaps better than anything else illustrates the problem of induction in economics. As a rule macroeconomic forecasts tend to be little better than intelligent guesswork. Or in other words – macroeconomic mathematical-statistical forecasting models, and the inductive logic upon which they ultimately build, are as a rule far from successful. The empirical and theoretical evidence is clear. Predictions and forecasts are inherently difficult to make in a socio-economic domain where genuine uncertainty and unknown unknowns often rule the roost. The real processes underlying the time series that economists use to make their predictions and forecasts do not confirm with the inductive assumptions made in the applied statistical and econometric models. The forecasting models fail to a large extent because the kind of uncertainty that faces humans and societies actually makes the models strictly seen inapplicable. The future is inherently unknowable – and using statistics and econometrics does not in the least overcome this ontological fact. The economic future is not something that we normally can predict in advance. Better then to accept that as a rule "we simply do not know".

Induction is sometimes a good guide for evaluating hypotheses. But for the creative generation of plausible and relevant hypotheses it is conspicuously silent. For that we need, as noted already by Peirce (1931:§145), another – non-algorithmic and ampliative – kind of reasoning.

#### Abduction

Premise 1: All Chicago economists believe in REH Premise 2: These economists believe in REH

Conclusion: These economists are from Chicago

In this case, again, we have an example of a logically non-valid inference – the fallacy of affirming the consequent:

 $\frac{p \Longrightarrow q}{p}$ 

or, in instantiated form

$$\begin{array}{c} \forall x \ (Gx => Px) \\ \hline Pa \\ \hline Ga \end{array}$$

But it is nonetheless an inference that may be a strongly warranted and *truth-producing* – in contradistinction to *truth-preserving* deductions – reasoning, following the general pattern

Evidence => Explanation => Inference.

Here we infer something based on what would be the best explanation given the law-like rule (premise 1) and an observation (premise 2). The truth of the conclusion (explanation) is nothing that is *logically* given, but something we have to justify, argue for, and test in different ways to possibly establish with any certainty or degree. And as always when we deal with explanations, what is considered best is relative to what we know of the world. In the real world all evidence has an irreducible holistic aspect. We never conclude that evidence follows from hypothesis *simpliciter*, but always given some more or less explicitly stated contextual background assumptions. All non-deductive inferences and explanations are *a fortiori* context-dependent.

If extending the abductive scheme to incorporate the demand that the explanation has to be the *best* among a set of *plausible* competing/rival/contrasting potential and satisfactory explanations, we have what is nowadays usually referred to as *inference to the best explanation* (IBE). In this way IBE is a refinement of the original (Peircean) concept of abduction by making the background knowledge requirement more explicit.

In abduction we start with a body of (purported) data/facts/evidence and search for explanations that can account for these data/facts/evidence. Having the best explanation means that you, given the context-dependent background assumptions, have a satisfactory explanation that can explain the fact/evidence better than any other competing explanation – and so it is *reasonable* to consider/believe the hypothesis to be true. Even if we do not (inevitably) have deductive certainty, our abductive reasoning gives us a license to consider our belief in the hypothesis as reasonable. The model of inference to the best explanation is, as Peter Lipton (2000:184) writes,

"...designed to give a partial account of many inductive inferences, both in science and in ordinary life... Its governing idea is that explanatory considerations are a guide to inference, that scientists infer from the available evidence to the hypothesis which would, if correct, best explain that evidence. Many inferences are naturally described in this way... When a detective infers that it was Moriarty who committed the crime, he does so because this hypothesis would best explain the fingerprints, blood stains and other forensic evidence. Sherlock Holmes to the contrary, this is not a matter of deduction. The evidence will not entail that Moriarty is to blame, since it always remains possible that someone else was the perpetrator. Nevertheless, Holmes is right to make his inference, since Moriarty's guilt would provide a better explanation of the evidence than would anyone else's.

Inference to the Best Explanation can be seen as an extension of the idea of 'self-evidencing' explanations, where the phenomenon that is explained in turn provides an essential part of the reason for believing the explanation is correct... According to Inference to the Best Explanation, this is a common situation in science: hypotheses are supported by the very observations they are supposed to explain. Moreover, on this model, the observations support the hypothesis precisely because it would explain them."

Accepting a hypothesis means that you consider it to explain the available evidence better than any other competing hypothesis. The acceptability warrant comes from the explanatory power of the hypothesis, and the conscious act of trying to rule out the possible competing potential explanations in itself increases the plausibility of the preferred explanation. Knowing that we – after having earnestly considered and analysed the other available potential explanations – have been able to *eliminate* the competing potential explanations, warrants and enhances the confidence we have that our preferred explanation is the best – "loveliest" – explanation, i.e., the explanation that provides us with the greatest understanding (given it is correct). As Sherlock Holmes had it (in *The Sign of Four*): "Eliminate the impossible, and whatever remains, however improbable, must be the truth". Subsequent confirmation of our hypothesis – by observations, experiments or other future evidence – makes it even more well-confirmed (and underlines that all explanations are incomplete, and that the models and theories that we as scientists use, cannot only be assessed by the extent of their fit with experimental or observational data, but also need to take into account their explanatory power).

This, of course, does not in any way mean that we cannot be wrong. Of course we can. But as Alan Musgrave (2010:94) writes:

"Quite so – and so what? It goes without saying that any explanation might be false, in the sense that it is not necessarily true. It is absurd to suppose that the only things we can reasonably believe are necessary truths.

What if the best explanation not only might be false, but actually is false. Can it ever be reasonable to believe a falsehood? Of course it can... What we find out is that what we believed was wrong, not that it was wrong or unreasonable for us to have believed it.

People object that being the best available explanation of a fact does not prove something to be true or even probable. Quite so – and again, so what? The explanationist principle – 'It is reasonable to believe that the best available explanation of any fact is true' – means that it is reasonable to believe or think true things that have not been shown to be true or probable, more likely true than not."

Abductions are *fallible* inferences – since the premises do not logically entail the conclusion – so from a *logical* point of view, abduction is a weak mode of inference. But if the abductive arguments put forward are strong enough, they can be warranted and give us justified true belief, and hence, knowledge, even though they are fallible inferences. As scientists we sometimes – much like Sherlock Holmes and other detectives that use abductive reasoning – experience disillusion. We thought that we had reached a strong abductive conclusion by ruling out the alternatives in the set of contrasting explanations. But – what we thought was true turned out to be false. But that does not necessarily mean that we had no good reasons for believing what we believed. If we cannot live with that contingency and uncertainty, well, then we're in the wrong business. If it is deductive certainty you are after, rather than the *ampliative* and *defeasible* reasoning in abduction – well, then get in to math or logic, not science.

What makes the works of people like Galileo, Marx, or Keynes, truly interesting is not that they describe new empirical facts. No, the truly seminal and pioneering aspects of their works is that they managed to find out and analyse what makes empirical phenomena possible. What are the fundamental physical forces that make heavy objects fall the way they do? Why

do people get unemployed? Why are market societies haunted by economic crises? Starting from well known facts these scientists discovered the mechanisms and structures that made these empirical facts possible.

"Newton pressed on; Einstein, DeBroglie, Schrödinger, Heisenberg and Dirac pressed on – for explanations, which no amount of statistical repetition or deductive ingenuity could ever supply ... From the observed properties of phenomena the physicist reasons his way towards a keystone idea from which the properties are explicable as a matter of course. The physicist seeks not a set of possible objects, but a set of possible explanations" N. R. Hanson (1965:88).

The works of these scientists are good illustrations of the fact that in science we are usually not only interested in observable facts and phenomena. Since structures, powers, institutions, relations, etc., are not *directly* observable, we need to use theories and models to *indirectly* obtain knowledge of them (and to be able to *recontextualize* and *redescribe* observables to discover new and (perhaps) hitherto unknown dimensions of the world around us). Deduction and induction do not give us access to these kinds of entities. They are things that to a large extent have to be *discovered*. Discovery processes presupposes creativity and imagination, virtues that are not very prominent in inductive analysis (statistics and econometrics) or deductive-logical reasoning. We need another mode of inference. We need inference to the best explanation.

Inference to the best explanation is a (non-demonstrative) ampliative method of reasoning that makes it possible for us to gain new insights and come up with - and evaluate - theories and hypotheses that - in contradistinction to the entailments that deduction provide us with transcend the epistemological content of the evidence that brought about them. And instead of only delivering inductive generalizations from the evidence at hand - as the inductive scheme - it typically opens up for conceptual novelties and retroduction, where we from analysis of empirical data and observation reconstruct the ontological conditions for their being what they are. As scientists we do not only want to be able to deal with observables. We try to make the world more intelligible by finding ways to understand the fundamental processes and structures that rule the world we live in. Science should help us penetrate to these processes and structures behind facts and events we observe. We should look out for causal relations, processes and structures, but models - mathematical, econometric, or what have you - can never be more than a starting point in that endeavour. There is always the possibility that there are other (non-quantifiable) variables - of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible - that were not considered for the formalized mathematical model. The content-enhancing aspect of inference to the best explanation gives us the possibility of acquiring new and warranted knowledge and understanding of things beyond empirical sense data. Arguably, realism in its different guises ultimately rests on inference to the best explanation to found the existence of such unobservable entities.

Outside mathematics and logic, scientific methods do not deliver absolute certainty or prove things. However, many economists are still in pursuit of absolute certainty. But there will always be a great number of theories and models that are compatible / consistent with facts, and no logic makes it possible to select one as the right one. The search for absolute certainty can never be anything else but disappointing since all scientific knowledge is more

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or less uncertain. That is a fact of the way the world is, and we just have to learn to live with that inescapable limitation of scientific knowledge.

"Traditionally, philosophers have focused mostly on the logical template of inference. The paradigm-case has been deductive inference, which is topicneutral and context-insensitive. The study of deductive rules has engendered the search for the Holy Grail: syntactic and topic-neutral accounts of all prima facie reasonable inferential rules. The search has hoped to find rules that are transparent and algorithmic, and whose following will just be a matter of grasping their logical form. Part of the search for the Holy Grail has been to show that the so-called scientific method can be formalised in a topic-neutral way. We are all familiar with Carnap's inductive logic, or Popper's deductivism or the Bayesian account of scientific method.

There is no Holy Grail to be found. There are many reasons for this pessimistic conclusion. First, it is questionable that deductive rules are rules of inference. Second, deductive logic is about updating one's belief corpus in a consistent manner and not about what one has reasons to believe simpliciter. Third, as Duhem was the first to note, the so-called scientific method is far from algorithmic and logically transparent. Fourth, all attempts to advance coherent and counterexample-free abstract accounts of scientific method have failed. All competing accounts seem to capture some facets of scientific method, but none can tell the full story. Fifth, though the new Dogma, Bayesianism, aims to offer a logical template (Bayes's theorem plus conditionalisation on the evidence) that captures the essential features of non-deductive inference, it is betrayed by its topic-neutrality. It supplements deductive coherence with the logical demand for probabilistic coherence among one's degrees of belief. But this extended sense of coherence is (almost) silent on what an agent must infer or believe" (Psillos (2007:441)).

Explanations are *per se* not deductive proofs. And deductive proofs often do not explain at all, since validly deducing X from Y does not *per se* explain *why* X is a fact, because it does not say anything at all about *how* being Y is connected to being X. Explanations do not necessarily have to *entail* the things they explain. But they can nevertheless confer warrants for the conclusions we reach using inference to the best explanation. The evidential force of inference to the best explanation is consistent with having less than certain belief.

Explanation is prior to inference. Inferring means that you come to believe something and have (evidential) reasons for believing so. As economists we entertain different hypotheses on inflation, unemployment, growth, wealth inequality, and so on. From the available evidence and our context-dependent background knowledge we evaluate how well the different hypotheses would explain these evidence and which of them qualifies for being the best accepted hypothesis. Given the information available, we base our inferences on explanatory considerations (noting this, of course, does not exclude that there exist other, non-explanatory, factors that may influence our choices and rankings of explanations and hypotheses).

#### Where did economics go wrong?

If only mainstream economists also understood these basics. But most of them do not. Why? Because in mainstream economics it is not inference to the best explanation that rules the methodological-inferential roost, but deductive reasoning based on logical inference from a set of axioms. Although – under specific and restrictive assumptions – deductive methods may be usable tools, insisting that economic theories and models ultimately have to be built on a deductive-axiomatic foundation to count as being economic theories and models, will only make economics irrelevant for solving real world economic problems. Modern deductive-axiomatic mainstream economics is sure very rigorous – but if it's rigorously wrong, who cares?

Instead of making formal logical argumentation based on deductive-axiomatic models the message, we are better served by economists who more than anything else try to contribute to solving real problems – and in that endeavour inference to the best explanation is much more relevant than formal logic.

"The weaknesses of social-scientific normativism are obvious. The basic assumptions refer to idealized action under pure maxims; no empirically substantive law-like hypotheses can be derived from them. Either it is a question of analytic statements recast in deductive form or the conditions under which the hypotheses derived could be definitively falsified are excluded under ceteris paribus stipulations. Despite their reference to reality, the laws stated by pure economics have little, if any, information content. To the extent that theories of rational choice lay claim to empirical-analytic knowledge, they are open to the charge of Platonism (Modellplatonismus). Hans Albert has summarized these arguments: The central point is the confusion of logical presuppositions with empirical conditions. The maxims of action introduced are treated not as verifiable hypotheses but as assumptions about actions by economic subjects that are in principle possible. The theorist limits himself to formal deductions of implications in the unfounded expectation that he will nevertheless arrive at propositions with empirical content. Albert's critique is directed primarily against tautological procedures and the immunizing role of qualifying or 'alibi' formulas. This critique of normative-analytic methods argues that general theories of rational action are achieved at too great a cost when they sacrifice empirically verifiable and descriptively meaningful information" (Habermas (1988:48)).

Science is made possible by the fact that there are structures that are durable and are independent of our knowledge or beliefs about them. There exists a reality beyond our theories and concepts of it. It is this independent reality that our theories in some way deal with. Contrary to positivism, the main task of science is arguably not to detect event-regularities between observed facts, but rather, to identify the underlying structure and forces that produce the observed events.

From that point of view, it could be argued that the generalizations we look for (often with statistical and econometric methods) when using inductive methods (to say anything about a population based on a given sample) are abductions. From the premise "all *observed* real-world markets are non-perfect" we conclude "all real-world markets are non-perfect". If we

have tested all the other potential hypotheses and found that, e.g., there is no reason to believe that the sampling process has been biased and that we are dealing with a non-representative non-random sample, we could, given relevant background beliefs / assumptions, say that we have justified belief in treating our conclusion as warranted. Being able to eliminate / refute contesting / contrastive hypotheses – using both observational *and* non-observational evidence – confers an increased certainty in the hypothesis believed to be "the loveliest".

Instead of building models based on logic-axiomatic, topic-neutral, context-insensitive and non-ampliative deductive reasoning – as in mainstream economic theory – it would be more fruitful and relevant to apply inference to the best explanation, given that what we are looking for is to be able to explain what's going on in the world we live in. The world in which we live is – as argued by e.g. Keynes and Shackle – genuinely uncertain. By using abductive inferences we can nonetheless gain knowledge about it. Although inevitably defeasible, abduction is also our only source of scientific discovery.

Most mainstream economic models build on a theory that is abstract, unrealistic and presenting mostly non-testable hypotheses. One important rational behind this kind of model building is the quest for rigour, and more precisely, *logical* rigour. Formalization of economics has been going on for more than a century and with time it has become obvious that the preferred kind of formalization is the one that rigorously follows the rules of formal logic. As in mathematics, this has gone hand in hand with a growing emphasis on axiomatics. Instead of basically trying to establish a connection between empirical data and assumptions, "truth" has come to be reduced to, a question of fulfilling internal consistency demands between conclusion and premises, instead of showing a "congruence" between model assumptions and reality. This has, of course, severely restricted the applicability of economic theory and models.

Unpacking premises and relationships within a consistent model is not enough in empirical sciences. In empirical sciences we do also have to be concerned with the truth-status of the premises and conclusions *re* the world in which we live.

In their search for the Holy Grail of *deductivism* – an idea originating in physics and maintaining the feasibility and relevance of describing an entire science as (more or less) a self-contained axiomatic-deductive system – mainstream economists are forced to make assumptions with often preciously little resemblance to reality. When applying this deductivist thinking to economics, mainstream economists usually set up "as if" models based on a set of tight axiomatic assumptions from which consistent and precise inferences are made. The beauty of this procedure is of course that if the axiomatic premises are true, the conclusions *necessarily* follow. The snag is that if the models are to be relevant, we also have to argue that their precision and rigour still holds when they are applied to real-world situations. They (almost) never do. In the positivist (Hempelian, deductive-nomological) tradition, explanation is basically seen as deduction from general laws. In social sciences these laws are non-existent, and so, *a fortiori*, are the deductivist explanations. When addressing real economies, the idealizations necessary for the deductivist machinery to work simply don't hold.

"The thrust of this realist rhetoric is the same both at the scientific and at the meta-scientific levels. It is that explanatory virtues need not be evidential virtues. It is that you should feel cheated by 'The world is as if T were true', in

the same way as you should feel cheated by 'The stars move as if they were fixed on a rotating sphere'. Realists do feel cheated in both cases" Musgrave (1999:68).

The one-eyed focus on validity and consistency makes much of mainstream economics irrelevant, since its insistence on deductive-axiomatic foundations does not earnestly consider the fact that its formal logical reasoning, inferences and arguments show an amazingly weak relationship to their everyday real world equivalents. Searching in vain for absolute and deductive knowledge and "truth", these economists forgo the opportunity of getting more relevant and better (defeasible) knowledge. For although the formal logic focus may deepen our insights into the notion of validity, the rigour and precision has a devastatingly important trade-off: the higher the level of rigour and precision, the smaller is the range of real world applications. Consistency does not take us very far. As scientists we can not only be concerned with the consistency of our universe of discourse. We also have to investigate how consistent our models and theories are with the universe in which we happen to live.

To understand and explain relations between different entities in the real economy the predominant strategy is to build models and make things happen in these "analogue-economy models" rather than engineering things happening in real economies. This formalistic-deductive modeling strategy certainly impresses some people, but the one-sided, almost religious, insistence on axiomatic-deductivist modeling as the only scientific activity worthy of pursuing in economics, forgets that in the realm of science it ought to be considered of little or no value to simply make claims about the model and lose sight of reality. Although the formalistic tractability of deductivist mathematical modeling method makes conclusions follow with certainty from given assumptions, that should be of little interest to scientists, since what happens with certainty in a model world is no warrant for the same to hold in real world economies.

"Mathematics, especially through the work of David Hilbert, became increasingly viewed as a discipline properly concerned with providing a pool of frameworks for possible realities...

This emergence of the axiomatic method removed at a stroke various hitherto insurmountable constraints facing those who would mathematise the discipline of economics. Researchers involved with mathematical projects in economics could, for the time being at least, postpone the day of interpreting their preferred axioms and assumptions. There was no longer any need to seek the blessing of mathematicians and physicists or of other economists who might insist that the relevance of metaphors and analogies be established at the outset. In particular it was no longer regarded as necessary, or even relevant, to economic model construction to consider the nature of social reality, at least for the time being...

The result was that in due course deductivism in economics, through morphing into mathematical deductivism on the back of developments within the discipline of mathematics, came to acquire a new lease of life, with practitioners (once more) potentially oblivious to any inconsistency between the ontological presuppositions of adopting a mathematical modelling emphasis and the nature of social reality. The consequent rise of mathematical deductivism has culminated in the situation we find today" Lawson (2015:84).

Theories and models being "coherent" or "consistent" with data do not make the theories and models success stories. To have valid evidence is not enough. What economics needs is *sound* evidence. The premises of a valid argument do not have to be true, but a sound argument, on the other hand, is not only valid, but builds on premises that are true. Aiming only for validity, without soundness, is setting the economics aspirations level too low for developing a realist and relevant science.

In science, nothing of substance has ever been decided by just putting things in the right logical form. Those scientific matters that can be dealt with in a purely formal-analytical matter are only of second-order interest. The absurdity of trying to analyse and explain (necessarily "non-Laplacian") real world systems equipped with analytical rather than substantial scientific arguments, becomes clear as soon as we become aware that this is fundamentally a denial of the *field-dependent* character of all science. What counts as a justified inference in economics is not necessarily equivalent to what counts in sociology, physics, or biology. They address different problems and questions, and -a fortiori – what is considered absolutely necessary in one field, may be considered totally irrelevant in another. In the case of substantial arguments there is, as Toulmin (2003:163) notes,

"...no question of data and backing taken together entailing the conclusion, or failing to entail it: just because the steps involved are substantial ones, it is no use either looking for entailments or being disappointed if we do not find them. Their absence does not spring from a lamentable weakness in the arguments, but from the nature of the problems with which they are designed to deal. When we have to set about assessing the real merits of any substantial argument, analytical criteria such as entailment are, accordingly, simply irrelevant ... 'Strictly speaking' means, to them, *analytically* speaking; although in the case of substantial arguments to appeal to analytic criteria is not so much strict as beside the point ... There is no justification for applying analytic criteria in all fields of argument indiscriminately, and doing so consistently will lead one (as Hume found) into a state of philosophical delirium."

### Bayesianism

Bayesian statistics has during the last couple of decades led a substantial school in the philosophy of science to identify Bayesian inference with inductive inference as such. However, there is really very little to warrant that belief.

Neoclassical economics nowadays usually assumes that agents that have to make choices under conditions of uncertainty behave according to Bayesian rules (preferably the ones axiomatized by Ramsey (1931), de Finetti (1937) or Savage (1954)) – that is, they maximize expected utility with respect to some subjective probability measure that is continually updated according to Bayes theorem. If not, they are supposed to be irrational, and ultimately – via some "Dutch book" or "money pump" argument – susceptible to being ruined by some clever "bookie".

Bayesianism reduces questions of rationality to questions of internal consistency (coherence) of beliefs, but - even granted this questionable reductionism - do rational agents really have to be Bayesian? Actually, there is no strong warrant for believing so.

The "problem of induction" is usually described as a problem of how we can learn things about a population from knowledge of a sample (spatial version) or how the past may give us information and help us to decide what to believe about the future (temporal version). In both cases Bayesians think they solve the problem through application of probabilistic calculus (especially with the help of Bayes Theorem).

This is however wrong, since from a Bayesian point of view *any* prior probability distribution is "as good as any other", which means that the probability calculus actually does not rule out anything. Anything goes. The sample does not tell us anything about the population. And the past does not – as argued by e.g. Max Albert (2009:55) – tell us anything about the future:

"Keeping to the Bayesian recipe, then, cannot, by and in itself, help us make better decisions. It just burdens us with a lot of calculations... From a Bayesian point of view, any beliefs, and consequently, any decisions are as rational or irrational as any other, no matter what our goals and experiences are. Bayesian rationality is just a probabilistic version of irrationalism... Any conclusions result from the choice of the prior probability distribution, but Bayesianism does not help us in choosing this distribution."

In many of the situations that are relevant to economics one could argue that there is simply not enough of adequate and relevant information to ground beliefs of a probabilistic kind, and that in those situations it is not really possible, in any relevant way, to represent an individual's beliefs in a single probability measure.

Say you have come to learn (based on own experience and tons of data) that the probability of you becoming unemployed in the US is 10%. Having moved to another country (where you have no own experience and no data) you have no information on unemployment and a fortiori nothing to help you construct any probability estimate on. A Bayesian would, however, argue that you would have to assign probabilities to the mutually exclusive alternative outcomes and that these have to add up to 1, if you are rational. That is, in this case – and based on symmetry – a rational individual would have to assign probability 10% to becoming unemployed and 90% of becoming employed.

That feels intuitively wrong though, and I guess most people would agree. Bayesianism cannot distinguish between symmetry-based probabilities from information and symmetry-based probabilities from an absence of information. In these kinds of situations most of us would rather say that it is simply irrational to be a Bayesian and better instead to admit that we "simply do not know" or that we feel ambiguous and undecided. Arbitrary an ungrounded probability claims are more irrational than being undecided in face of genuine uncertainty, so if there is not sufficient information to ground a probability distribution, it is better to acknowledge that simpliciter, rather than pretending to possess a certitude that we simply do not possess.

I think this critique of Bayesianism is in accordance with the views of Keynes, *A Treatise on Probability* (1921) and *General Theory* (1936). According to Keynes we live in a world permeated by unmeasurable uncertainty – not quantifiable stochastic risk – which often forces us to make decisions based on anything but rational expectations. Sometimes we "simply do not know". Keynes would not have accepted the view of Bayesian economists, according to whom expectations "tend to be distributed, for the same information set, about the prediction of the theory". Keynes, rather, thinks that we base our expectations on the confidence or "weight" we put on different events and alternatives. To Keynes expectations are a question of weighing probabilities by "degrees of belief", beliefs that have preciously little to do with the kind of stochastic probabilistic calculations made by the rational agents modeled by Bayesian economists.

There is also a kind of bias toward the superficial in Bayesian thought, which to Richard Miller (1987:325) is an example of:

"...real harm done in contemporary social science by a roughly Bayesian paradigm of statistical inference as the epitome of empirical argument. For instance the dominant attitude toward the sources of black-white differential in United States unemployment rates (routinely the rates are in a two to one ratio) is 'phenomenological.' The employment differences are traced to correlates in education, locale, occupational structure, and family background. The attitude toward further, underlying causes of those correlations is agnostic... Yet on reflection, common sense dictates that racist attitudes and institutional racism *must* play an important causal role. People do have beliefs that blacks are inferior in intelligence and morality, and they are surely influenced by these beliefs in hiring decisions... Thus, an overemphasis on Bayesian success in statistical inference discourages the elaboration of a type of account of racial disadvantages that almost certainly provides a large part of their explanation."

And as Henry E. Kyburg (1968:56) writes (emphasis added) in perhaps the ultimate takedown of Bayesian hubris:

"From the point of view of the 'logic of consistency' (which for Ramsey includes the probability calculus), no set of beliefs is more rational than any other, so long as they both satisfy the quantitative relationships expressed by the fundamental laws of probability...

Now this seems patently absurd. It is to suppose that even the most simple statistical inferences have no logical weight where my beliefs are concerned. It is perfectly compatible with these laws that I should have a degree of belief equal to 1/4 that this coin will land heads when next I toss it; and that I should then perform a long series of tosses (say, 1000), of which 3/4 should result in heads; and then that on the 1001st toss, my belief in heads should be unchanged at 1/4. It could increase to correspond to the relative frequency in the observed sample, or it could even, by the agency of some curious maturity-of-odds belief of mine, decrease to 1/8. *I think we would all, or almost all, agree that anyone who altered his beliefs in the last-mentioned way should be regarded as irrational.*"
The standard view in statistics – and the axiomatic probability theory underlying it – is to a large extent based on the rather simplistic idea that "more is better". But as Keynes argues in *A Treatise on Probability* – "more of the same" is not what is important when making inductive inferences. It's rather a question of "more but different".

Variation, not replication, is at the core of induction. Finding that p(x|y) = p(x|y & w) doesn't make w "irrelevant". Knowing that the probability is unchanged when w is present gives p(x|y & w) another evidential weight ("weight of argument"). Running 10 replicative experiments do not make you as "sure" of your inductions as when running 10,000 varied experiments – even if the probability values happen to be the same.

Keynes argued that it was inadmissible to project history on the future. Consequently we cannot presuppose that what has worked before, will continue to do so in the future. That statistical models can get hold of correlations between different "variables" is not enough. If they cannot get at the causal structure that generated the data, they are not really "identified".

"A major, and notorious, problem with this approach, at least in the domain of science, concerns how to ascribe objective prior probabilities to hypotheses. What seems to be necessary is that we list all the possible hypotheses in some domain and distribute probabilities among them, perhaps ascribing the same probability to each employing the principal of indifference. But where is such a list to come from? It might well be thought that the number of possible hypotheses in any domain is infinite, which would yield zero for the probability of each and the Bayesian game cannot get started. All theories have zero probability and Popper wins the day. How is some finite list of hypotheses enabling some objective distribution of nonzero prior probabilities to be arrived at? My own view is that this problem is insuperable, and I also get the impression from the current literature that most Bayesians are themselves coming around to this point of view" Alan Chalmers (2013:165).

# Econometrics and randomized experiments

Bayesianism has its root in statistics – and within economics, more specifically, in the statistical application of inductive reasoning in the form of econometrics.

Firmly stuck in an empiricist tradition, econometrics is only concerned with the measurable aspects of reality, But there is always the possibility that there are other variables – of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible – that were not considered for the model. Those who were can hence never be guaranteed to be more than potential causes, and not real causes.

When causal mechanisms operate in real world social systems they only do it in everchanging and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do it (as a rule) only because we engineered them for that purpose. Outside man-made "nomological machines" they are rare, or even nonexistant. Unfortunately that also makes most of the achievements of econometric forecasting rather useless. The increasing use of natural and quasi-natural experiments in economics during the last couple of decades has led some economists to triumphantly declare it as a major step on a recent path toward empirics, where instead of being a deductive philosophy, economics is now increasingly becoming an inductive science.

In defence of this view, the works of Joshua Angrist and Jörn-Steffen Pischke are often apostrophized, so let us start with one of their later books and see if there is any real reason to share the optimism on this 'empirical turn' in economics. In *Mastering Metrics,* Angrist and Pischke (2014:xiii) write:

"Our first line of attack on the causality problem is a randomized experiment, often called a randomized trial. In a randomized trial, researchers change the causal variables of interest... for a group selected using something like a coin toss. By changing circumstances randomly, we make it highly likely that the variable of interest is unrelated to the many other factors determining the outcomes we want to study. Random assignment isn't the same as holding everything else fixed, but it has the same effect. Random manipulation makes *other things equal* hold on average across the groups that did and did not experience manipulation. As we explain... 'on average' is usually good enough."

Angrist and Pischke may "dream of the trials we'd like to do" and consider "the notion of an ideal experiment" something that "disciplines our approach to econometric research", but to maintain that "on average" is "usually good enough" is an allegation that is rather unwarranted, and for many reasons.

"RCTs... fail to demonstrate any form of universal causality. They show us that by the use of the law of large numbers, we can describe the average characteristics of a large population and changes over time, by appropriately studying a small sample drawn from the population. RCTs do this extremely well, though even here one should add the reminder that average characteristics are not the only pertinent features of populations" Basu (2014:461).

It amounts to nothing but hand waving to *simpliciter* assume, without argumentation, that it is tenable to treat social agents and relations as homogeneous and interchangeable entities. When Joshua Angrist and Jörn-Steffen Pischke in an earlier article of theirs (Angrist & Pischke (2010:23)) say that "anyone who makes a living out of data analysis probably believes that heterogeneity is limited enough that the well-understood past can be informative about the future," I really think they underestimate the heterogeneity problem. It does not just turn up as an *external* validity problem when trying to "export" regression results to different times or different target populations. It is also often an *internal* problem to the millions of regression estimates that economists produce every year.

"Like us, you want evidence that a policy will work here, where you are. Randomized controlled trials (RCTs) do not tell you that. They do not even tell you that a policy works. What they tell you is that a policy worked there, where the trial was carried out, in that population. Our argument is that the changes in tense – from 'worked' to 'work' – are not just a matter of grammatical detail. To move from one to the other requires hard intellectual and practical effort. The fact that it worked there is indeed fact. But for that fact to be evidence that it will work here, it needs to be relevant to that conclusion. To make RCTs relevant you need a lot more information and of a very different kind" Cartwright & Hardie (2014:ix).

It is hard to share the enthusiasm and optimism on the value of (quasi)natural experiments and all the statistical-econometric machinery that comes with it. Guess we are still waiting for the export-warrant.

In econometrics one often gets the feeling that many of its practitioners think of it as a kind of automatic inferential machine that solves the problem of induction: input data and out comes casual knowledge. This is like pulling a rabbit from a hat. Great – but first you have to put the rabbit in the hat. And this is where assumptions come in to the picture.

As social scientists – and economists – we have to confront the all-important question of how to handle uncertainty and randomness. Should we equate randomness with probability? If we do, we have to accept that to speak of randomness we also have to presuppose the existence of nomological probability machines, since probabilities cannot be spoken of – and actually, to be strict, do not at all exist – without specifying such system-contexts.

In his book *Statistical Models and Causal Inference: A Dialogue with the Social Sciences* David Freedman (2010:14) touches on this fundamental problem, arising when you try to apply statistical models outside overly simple nomological machines like coin tossing and roulette wheels:

"Regression models are widely used by social scientists to make causal inferences; such models are now almost a routine way of demonstrating counterfactuals. *However, the 'demonstrations' generally turn out to depend on a series of untested, even unarticulated, technical assumptions.* Under the circumstances, reliance on model outputs may be quite unjustified. Making the ideas of validation somewhat more precise is a serious problem in the philosophy of science. That models should correspond to reality is, after all, a useful but not totally straightforward idea – with some history to it. Developing appropriate models is a serious problem in statistics; testing the connection to the phenomena is even more serious...

In our days, serious arguments have been made from data. Beautiful, delicate theorems have been proved, although the connection with data analysis often remains to be established. And *an enormous amount of fiction has been produced, masquerading as rigorous science.*"

Making outlandish statistical assumptions does not provide a solid ground for doing relevant social science.

A popular idea in quantitative social sciences is to think of a cause (C) as something that increases the probability of its effect or outcome (O). That is:

P(O|C) > P(O|-C)

However, as is also well-known, a correlation between two variables, say A and B, does not necessarily imply that that one is a cause of the other, or the other way around, since they may both be an effect of a common cause, C.

In statistics and econometrics we usually solve this "confounder" problem by "controlling for" C, i.e. by holding C fixed. This means that we actually look at different "populations" – those in which C occurs in every case, and those in which C doesn't occur at all. This means that knowing the value of A does not influence the probability of C [P(C|A) = P(C)]. So if there then still exist a correlation between A and B in either of these populations, there has to be some other cause operating. But if *all* other possible causes have been "controlled for" too, and there is still a correlation between A and B, we may safely conclude that A is a cause of B, since by "controlling for" all other possible causes, the correlation between the putative cause A and all the other possible causes (D, E, F, ...) is broken.

This is of course a very demanding prerequisite, since we may never actually be sure to have identified *all* putative causes (cf. Basu (2014:460)). Even in scientific experiments may the number of uncontrolled causes be innumerable. Since nothing less will do, we do all understand how hard it is to actually get from correlation to causality. This also means that *only* relying on statistics or econometrics is not enough to deduce causes from correlations.

"If the assumptions of a model are not derived from theory, and if predictions are not tested against reality, then deductions from the model must be quite shaky...

In my view, regression models are not a particularly good way of doing empirical work in the social sciences today, because the technique depends on knowledge that we do not have. Investigators who use the technique are not paying adequate attention to the connection – if any – between the models and the phenomena they are studying...

Causal inference from observational data presents may difficulties, especially when underlying mechanisms are poorly understood. There is a natural desire to substitute intellectual capital for labor, and an equally natural preference for system and rigor over methods that seem more haphazard. These are possible explanations for the current popularity of statistical models.

Indeed, far-reaching claims have been made for the superiority of a quantitative template that depends on modeling – by those who manage to ignore the far-reaching assumptions behind the models. However, the assumptions often turn out to be unsupported by the data. If so, the rigor of advanced quantitative methods is a matter of appearance rather than substance" David Freedman (2010:56).

### Conclusion

Abduction and inference to the best explanation show the inherent limits of formal logical reasoning in science. No new ideas or hypotheses in science originate by deduction or induction. In order to come up with new ideas or hypotheses and explain what happens in our world, scientists *have to* use inference to the best explanation. All scientific explanations inescapably relies on a reasoning that is, from a *logical* point of view, fallacious. Thus – in order to explain what happens in our world, we have to use a reasoning that *logically* is a fallacy. There is no way around this – unless you want to follow the barren way that mainstream economics has been following for more than half a century now – retreating into the world of thought experimental "as if" axiomatic-deductive-mathematical models.

The purported strength of modern mainstream economics is that it ultimately has a firm anchorage in "rigorous" and "precise" deductive reasoning in mathematical models. To some of us, however, this "strength" has come at too high a price. Perhaps more than anywhere else can this be seen in macroeconomics, where an almost quasi-religious insistence that economics has to have microfoundations – without ever presenting neither ontological nor epistemological justifications for this patently invalid claim – has put a blind eye to the weakness of the whole enterprise of trying to depict a complex economy based on an all-embracing representative actor equipped with superhuman knowledge, forecasting abilities and forward-looking rational expectations. How can we be sure the lessons learned in these models have external validity, when based on a set of highly specific assumptions with an enormous descriptive deficit? To have a deductive warrant for things happening in a closed model is no guarantee for them being preserved when applied to the real world.

The urge to view all inferences as more or less deductive and equating good arguments with logical entailment of the "All Xs are Ys" kind, has led mainstream economics down the wrong path. The more mainstream economists insist on formal logic validity, the less they have to say about the real world. And real progress in economics, as in all sciences, presupposes real world involvement, not only self-referential deductive reasoning within formal-analytical mathematical models.

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# Heterodox economics or political economy?

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Seven years after the onset of the global financial crisis, when it was widely said that orthodox economic thinking had failed and should change, it is evident that that any such changes have been far from decisive. Some would say it looks pretty much like business as usual. This is not just the case in the academy, where the core courses in university economics departments continue to emphasise basic training (some would say indoctrination) in neoclassical economic principles. It is also evident in the realm of economic policy which continues to be dominated by a neoliberal agenda, augmented by the post-crash politics of austerity. This resilience of mainstream economics makes it essential for dissenters to continually reconsider how best to challenge orthodoxy both in theory and practice.

Active participants in the RWER recognise the inadequacies of mainstream economics and the need for progressive alternatives in economics education, public debate and policy formulation. But how is progress to be achieved? A major lesson from the experience of struggles to establish alternative teaching and research programs is that doing so within territory occupied by orthodox economists is extraordinarily difficult. Intervening in broader public discourse is also a big challenge, given the entrenched interests and institutions that continue to shape its direction. Profound questions need to be considered for future strategy. In this context, labels matter, as they do in marketing. They construct imagery and signal strategic choices. The two particular labels with which this article is concerned are "heterodox economics" and "political economy". Is the ongoing challenge to mainstream economics better mounted under the former or latter rubric? Are the two terms synonymous? Is it just a matter of terminology? Or does the question touch on a bigger strategic choice - between seeking space for research and teaching of non-neoclassical economics or seeking to promote political economy as part of a broader interdisciplinary social and political project? Dissident groups commonly confront the questions of focus and self-identification and much may hinge on it. While the terminology itself is not crucial, it has implications for strategy.

In making the case for political economy, this article briefly explores five interrelated considerations, relating to issues of pluralism, politics, professional status, academic partnerships and public recognition.

# Identifying as pluralist

The need for a pluralist perspective, both in economics education and in economic policy formulation, has become a widely accepted view among critics of orthodoxy. Because the monist character of orthodoxy is the problem, pluralism is the solution. This case has been extensively elaborated in the last two decades, including in my own writing. As John King argues, "economics is unique among the social sciences in having a single monolithic mainstream, which is either unaware of or actively hostile to alternative approaches" (King 2013: 17). This has had deeply unfortunate educational consequences, limiting the understanding of political economic processes and events. It marginalizes consideration of

economic history and the history of economic thought, producing an ahistorical approach that creates a sort of professional amnesia. Worse still, social harm results, such as the dysfunctional policies enacted in the name of "sound economics" since the onset of the global financial crisis. Concurrently, the official university research evaluation processes marginalise non-mainstream approaches, thereby operating to compound the above problems. This is a process of circular and cumulative causation, whereby monism in the mainstream excludes the very elements that could lead to better explanations of real-world economic phenomena. It is a vicious cycle. Only the widespread embrace of pluralism can correct it.

Such observations may be taken as supporting the case for challenging mainstream economics under the banner of "heterodox economics". On this reasoning, self-identifying as heterodox economists signals our concern that non-mainstream views should get a better hearing as alternative analyses of how the world actually works. In practice, it means confronting the dominant mainstream neoclassical economics with critical alternatives, including various strands of post-Keynesian economics, Marxist economics, institutional economics and analyses from feminist, ecological and various other perspectives. Indeed, this is what heterodox economists do. Moreover, a "heterodox economics" label fits well with claims about pursuing academic practices that are conducive to an "open society" in which freedom of expression exists and innovation and creativity flourish. It is also an antidote to right wing politicians' claims that "there is no alternative".

However, more careful consideration reveals that conflating the case for pluralism with the advocacy of heterodox economics raises three tricky problems.

First, heterodoxy and pluralism are not synonymous. Pluralism is a methodological position that embraces diversity as a matter of principle: it would normally include both the mainstream and a broad range of other viewpoints. Heterodoxy, on the other hand, usually entails opposition to orthodoxy: individual heterodox economists may also be hostile to approaches other than their preferred alternative. In struggles for progress in economics, heterodoxy and pluralism may seem to be close relatives, but it is pertinent to identify this conceptual difference. Pluralism is an "in principle" position, based on ontological, epistemological and ethical propositions (as discussed by Mariyani-Squire and Moussa 2013), whereas the movement for heterodox economics is, more pragmatically, about creating space for preferred alternatives to the orthodoxy.

Second, there is the question of which alternatives should get that hearing. What is to be included in "heterodox economics"? There seems to be reasonable consensus about the presence of contributions from Marxism, post-Keynesianism and institutional economics. Embracing ecological economics and feminist economics is also widely advocated, although neoclassical economists also assert their presence in these territories. But what about Austrian economics, with its right-wing libertarian "free market" implications: should it also be included? And what of contributions in the distinctive traditions of Karl Polanyi or Henry George: shouldn't they also be in the mix? A "heterodox economics" that simply accommodates all these elements may be quite eclectic, potentially dissipating or even blunting the challenge to the neoclassical paradigm. Some stronger conception of 'unity in diversity' may be necessary for progress.

The third difficulty is that some economists claim that the mainstream has itself already become heterodox. Diana Coyle (2007) and David Colander (2009) are among those who

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have propounded this view. They posit that the embrace of game theory, new growth theory, behavioural and experimental economics, complexity economics and other theoretical innovations have reduced the dominance of neoclassicism (Thornton 2015). Personally, I find the argument unconvincing for at least three reasons. First, while these analytical developments give the appearance of some diversity within the mainstream, the underlying assumptions of methodological individualism and systemic stability through market forces remain pervasive. Indeed, invoking the novelty of pluralism within neoclassical economics as a response to the deficiencies of the discipline ignores the diversity that has characterised the discipline since its inception, without this ever entailing a paradigm shift in, or a radical break from, these underlying presumptions. Second, these elements of product differentiation have not generally displaced neoclassical economics from the core curriculum that is taught to students, although they often form the basis for electives in later years of study. Because what is taught in the foundation years determines the basis on which the discipline and profession is defined and reproduced, the changes are neither fundamental nor transformative. Third, if an academic profession is to be judged ultimately by its external influences and effects, it is evident that it is still largely "business as usual" for an orthodox economics that functions as capitalist ideology. The close association between neoclassicism in the academy and neoliberalism in the realm of public policy remains much in evidence. Even Diane Coyle, notable for her previous emphasis on the flexibility of the economics discipline, has conceded that the catastrophic impact of the global financial crisis of 2007-08 has had little impact on how the academic orthodoxy in economics is constructed and reproduced (Coyle 2013). Nevertheless, the assertion of an "internal heterodoxy" within mainstream economics unfortunately muddles the waters. Maybe that is its purpose.

These considerations illustrate the importance of pluralism in challenging the orthodoxy, but also indicate some of the associated tensions. Seeking more unity, analytically and organizationally, would be helpful in a conflict situation like this. This entails working together to extend and broaden the critique of mainstream economics and challenging its influences, both on students and in the wider worlds of economic discourse and public policy. Concurrently, seeking common elements, even partial syntheses, of the principal progressive, non-neoclassical currents of analysis is important in developing effective and influential alternatives. The following considerations indicate why the embrace of a broader "political economy" can contribute to this more assertive strategy for challenging the mainstream.

# Being explicitly political

Self-identifying as a movement for "political economy" points to the inherently political character of the undertaking, explicitly challenging mainstream economists' claim to be pursuing "value-free" lines of inquiry. Talking of political economy necessarily raises questions about "what politics?" and "whose values?" It also signals the quest for finding commonalities among the currents of thought that are critical of the mainstream. This shared ground may be *ontological*, as Tony Lawson has recurrently emphasised in making the case for a critical realist approach (Lawson 2006, Morgan 2016). Thus, the unity may come from a "world view" of the capitalist economy structured by power relations and prone to inequality and crises. The common ground may also be *methodological*, emphasising a shared commitment to analysis of the historically-contingent character of economic phenomena rather than "equilibrium" conditions. The common ground may also be explicitly *political*, emphasising the

mission of putting democratic politics in command *vis-a-vis* market forces, as posited by Higgins and Dow (2105). It may be, and probably should be, all three.

Conventional claims and protestations to the contrary, mainstream economics is inherently value-laden and political. So too is challenging and changing it. Signalling this in the labelling of the challenge is a healthy antidote to spurious claims about economics being a value-free science. As Gunnar Myrdal (1953) strongly emphasised, the development of economic thought has always and everywhere had a political character. In the real world economics and politics are intertwined. Therefore, successfully challenging the dominant orthodoxy cannot be just a matter of developing "better" economic models (claiming higher values for explanatory capacity). It also has to be a political process.

### Asserting professional status

Another advantage of the term "political economy" is its long and respectable lineage. Indeed, political economy has a strong claim to actually **be** the mainstream, running from the eighteenth through to the twenty-first century and including seminal contributions from Smith, Ricardo, Marx, Veblen, Keynes, Kalecki, Robinson, Myrdal, Galbraith, Heilbroner and modern contributors to that broad tradition of social inquiry. From this perspective, neoclassical economics may be regarded as an initially interesting side-track that became a *cul-de-sac* (albeit one with a massive volume of traffic).

This claim about the status and centrality of political economy is contested by modern mainstream economists, of course. If the term political economy is mentioned at all, it is usually treated as being of purely historical interest, relating to the characteristics of economic inquiry in a pre-scientific era. Or it may be taken as referring only to the analysis and prescription of public policy. That use of the term by practitioners associated with the Virginia School, the Chicago School and contemporary "public choice" theory is a common source of confusion in the USA, in particular. In historical usage, political economy has also been the label given to concerns with policies that may modify or regulate the economy's functioning in pursuit of specified social objectives, contrasting with economics as the study of how the economy works. These are terminological difficulties that unfortunately muddy the waters, once again. However, trying to avoid these issues by retreating to "heterodox economics" seems to accept marginal disciplinary status as all that is ever likely to be on offer. This is indicated by the usual inclusion (at best!) of "heterodox economics" in a residual "other economics" category in official research publication classifications. Challenging this marginalisation, and defining a positive agenda for alternative economic analysis rather than merely reacting to the mainstream, is a substantial long-term concern that requires redefining the relevant territory - in effect, challenging the nature and limits of the discipline.

# **Building interdisciplinary partnerships**

Even if the economics profession continues to deflect the challenges posed by heterodox economists, substantial progress can be made in relation to cognate social sciences. This is a necessary element in a strategy for progress because mainstream economists working in universities usually resist attempts to reconstitute their discipline on genuinely pluralist principles. Marxist political economy, for example, can usually only get a hearing as an historically discredited view; while "old" institutionalism, if mentioned at all, is merely a

precursor to "new institutional economics", which is more compatible with a neoclassical approach. Heterodox economists may get jobs in economics departments: some do, especially if their "deviance" develops after secure employment has been achieved, but they are often not replaced by people of similar inclination when they retire or move on.

Establishing more secure territory for teaching and research in political economy can be easier in other areas within the social sciences where there is concern to deal with the economic dimensions of social problems and public policy. In my experience, political economists are usually welcomed into the latter territories (if they eschew the imperialist ambitions that have been evident when neoclassical economists seek to invade other territories in the social sciences). Thus, it is the mutual-learning relationship of political economy with subjects like sociology, geography, politics or history that is crucial. Interestingly, it is the commitment to interdisciplinary studies, rather than the commitment to pluralism in economics, that is more important in building these partnerships.

In other words, establishing a foothold for political economy, whether as a university department separate from economics (as in the University of Sydney's Department of Political Economy where I taught for over four decades) or in conjunction with other social science disciplines, is a precondition for heterodox economics having a sustainable place in university education and research. Having established any such foothold, good teaching can show that political economy has the potential to provide a richer learning experience than straight mainstream economics (O'Donnell 2014). Similarly, research and policy advocacy can usually get a good hearing where political economists contribute to interdisciplinary studies on matters of public significance and concern.

# Achieving public recognition

The challenge to orthodox economics will always be marginal if it remains a purely academic concern. Beyond the universities are important avenues for public influence. Education within the labour movement is one such focus (Stanford 2015). Political economic writing for popular magazines and websites is also important, as is participation in the media and working with political parties and progressive NGOs. Preparing submissions to government inquiries – and concurrently using that material in academic and/or journalistic contributions – are activities that fit well into this strategy. Being in tune with the concerns of, and having an identity that is recognized by, a broader public is crucial for challenging the influence of mainstream economics.

In this last respect, the term "political economy" clearly has the edge over "heterodox economics". Political economy signals a broad conception of how we understand the economy in a historical, social and political context. It has substantially greater potential for public recognition, being a label that is reasonably intelligible to non-specialists. It contrasts, in this respect, with the typically bemused response of members of the general public when hearing the term "heterodox economics" – "huh, what's that?".

# Conclusion

Challenging mainstream economics does not produce a simple or straightforward clash, like that between two sporting teams seeking to score more runs or goals to determine the winner. The rules of the contest are not agreed beforehand and there is no agreed venue, nor impartial umpire. Even the criteria by which success is judged are contestable.

For dissident economists to label themselves as heterodox is academically respectable but may be strategically weak. On the positive side, the big plus of heterodoxy is its apparent association with pluralism, indicating aversion to dogma and openness to alternative ways of seeing. However, there is a tension between pluralism and heterodox economics, as I have argued in this article. The strategic problem is that arguing for heterodox economics presumes the centrality of an orthodoxy: almost by definition, therefore, it is consigned to the outer. This tends to put advocates of heterodox economics "on the back foot", seeking shelter for an array of different views whatever their coherence or relationship to each other. Pushing for political economy to have a central place in economic discourse is a more assertive strategy. It is compatible with the embrace of pluralism, but more emphasis is placed on unity in diversity, the political elements that infuse the competing currents of economic thought, the potential for gaining strength through interdisciplinary partnerships and the need for broader public recognition and influence.

I do not present these views in the expectation that individuals and associations will abandon their current self-identification as "heterodox economists". That is not the principal point of this article. Rather, it is to locate heterodox economics as part of a broader political economy project that is concerned with understanding how economies function in relation to social processes and developing political practices that can contribute to more equitable and sustainable arrangements for the future. This is an inherently interdisciplinary and explicitly political project. It matters because it opposes a misleading and class-biased economic orthodoxy and because of its critique of "actually existing capitalism" and neoliberal policy practices in society at large. This political economy project cannot properly be an exclusively academic concern, partly because of the internal constraints within academia but, more positively, because there is considerable thirst for it in the broader society. Indeed, whether under the rubric of heterodox economics or political economy, there is much work to be done.

*Note*: An earlier version of this article appeared in the *World Economics Association Newsletter*. The article was originally developed from a paper presented by the author to the conference of the Society for Heterodox Economics (SHE) held in December 2015 in Sydney, Australia, and the editorial written for the special issue of *The Journal of Australian Political Economy* on the theme of "heterodox economics". I thank participants at the conference and colleagues who provided constructive feedback on an earlier draft of this article.

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# History as a source of economic policy

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In a debate about whether economics is a science, "Nobel Prize" winner in economics Robert J. Shiller observes that economics differs from fields in natural science in one important respect:

"Economics is necessarily focused on policy, rather than discovery of fundamentals. Nobody really cares much about economic data except as a guide to policy: economic phenomena do not have the same intrinsic fascination for us as the internal resonances of the atom or the functioning of the vesicles and other organelles of a living cell. We judge economics by what it can produce. As such, economics is rather more like engineering than physics, more practical than spiritual."

To this difference in purpose, Alan Y. Wang, joining the debate at Harvard, added that economics also differs from fields in natural science as a subject of inquiry:

"Merriam-Webster's definition of science is 'a study of the natural world based on facts learned through experiments and observation.' What physics and chemistry and molecular biology have in common is that the building blocks of what they observe and experiment with don't change. Such is the natural world. But what is the building block of economics? People. Economics does not study any unit smaller than a collection of people. And human behavior can never be absolutely predicted or explained—not if we wish to believe in free will, at any rate." ("No, Economics Is Not a Science.")

Shiller commented in the same vein:

"[M]odels describe people rather than magnetic resonances or fundamental particles. People can just change their minds and behave completely differently. They even have neuroses and identity problems, complex phenomena that the field of behavioral economics is finding relevant to understanding economic outcomes."

And he could have added that these behaviors are often projected irrationally as well as rationally in different places and times in their specificities into the present from the past.

Historians agree with the subject and aim of economic inquiry as Shiller and Wang state them, because they discuss people and policy in their work. Orthodox economists have excluded historians from the study of economics, because their work is not "scientific." Here is how Egmont Kakarot-Handtke phrased the objection:

"Heterodox history should not be confounded with heterodox economics. Heterodox economics can tell historians ... that they have no idea of the fundamental Law of Profit as they have no idea of the Laws of energy transformation. Economics is neither psychology, nor sociology, nor history. Economics is the science which studies how the monetary economy works. Science looks for what remains unchanged in time, i.e. 'eternal' laws, history looks at what changes over time. 'That is why Descartes said that history was not a science – because there were no general laws which could be applied to history."

Most non-historians have a simplistic view of how historians do history: History is something that exists "out there" to be observed and explained by historians based on data, printed matter, and archival sources, the debris of history that historians dredge up in their research. History, however, no less than natural science or economics, does not exist "out there" but in the mind of the historian, as individual and guild. (Capra, 1982)

Wilhelm Dilthey, who wrote extensively on the subject, also stressed that unlike natural science, which looks for law-based explanations, the task of the humanities is to "understand" human and historical life:

"The way the historical world is represented and explained must in some way reflect the way history has been lived and understood. Understanding (*Verstehen*) for Dilthey is a process that employs all our capacities (thinking, feeling, willing of life's experiences) and is to be distinguished from pure intellectual understanding (*Verstand*)" (Dilthey, Stanford Encyclopedia of Philosophy).

Accordingly, historians comprehend an economic-influencing occurrence differently from orthodox economists who observe and explain (*Verstand*). Just as a person understands what is meant when he/she hears a person is in passionate love, not because he/she observes the symptoms following some natural law but because he/she has been in passionate love and has felt its force, an historian understands the power of self-preservation of an interest group (like neo-classical economists) because he has experienced the hold that self-preservation has over people. The historian's understanding of people does not come through "objective concepts," but only from past views, interpretations, and a shared world, that "springs out of the depth of his own experience" (Dilthey, *Einleitung in die Geisteswissenschaften*, p. 1).

That understanding comes to a great extent out of an inherited culture which historians learn about from the sources and create in their debates. Historians have a heightened sense of the specious present – the duration that is perceived as present and as extended in time. They have more of the past in it because they perceive it as still active in the now.

The question is, then, do the results of the intellectual work Egmont Kakarot-Handtke claims economists do in their search for laws provide a clearer guide for policy making that Shiller says is the aim of economics than the understanding historians achieve in their research methodology and interpretation of the specious present?

# The scientific paradigm as an instrument of policy

Stanley Jevons, inventor with Karl Menger of marginal-utility analysis, described the first step that neoclassical economists had to make in order to turn their subject into a scientific object:

"...all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such principles – to the tracing out of the mechanics of self-interest and utility, that [economics] is devoted" (Jevons quoted in Fullbrook, 2006, p. 2).

Fullbrook describes how the architects of the new discipline mapped it isomorphically with Newtonian mechanics in order to enhance its scientific stature:

"In Neoclassical economics, 'bodies' translates 'individuals' or agents,' 'motions' translates 'exchange of goods,' 'forces' translates 'desires' or 'preferences,' which when summed become 'supply and demand, 'mechanical equilibrium' becomes 'market equilibrium,' this being when the difference between supply and demand is zero, and 'physical systems' translates 'markets.' ...All exchanges were said to magically take place at the prices that equated demand and supply" (Fullbrook, 2006, p. 2).

No viable science means much to the neoclassical economist unless it can be expressed mathematically. That was the glory of Newtonian mechanics. In the second half of the nineteenth century Léon Walras, preceded by some French economic-engineers (Locke, 1989, pp. 124-26), mathematized neo-classical economics. With this achievement he stated in 1884 in his *Elements of Pure Economics that* economics has become a "science, which resembles the physico-mathematical sciences in every respect" (Quoted in Fullbrook, 2006, p. 3).

A claim, however, is not a reality. Seventy years after Walras' achievement, John von Neumann and Oskar Morgenstern questioned the prescriptive power of mathematical economics. In the forward to their remarkable book, *Theory of Games and Economic Behavior (1944)*, they wrote: "The concepts of economics are fuzzy but even in those parts of economics where the descriptive problem has been handled more satisfactorily, mathematical tools have seldom been used appropriately. Mathematical economics has not achieved very much."

During WWII and the Cold War, scientists and engineers working on operations research problems, developed analytical tools that people thought finally would turn management and economics into prescriptive management sciences. The work of George B. Dantzig and his associates at the Rand Corporation on the linear programming algorithm used in decision making drew the most attention. The procedure utilized modern mathematics (vector algebra, matrix theory, symbolic logic) and statistical techniques in an effort to take the guesswork out of management. The US Air Force, for which Rand worked on contract, used it logistically in the Berlin Airlift and to maximize the effectiveness of bombing patterns in the Korean War. The operations research methods were then proselytized among US NATO allies in seminars and symposiums held in America or abroad and by management consultancies in the broader business and corporate world.

Prescient neoclassical economists latched on to the new methods; while at Rand in 1948, the economist Kenneth Arrow used Dantzig's toolkit in his work on Rational Choice Theory. His book *Social Choice and Individual Value* (1951) was the "first real classic" on what, Robert N. Bellah remarked in 2000, "is now taken as a given in economics and has spread out into many neighboring disciplines." (Bellah, 2000, p. 7; Amadae, 2003) The neoclassical economists Joseph Dorfman, Paul Samuelson, and Robert Solow applied linear programming to their subject in *Programming and Economic Analysis* (1958).

This enthusiasm for scientific decision making prompted the founding of and permeated the thinking in postwar operation research groups, and by extension into economics. The British *Operational Research Quarterly* printed their scientific credo on its masthead:

"Operational Research is the application of the methods of science to complex problems arising in the direction and management of large systems of men, machines, materials and money, in industry, business and defence. The distinctive approach is to develop a scientific model of the system, incorporating measurement of the factors such as choice and risk, with which to predict and compare the outcomes of alternate decisions strategies or controls. The purpose is to help management determine its policies and actions scientifically" (see any copy of the journal).

The simplex linear programming decision-making algorithm that influenced thinking in neoclassical economics is an example of this scientific model building. The procedure, which provided for comparisons of sets of ratios existing among sets of consumption of various inputs and rates of production of various outputs, utilized modern mathematical and statistical techniques. The algorithm was based on the following assumptions:

- 1. Objectives can be stated mathematically
- 2. Resources can be stated mathematically
- 3. Alternative courses of action are too numerous for discussion by older methods
- 4. Variables had to be related linearly, i.e., when two variables are linearly related this means that a change in one causes exactly proportionate change in the other.

Up to 1968 "optimism about the future of OR" reigned in the pages of this quarterly; there was "almost a total lack of criticism and debate" (Dando and Sharp, pp. 93-94). By 1973 the papers reflected considerable doubt about the practical effectiveness of OR, a doubt which by 1978 was being voiced in about a quarter of the major papers appearing in the journal. Essays in the late 1970s, therefore, expressed a decade of ever-increasing doubt at the center of the OR paradigm about the effectiveness of decision science models.

Models of macroeconomic prognostication were also scrutinized critically. Such models were relatively successful as long as the future resembled the past, but when the future did not resemble the past, spectacular failure to predict rate of economic growth, business profitability, inflation rates, private consumption levels, employment, etc. ensued. The conclusions Frederichs and Kübler reached about the reliability of German econometric models seemed to apply to the entire macroeconomic exercise in model building: "Neither the econometric, nor the naïve prognosis, nor the judgmental forecasts could satisfactorily predict future economic development" (p. 814). It was difficult to formulate useful economic policy recommendations with mathematical models. A well-known economist Kenneth Boulding, called the whole mathematical enterprise a mistake: "Perhaps the real villain," he wrote, "is

the discovery of seventeenth century mathematics two hundred years later by Cournot, Jevons, and most of all Walras, whose influence and brilliance set economics on a path that increasingly has become a dead end" (Boulding, p. 5).

Clearly there are fatal deficiencies in modeling; in the case of the linear programming decision model the requirement that objectives and resources be stated mathematically is one of them. At the individual manager's decision making level, the operations research scientists like to think that the manager's job is to plan, organize, co-ordinate and control, in order to optimize. These are rational, analytical acts, which seem well suited for scientific operations research techniques. But as Ian Glover and Michael Fores (p. 121), noted the manager's job is actually

"...of an unprogrammed character; he... is not much concerned with the flow and use of 'hard' information, his information is distorted, incomplete, his job is ambiguous. He is not primarily a decision-maker, a planner, but an 'inspirer,' a fire fighter, and a rationalizer after the fact." As Glover phrased it, "attempts to 'study' decision making are overly academic; attempts at 'programming' it all seems to be like the search for fool's gold, confusing the academic ballgame of analysis with the executive task of synthesis" (Glover and Fores, p. 118).

Glover and Fores highlight variables that are important to success that cannot be stated mathematically. Charisma or leadership qualities in a manager are often among these variables. Clausewitz in his famous work, *On War* could and did measure the material resources of an army when estimating its strength, but in the fog of war he knew that successful outcomes depend much on the chief executive's genius. Napoleon was famous for his rhetoric; other great commanders, including Napoleon himself, had charisma. Nobody could predict the appearance of these irrational traits in commanders or define what precisely they are, or state them mathematically, but they are palpable and at times event-deciders (Locke, 2012).

Similar points can be made when distinguishing between managers and entrepreneurs. Friedrich Nietzsche's mythopoetic vision of Apollo and Dionysus can be used to sum up the difference. In his study of *Young Nietzsche* Carl Pletsch observed that for Nietzsche, the Apollonian "is the principle of clearly delineated images, permanence, optimism, individualization, and rationality. It is striving for clarity." This is the ethos of analytical formalism and mathematical methods. On the other hand, for Nietzsche the Dionysian expresses "the principle of flux, impermanence, suffering and pessimism...an irrational force, wild impulsive, and instinctive." This is the creative power of the great entrepreneur. Whereas the Apollonian vision is timeless and "responsible for the constant formulation and reformulation of the forms of knowledge and rationality that order our everyday life," the Dionysian urge, which is "momentary, exceptional, and counter-intuitive," is "dangerous to any structure of reality; it is the maelstrom of any impulse caught in the flux of time," unknowable and unpredictable (Pletsch, pp. 131-32, Locke and Schöne, pp. 2-5).

The economist Gunnar Eliasson, struggling to come to terms with entrepreneurialism and intrapreneurism within firms in the late 20<sup>th</sup> century, frustratingly commented about the inadequacies of the economist's scientific toolbox.

"Management teachers as well as economic theorist need a realistic model to support teaching and thinking. Since no realistic teaching of dynamic markets exists, no good theory of the firm has been created. The moral, hence, is that so far we have excellent firms, not thanks to but despite management teaching" (Eliasson, 1997, p.12).

Perhaps for historians the most convincing evidence of the failure of this new scientific paradigm is in apostasy. Two examples of personal turnabouts are briefly outlined here. One is the operations research pioneer Russell Ackoff. He developed operations research at the Case Institute of Technology, Cleveland, before moving to the Wharton School of Finance, where OR techniques could be applied to a broader range of subjects in finance and business than just industrial administration. He is also credited with introducing more rigorous scientific operation research methods into British academia. K. Brian Haley notes that Russell Ackoff's coming to the University of Birmingham, as Joseph Lucas Visiting Professor, in 1961 was a signal event. "His presence had a major impact on the whole of the UK educational scene, inspired a number of initiatives in the way the subject was viewed in industry, and was one of the prime movers in the establishment of the Institute for Operational Research" (Haley, 2002, p. 85). The University of Birmingham, which had invited Ackoff, had instituted a master's in OR in 1958; his presence seemed to stimulate the development of academic OR in the UK, with masters' in OR initiated at Imperial College London and at Cranfield in 1961 and a master's in the subject started at the University of Hull in 1962.

By the 1970s Ackoff began to have misgivings about operations research as a scientific endeavor. He especially attacked the use of mathematics in modeling. In a remarkable essay, "The Future of Operational Research is Past," in 1979 (JORS 30, pp. 93-104), he accused operational research scientists in rather crude phraseology of engaging in "mathematical masturbation without substantive knowledge of organizations, institutions, or their management" (p.97). "OR problems," he concluded, "can never be a perfect representation of a problem. They leave out the human dimension, the motivational one;" indeed, he affirmed that the successful treatment of managerial problems deserves "the application not only of science with a capital S but, also, all the arts and humanities we can command" (p.102).

The second apostate is H. Thomas Johnson. He taught management accounting at a respectable university in the 1970s, where he took an interest in the financial accounting systems introduced in the 1920s into US multi-national automobile firms (Johnson, 1978). Then in the 1980s Johnson embarked on a twenty year journey of apostasy during which he studied, among other things, W. Edwards Deming on quality management and conducted a decade-long inquiry into the Toyota Management System at its Georgetown, Kentucky plant (Locke, 1996, pp. 176-77). Johnson is associated with the group in US manufacturing that developed the idea of lean management – to which the name of Mike Rother can also be attached. They became sharp critics of the command and control management culture, what Johnson called "management by results," that were set up to run mass production factories in America, which by the 1980s were failing to meet the organizational challenge posed by Japanese manufacturing, what he called "management by means."

He portrayed US Big Three automakers management under 7 rubrics:

- 1. the individual is responsible,
- 2. control results
- 3. follow finance-driven rules
- 4. manipulate output to control costs
- 5. increase speed of work

- 6. specialize and decouple processes
- 7. the individual is the cause blame

compared to the Toyota management Kata (a kata is an organization specific practice routine), a system wherein:

- 1. relationships are reality, and management
- 2. nurtures relationships,
- 3. masters life-oriented practices,
- 4. provides output as needed on time,
- 5. changes how work is done,
- 6. enhances continuous flow, and
- 7. when troubleshooting, considers mutual interaction as the cause of a problem not individuals (Johnson and Bröms, pp. 186–87)

Johnson complained about the shortcomings of management by result in an article published in 1992, in a paper he did with Anders Bröms in 1995 (Locke, 1996, p. 287), and he returned to the theme in the book he did with Bröms in 2000. He observed:

"Successful [U.S] managers believed they could make decisions without knowing the company's products, technologies, or customers. They had only to understand the intricacies of financial reporting. ... [B]y the 1970s managers came primarily from the ranks of accountants and controllers, rather than from the ranks of engineers, designers, and marketers. [This new managerial class} moved frequently among companies without regard to the industry or markets they served. ... A synergistic relationship developed between the management accounting taught in MBA programs and the practices emanating from corporate controllers' offices, imparting to management accounting a life of its own and shaping the way managers ran businesses" (Johnson and Bröms, 2000, p. 57).

Johnson came to despise these lifeless pyramidal structures imposed on work processes and managed by computer-oriented-production-control experts:

"At first the abstract information compiled and transmitted by these computer systems merely supplemented the perspectives of managers who were already familiar with concrete details of the operations they managed, no matter how complicated and confused those operations became. Such individuals, prevalent in top management ranks before 1970 had a clear sense of the difference between 'the map' created by abstract computer calculations and 'the territory' that people inhabited in the workplace. Increasingly after 1970, however, managers lacking in shop floor experience or in engineering training, often trained in graduate business schools, came to dominate American and European manufacturing establishments. In their hands the 'map was the territory.' In other words, they considered reality to be the abstract quantitative models, the management accounting reports, and the computer scheduling algorithms..." (p. 23).

The US system of management by result is not only different from management by means but inimical to it adoption. Mike Rother learned this when he and his team spent five years

investigating the Toyota Kata (2004-2009), a system of "unseen management routines and thinking" through which the investigator has to find his way "along unpredictable paths through a systematic process of discovery and adjustments." This became particularly challenging to this group of management consultants when they tried to teach management by means in Western firms whose managers have a command and control mindset. Rother ran into the difficulty especially when teaching Western managers about empowerments. Empowerments seek

"To move decision-making close to where the action is... but just telling people they are empowered [a command and control approach] is insufficient for tapping the brainpower inside an organization in a purposeful way. If people in organizations are expected to make decisions and navigate rapidly at their level, rather than waiting to be told what to do, they need to be taught effective skills for how to do it" (Rother, 2014, p. 4).

To appreciate management by means investigators must be able to grasp all the capacities – thinking, feeling, and willing – that go into an organization wherein employees are empowered. This requires the historian's investigative methods not just those of the mathematically shaped scientific paradigm codified and taught in departments of economics and business schools. The upshot is that economists and MBAs lacked the wherewithal to do much to explain or even to understand the Japanese organizational challenge to US manufacturing from an economic policy standpoint.

For those smitten like Walras with the "physico-mathematical" paradigm, Boulding's suggestion that mathematics is the culprit is unacceptable. They cling to the scientific method while trying to make it relevant by multiplying model building.

But, as Lars Syll noted,

"The insistence on using analytical formalism and mathematical methods comes at a high cost:

[I]t often makes the analysis irrelevant from an empirical-realist point of view. Applying closed analytical – formalist – mathematical – deductive – axiomatic models, built on atomistic-reductionist assumptions to a world assumed to consist of atomistic-isolated entities [the view of orthodox economics], is a sure recipe for failure when the real world is known to be an open system where complex and relational structures and agents interact. Validly deducing things in models of that kind doesn't much help us understanding or explain what is taking place in the real world we happen to live in" [Syll, 2015].

If the economists' models cannot explain what is taking place in the real world, they cannot delineate useful economic policy.

There is, therefore, something wrong with people who continue to follow the same analytic formalism and mathematical methods over and over again, expecting a different outcome – it is one definition of being crazy. Rational people, if analytic formalism and mathematical methods do not uniformly succeed as a guide to economic policy formulation in the real world, would not make them the sole basis of economics.

### Anchoring economic policy discussion in historical analysis

Historians deal with people in their specificities, but historians are generalists not, unless qualified accidentally additionally, experts in science, mathematics, and technology. This means, for example, in the case of Russell Ackoff, that historians understand that he, an acknowledged expert in operations research, developed serious doubts about the usefulness of OR modeling, but they do not try to examine the validity of Ackoff's claims because they are not mathematically and scientifically able to do so; they leave that to the knowledge experts, and report on the findings. The critics of scientific decision modeling cited in this article, are not historians but qualified experts in fields of natural and social science, and mathematics.

The historians' contribution to economic policy formulation stems from elsewhere, that is, their particular expertise in examining the economic experience of people, in the peculiarity as well as similarity of individual and group activities (in companies, communities, regions, and/or nations). The method is comparative history, which excites and informs economic policy discussion, not by revealing eternal laws like economists try to discover in economic models of their own device, but by showing through historical research how different human experiences in the flux of time and specificity of place stimulate discussions of policy making, that, of necessity, are constantly under revision according to circumstance.

Nothing illustrates this approach better than the work of the economic historian Friedrich List in his fight with classical economists about policy matters pertaining to economic development. List, probably the most prominent economic historian during the age of classical economics, lived on the periphery of the London market emporium, in List's case mostly in German lands and America, where in the era of the First Industrial Revolution (1750-1850) people believed that they were disadvantaged by the operation of market forces tied into the market heartland. David Ricardo might talk about comparative advantages in trade, but List thought that the London centered market emporium did not offer a level playing field for less technologically advanced areas competing with the more advanced. Classical economists focused on markets, List focused on nations and regions, whose economic and industrial welfare have incidentally been the principal preoccupation of the educated public in the real world at least since the French Revolution and the Napoleonic wars.

When trying to explain why some peoples succeed more than others, List delved into the histories of communities and national states in various stages of existence, seeking timebased cause and effect reasons for transformation or resistance to economic change. He included classical economics in his nation-based scenario because he thought that it provided an ideological superstructure that promoted the prosperity of London's market emporium at the outsiders' expense (List, 1841, p. 159. Daastøl, p. 241).

For those use to looking at America and Germany as technologically advanced nations, this view appears nonsensical, but in List's time, German-speaking Central Europe was a poor, politically divided, underdeveloped, market segmented land, *vis-à-vis* the economic leader Great Britain, and America in the years List lived there (in the 1820s and 1830s) a technologically backward, half-slave ridden, undeveloped country.

At the national and international level List favored protectionism, for which today's orthodox economists almost exclusively remember and condemn him. The proof he cited to support the policy was not grounded in the *a priori* reasoning of classical economists but in the evidence

provided by the effects of tariff policy in America, where protectionist measures, enacted to shield infant industries from unfair British competition, he believed, had produced prosperous communities. From an economic policy perspective List's views, which were broadly shared by historical economists, have usually prevailed over the free trade advocacy of classical economics. Classical and neoclassical economic models are incapable of explaining how a cartelized German economy became technologically the most advanced industrial economy in Europe between 1871 and 1914 or how the US rose to be the first industrial nation globally during the same period behind high tariff walls.

List's policy recommendations encompassed much more than tariffs. He was particularly outspoken about the benefits of railroads, affirming that transport served primarily a cultural or ideal function, much like the internet today: to convey ideas, spark new ideas, and promote innovation. When evaluating the importance of modern transportation and communications, he perceived the significance of what is now called clustering and networking as a manifestation of mental capital (Locke, 2000, 2015, Chapter I; Locke and Schöne, 2003). He asserted that only in towns do the populations enjoy safe and effective administration; he realized that better communication, because of geographical proximity among actors, boosts cooperation, improves synergy among skills and trades, and fosters efficiency through less travel and consumption of resources like time and energy (List, 1841, pp. 203-204; Daastøl, p. 247).

He thought that the population of an industrialized society had to be brought together in a few conurbations in which are concentrated a great variety of skills, productive powers, applied science, art, and literature. Here are to be found great public and private institutions and associations in which theoretical knowledge is applied to the practical affairs of industry and commerce. Only in such conurbations can a public opinion develop that assures national prosperity (List, 1837, p. 69. Daastøl, p. 246). On the other hand, he argued that scattered productive power in the countryside environment dissipated the productive force of nations (List, 1841, pp. 203-204; Daastøl, p. 247). He also spoke out about the positive effects of urbanization because communication density spread uniform systems of weights and measures and awareness of distance, and time, and urban institutions provide money and credit, regulated by law.

List argued that it was the statesman's duty to adopt policies that would promote these institutions. By making the nation the focus of economics and shifting the analysis from the market place to the competitiveness of nations and regions, he made the visible hand of the state, not the invisible hand of markets, an important source of economic policy formulation.

For those who focus on how to overcome backwardness the Listian tradition is alive. This is true particularly of people who are concerned with national systems of innovation. Their work peaked perhaps with the OECD report in 1997: *National Innovation Systems* (OECD, 1997; Daastøl, pp. 233-36). The organizational theorist Bengt-Áke Lundvall writes that among the group preoccupied with innovation theory, the economist Christopher Freeman by referring to List established a connection between their theory and the development theory that List had pushed in his work on mental capital.

Lundvall noted that List's analysis focused on the development of productive forces rather than on allocation issues, and that he had been critical of and polemical about Adam Smith's "cosmopolitical" approach, where free trade was assumed always to be to the advantage of the weak as well as the strong national economies (Daastøl, p. 236). Freeman explained that

to understand international competition "[W]e must go to the original source of the national competitivity school; Read List in the original, and notice" that the first of his "fundamental points... [is] the importance of mental capital..." ... "If we are really to understand international competitivity, then it is of no use to go back to Adam Smith and still less to Ricardo and the 'school' of neo-classical comparative advantage theory..." (Quotes in Daastøl, p. 236).

The historian's focus on intertemporal comparative analysis also contributes to policy making. During the first industrial revolution in Britain most skilled craftsmen were not formally educated in science or engineering. They were self-trained, or, rather, trained in apprenticeship or in a firm on the job. Know-how was acquired primarily through *tacit learning*, intuitive and inarticulate, only through individual experience in the relevant context, where the knowing subject is involved, as opposed to *explicit knowledge*, codifiable, generated often through logical deduction, capable of being aggregated at a single location, stored in objective forms without the knowing subject being involved in the aggregation – a learning process with which we became so familiar in the 20<sup>th</sup> century.

When Britain had the technological lead in the first half of the 19<sup>th</sup> century its mental capital depended on tacit learning and when British technology moved abroad, the tacitly trained workmen carried the most advanced technology with them. Accordingly, when the French entrepreneurs, Drouillard and Benoist decided to manufacture iron rails for the first French railroads, they turned to the English engineer Charles Manby to build the forges and blast furnaces (à l'anglaise) at the factory site in Alais, Gard. Manby recruited skilled English workers, identifiable from their names in the records of the firm (Wall, Saunders, Shakespear, Crane, and so forth) to construct and run the factory. The specificity of historical investigation, e.g., correspondence and company archives reveals the education and training suited to the state of productive forces at the time (Locke, 1978, p. 45).

During the Second Industrial Revolution the mental capital needs that drove productive forces during the First Industrial Revolution changed. This is obvious for the science-induced industries of the Second Industrial Revolution, like coal tar color dves, pharmaceuticals and in the new firms employing the electrolysis processes, like aluminum, but it was also the case in older industries as they progressed technologically, in metallurgy, for example, that moved from iron to the new alloy steels used in bearings for high speed machinery and internal combustion motors, and in other scientifically based product lines - in shipbuilding, in the conversion from steam to diesel and electrical power packs, and even in new methods of steam locomotion, in mechanics, etc. The administrative demands in firms also grew as the visible hand of management in large firms and in producer and distribution regional trade associations replaced the invisible hand guiding competition in a forest of firms, which classical and neoclassical economists affirm, guaranteed market efficiency. A new organized capitalism emerged whose mental capital requirements could no longer be entirely met through the know-who networks of men tacitly trained on the job. The older networking of practical men remained, but to it was added networks of scientifically trained chemists and engineers stemming from institutions of higher education to man the new high tech industries.

For those desiring to remain on the cusp of technological change, in Britain, in France, the new mental capital requirements set off alarm bells because they feared that the German education-high tech industrial Gestalt had provided the lead.

Investigating this transformation is the meat of historians as it was for contemporaries who were witnessing the change. In France and Britain contemporaries discussed comparative

systems of education with an eye to educational reform. Educators treated the subject directly, e. g., Charles Lauth (1900), *Rapport général sur l'historique et le fonctionnement de l'Ecole Municipale de Physique et de Chimie industrielle*; Albert Granger (1901), *Etude de quelques laboratoires industriels et des écoles techniques supérieures en Allemagne*; so did parliamentary and government publications that resulted from investigations, for example, "Report of the Select Committee on Scientific Instruction" (1868), "Report of the Royal Commission on scientific instruction," reports and minutes of evidence (1882-84) that discussed British worries about a faltering technology due to educational deficiencies in the mental capital stock of the late 19th century.

Because historians, unlike orthodox economists who develop a purely economic framework of understanding, add the dimension of institutional heritage to their understanding of the present, they also through intertemporal comparisons investigate the non-intellectual, noneconomic factors influencing policy making. Just as cultural historians look to Japanese group consciousness rooted in their past when they explain the effectiveness of management by means in the Toyota Kata, the historian's time based analytical dimension permits them to consider how noneconomic institutions that pre-existed the Second Industrial Revolution, and were not economically created by it, inadvertently fuelled high tech industrial development that brought a new technological order into being. In Germany the scientific and research values of *Wissenschaft* institutionalized in academic culture when Germany was disunited and economically backward, proved to be much more fruitful in the development of engineering and business educational networking during the Second Industrial Revolution than did English and French traditions in higher education.

Unlike economists who imbibed analytical formalism and mathematical methods, historians are not in the predicting business. They do not claim to be able to foretell the future from a knowledge of the past because they know when they learn their craft that there are too many unknowns left out of the inherited record, too much of the future is unexpected, irrational and, hence, unpredictable, and too many anachronisms exist inside the historian's mind to engage in accurate, precise prognostication. However, historians also know that human beings are immersed in the evidence of their past and that intertemporal and international comparative histories in their specificities provide a rich source of tacit and explicit knowledge for the public debate about economic policy options that are germane to the economic situations in which people live.

This is as true today as it was in List's time. Recently a group of German business economists asked me to participate in a workshop about the *Ideengeschichte der BWL* (Intellectual History of German Business Economics). My task was to compare German and American management education. I seized on the opportunity to compare different historical experience in a paper entitled: "Reflections on the Response of BWL and US MBA business school education to three major incidents in recent economic process: 1. The Japanese challenge in manufacturing in the 1980s and 1990s, 2. The start-up habitat of Phenomenal Silicon Valley at the turn of the 21<sup>st</sup> century, and 3. The financialization of the economy, which has occurred with growing intensity since the 1990s." Dominique Turcq, who heads a very savvy management discussion group in Paris (the Boostzone Institute) and has worked in consultancy for decades on comparative American, French, Japanese, and German management, published an edited version of the paper on his blog, with the following introductory comment:

"We generally do not present academic paper on our blog but we are happy to be able to present this because some of the major elements of this document, although lessons from history, are major inputs, in my view, on how to think about management, innovation, education and training, for the uncertain and complex future we are facing. In particular this paper shows how the lack of foresight, of contact with the field and of understanding of the Japanese manufacturing revolution by management education institutions in the US (and all Europe but Germany) disadvantaged the development of industry in Europe (but Germany), how most management education institutions missed completely the innovation and entrepreneurship challenges raised by the Silicon Valley, how the financial education bias in management education taken in the last two decades have led to another complete misunderstanding of what the real world is about and, quasi directly, into major and socially dangerous income discrepancies in our societies. The German model, with significant elements like a priority to technical education, to practical training, the importance of co-determination, the illegality of stock options as a mean of compensating senior executives until 1999, the closeness to field banking systems, etc. is brought to a new light and helps to understand major differences between the German economic model and most other ones in the Western world. This paper not only very seriously questions the model of MBAs but also shows us how education choices and financing choices can be determinant in the long term view of a corporate/ country competitiveness. One quote before you start reading: 'The competence acquired from a business school MBA education did not prompt start-up entrepreneurialism or produce venture capital innovative IT firms'" (Turcq, 2015).

If, as Shiller and Wang write, people are the subject of economics and policy formulation is its principal aim, and the work and methods of historical research and exposition can shed light on both, then why are historians so ignored in economics? The answer is that they were not neglected until after World War II when a combination of historical forces conspired to enable a number of wilful men and women to impose a "scientific paradigm" on their subject that eliminated historical and institutional economists from the discipline. Historians still read the works of Schmoller, Sombart, Weber, Veblen and other once famous economists but in economics their work has been dropped to be replaced by the new so-called scientific paradigm. George J Stigler in an article published in 1964, notes how quickly after the war neoclassical economists and econometricians triumphed over the old study programs:

"Whereas in 1892-93, forty percent of the references cited in American economics journals were in foreign languages and half of these in German, total foreign language citations have fallen to less than four percent in recent times and German has almost vanished as a foreign language from American economics" (Stigler, 1965, p. 47).

Stigler went on: "If references to Schmoller are now rare, references to differentials and matrices have made some sort of compensation" (Stigler, 1975, p. 47).

It is relatively easy for historians to find critics of orthodox economics to quote, like Syll and numerous other disappointed social scientists, e.g., Ackoff, Johnson, et al cited in the text. Their work combined with comments in the public media remind people every day of the

failure of the new paradigm economists to anticipate economic crises, to devise policies that can quickly end them, to even be able to decide "scientifically" when the Federal Reserve should raise or lower interest rates and how much. It is much harder, however, to find economists who can appreciate the historian's work, since the historical method, historical exposition -- history itself has been banished from their province. In their work, mainline economists and their critics read and quote the work of economists trained in the new paradigm almost exclusively. When historians make comments about policy decision making based on historical research, economists mostly ignore them – not because the historians are wrong – but because economists have in the last three decades of the 20<sup>th</sup> century created an academic culture that lacks the historical consciousness and knowledge necessary to dialogue.

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# The natural capital metaphor and economic theory

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### Introduction

The natural capital (NC) metaphor is currently being introduced to provide a framework for the economic measurement of environmental degradation. Advocates of the NC metaphor argue that because the depletion of "natural stocks" is not adequately measured, economic activity ignores the costs of environmental destruction. Measuring in economic terms the stocks of "natural capital" and of their "ecosystem services" is the central objective of the natural capital metaphor.

The metaphor makes a clear reference to neoclassical marginal theory that considers capital as a "factor of production", on the same footing as labour, and involves a conception of capital as a stock that produces an annual flow of final goods. The literature on natural capital typically implies there is a smooth passage from the domain of metaphor to the realm of rigorous economic analysis.

The purpose of this paper is to examine the roots, scope and limitations of the "natural capital" (NC) metaphor from the standpoint of economic theory. We identify several deep problems affecting the use of this metaphor and conclude that as a result this natural capital approach will not be able to deliver on its promises to measure natural capital stocks or the stream of natural capital services. It is likewise unable to assess the economic costs of environmental degradation or what it means to maintain natural capital intact.

The structure of this paper is as follows. The *first section* focuses on the nature and structure of this metaphor and on the attempts to provide a definition of natural capital. The *second section* examines the relation between the NC metaphor and the concept of capital in marginal economic theory and in the context of aggregate production functions. The backdrop here is provided by the Sraffa-based critique to marginal capital theory. In the *third section* we shift to general equilibrium theory, where there is no need for an aggregate production function and no uniform rate of profit. Stability analysis in general equilibrium systems, as well as the negative implications of the Sonnenschein-Mantel-Debreu theorem provide the setting for this part of the analysis. The *fourth section* concentrates on a different set of problems encountered by proponents of the NC metaphor and examines the difficulties in assigning prices to "nature's stocks" and their "environmental services", especially when reverse capital deepening becomes an empirical possibility and reswitching emerges in the use of discount rates. We include here a reference to uncertainty and to financial capital. Our concluding remarks summarize the main findings and offer some alternative approaches.

# SECTION I Natural capital: metaphors and definitions

Metaphors, similes and analogies have played a role in science and in explaining abstract concepts to non-specialists. They can indeed be useful to convey an image, but what may be

gained in communication may be lost in precision. There is a significant risk of making good metaphors that can lead to flawed reasoning. This is an important point because metaphors often acquire a life of their own and they can end up, as Vickers (1984) points out, owning the people that created them.

The origin, structure and scope of metaphors need to be well understood, not only to take advantage of their potential, but also to understand their limits. Failure to grasp the boundaries of a metaphor may lead to abuse in its application. In particular, when a concept is transposed metaphorically from one discipline to another, its theoretical status needs to be well understood. If that concept is undetermined, the validity of the metaphor needs to be questioned. The poetic value may be important, but its accuracy may be wanting or even misleading.

The notion of "natural capital" has a short history. It began to be used by authors like Schumacher (1973) and it appeared (under a slightly different terminology) in *Our Common Future* (1987), the report of the Brundtland Commission. The Millennium Ecosystem Assessment (2005) explicitly recognizes it as an "economic metaphor". Attempts to define this notion in more precise terms were first published by Pearce (1988), Costanza (1991), Costanza and Daly (1992), Daly (1994) and El Serafy (1996).<sup>1</sup>

In their attempt to define natural capital Costanza and Daly (1992) state that:

"Since 'capital' is traditionally defined as produced (manufactured) means of production, the term 'natural capital' needs explanation. It is based on a more functional definition of capital as 'a stock that yields a flow of valuable goods or services into the future'. What is functionally important is the relation of a stock yielding a flow - whether the stock is manufactured or natural is in this view a distinction between kinds of capital and not a defining characteristic of capital itself."

The background here is of course the original definition of capital in Fisher's book *The Theory of Interest* (1930).<sup>2</sup> In fact, almost all of the references to the notion of natural capital depend heavily on the analysis of Irving Fisher on capital as a stock that is a source for a flow of income (see Daly 1994 and Lawn 2006). Adding the word 'natural' to Fisher's definitions of capital and income takes these and other authors to the notion of 'natural capital'.

Today the use of the terminology of natural capital follows this tradition (TEEB 2010, Voora and Venema 2008, WAVES 2015). International business organizations and conferences promoting the use of this approach rely on the same definition.<sup>3</sup> But the problem with these "definitions" is that their simplicity betrays their inaccuracy.

The natural capital approach is based on the premise that human societies have at their disposal a collection of "assets" that provide streams of services. The different collections of assets include manufactured production goods, financial assets, as well as "human" and

<sup>&</sup>lt;sup>1</sup> A good analysis and synthesis of the evolution of this metaphor is found in Akerman (2005).

<sup>&</sup>lt;sup>2</sup> A modern account of Fisher's analysis is provided in Hirshleifer (1958).

<sup>&</sup>lt;sup>3</sup> See for example the definitions used by the Natural Capital Coalition

<sup>(&</sup>lt;u>https://www.naturalcapitalcoalition.org</u>), the World Forum on Natural Capital (<u>http://naturalcapitalforum.com</u>) and the Natural capital Declaration (<u>http://www.naturalcapitaldeclaration.org</u>).

"social" capital. All of these have their own problems when it comes to their conceptualisation, but this paper focuses on the notion of "natural" capital.<sup>4</sup>

Although the natural capital approach is based on a metaphor, many of its advocates feel they must provide a definition. Definitions need to express the essence or genus of the object being considered, plus a differentia that separates this object from others that are also contained in the genus. The differentia typically describes the attributes or the specific qualities that distinguish an object from all other elements of the same class. But in all the "definitions" of natural capital in the literature there is nothing of this to be found: there is no genus and there is no differentia.

Take into account the following example. The World Forum on Natural Capital (held in Edinburgh in November 2015) considered that "Natural Capital can be defined as the world's stocks of natural assets which include geology, soil, air, water and all living things". According to this text nature's components are a stock of capital, an asset. In this text, as in many others that make the same mistake, there is no explanation of the essence of the genus: there is no explanation of the essence of the differentia.

In addition, texts discussing natural capital typically involve circularity: natural capital, we are told, is the name given to a stock of capital assets that we find in nature. This adds nothing to our knowledge and fails the test of any definition. This type of "reasoning" is deeply flawed: in logic one cannot use the term being defined as a part of the definition.

The lack of a serious definition is accompanied by a shift from metaphorical analogy to identity as the notion that "Nature Is Capital" is repeated without respite. As Vickers (1984) points out this is similar to the procedure followed by Paracelsus and other alchemists who supported their "assertions with a style of argument that moved directly from analogy to identify, literalizing metaphors to elide distinctions and fuse disparate realms". In this process the limits of analogies become lost in translation and metaphors are transformed into identities.

Proponents of the NC metaphor say that the objective is to be able to measure natural capital in economic terms. Their procedure consists of two steps. First, the natural capital metaphor is used to establish an identity: "Nature's assets are Capital". In the second step the actual measurement of these 'assets' is attempted. Since these 'natural assets' are not man-made and many do not have prices attached to them, pricing the different components of "natural capital" and its flow of services requires different valuation techniques. But there is a question that should be examined first: is it possible to measure "capital" in economic terms?

# Section II. Natural capital and the aggregate production function

How much natural capital is there available? This is a question that adherents to the NC approach would like to answer. They also seem to think that in economic theory the same question about man-made capital has been answered satisfactorily. Indeed, this is not the

<sup>&</sup>lt;sup>4</sup> The notion of capital is therefore extended to almost everything that exists in our universe when this list of "assets" is considered. Proponents of this approach should be aware of the fact that when one word serves to designate everything (as in "Everything is Capital") it can become meaningless.

case. In this section we will see why the question 'What is the quantity of capital?' does not receive an unambiguous answer in economic theory.

The concept of capital has always been at the centre of theoretical discussions in economics. The most important theoretical debate in the second half of the twentieth century, known as the Cambridge controversy centred precisely on the concept of capital. The use of the natural capital (NC) metaphor cannot ignore the essence and implications of this debate.<sup>5</sup> The classic and comprehensive account of this debate is Harcourt (1972), while a more recent description is Cohen and Harcourt (2003).

The centre of this debate is simple. The word 'capital' has two different meanings in economic theory: it denotes a sum of money and it also serves to designate a set of machines, tools and other heterogeneous production instruments. Of course, it can be assumed (not without problems) that labour can be measured in man-hours with the help of some kind of index number solution, but capital poses a unique problem. Machine tools, blast furnaces, trucks and shuttle-less looms are heterogeneous objects that cannot be added in any simple manner. In other terms, there is no physically homogeneous and malleable substance called 'Capital' that can be applied to the production of all kinds of goods. However, neoclassical economists assume that the two notions can be used interchangeably: the money value of machine tools and buildings is assumed to be a good proxy for the physical quantities of these production goods. This is something analogue to the "solution" of the proponents of the natural capital approach: we only need to put a price on the different components of Nature (the stock) and on the stream of ecosystem services (the flow) in order to have a measure of Natural Capital.

Providing a theoretical foundation for this view of capital as a factor of production was a key component of marginalist theory (Wicksteed 1894, Clark 1899). The main objective of this variant of Neoclassical theory was to prove that the laws of distribution of income in a capitalist society were linked to the contribution to output made by each component of society (for a modern exposition see Ferguson 1969). In its simplest form, the neoclassical theory of marginal productivity formulation used an aggregate production function:

(1) 
$$Q = f(K,L)$$

In this expression Q is total output and K and L denote the "factors of production", capital and labour respectively. The marginal product of the factor capital is defined as the change in output Q that results from employing an additional unit of the factor K. A similar procedure defines the marginal product of the factor labour. The marginal product of each one of these two factors is given by the following expressions:

(1a) 
$$MP_{K} = \frac{\Delta Q}{\Delta K}, \quad MP_{L} = \frac{\Delta Q}{\Delta L}$$

From equations (1a) it is clear that Neoclassical economic theory required a measure of capital that was independent of prices and distribution for a fundamental reason. However, the result of the Cambridge controversy on capital theory showed that this is the case. The

<sup>&</sup>lt;sup>5</sup> Winnett (2005) is one of the few analyses where the Cambridge controversies are mentioned as an important issue in the context of debates on natural capital, but technical details are not examined.

starting point of the critique of the aggregate production function takes into account the crucial fact that these capital or productive goods are themselves commodities that have been produced. Once we take this into consideration it is possible to observe that the money value of machinery cannot be used as a proxy for the amount of machinery used in production.

The problem arises because machines and tools are themselves produced commodities and their prices depend on the rate of profit. Or, in other terms, the distribution of income affects prices. This is the crucial element in the debate on capital theory and the demonstration of this was first presented in Sraffa (1960). In his model prices are determined by conditions of production (i.e., technology) and by the distribution of income. So in order to determine prices it is first necessary to know what the distribution of income looks like.

This has devastating consequences: the entire neoclassical edifice is affected by circularity and this has fatal implications for the neoclassical concept of capital. The rate of profit is the output divided by the value (price) of the capital good used in production: Rate of profit = Output/Price of Capital. But if the price of capital is affected by distribution this involves circularity: the rate of profit depends on the price of capital, but the price of capital depends on the rate of profit. This means that the rate of profit cannot be determined endogenously in these models.

Following Sraffa and using a formulation in Pasinetti (1977) for an economy producing n commodities (and where the means of production are themselves produced commodities) the price system can be written as follows:

$$(2) \quad pA(1+\pi)+l_nw=p$$

In this expression **A** is the matrix of technical (interindustry) coefficients, **p** is the price vector,  $\pi$  is the rate of profit,  $l_n$  is the n-dimensional vector of direct labour coefficients (for the n industries) and w is the wage rate. The methods of production are such that each industry produces a single commodity by using certain physical quantity of direct labour (represented by the corresponding component of vector  $l_n$ ) and certain physical quantities of other commodities (represented by the corresponding technical coefficients of matrix **A**).

The solution of this system is given by:

(3) 
$$p = l_n [I - (1 + \pi)A]^{-1} w$$

In equation (2) we can observe that each price is thus determined by the conditions of production, but also by the state of distribution (given by the levels of w and r). In other terms, the structure of the price system depends in general on the technical coefficients of production (labour given by  $l_n$  and inputs given by matrix **A**), as well as on the particular level of the profit rate.<sup>6</sup>

<sup>&</sup>lt;sup>6</sup> Under certain conditions on matrix **A** the price vector **p** is strictly positive. The first is that matrix **A** must be non-negative. The second is that the maximum eigenvalue  $(\lambda_m)$  of matrix **A** must respect the condition  $\lambda_m \leq 1$ . If this condition is not satisfied we would be dealing with a system so backward that it would not be able to generate a profit even with a zero wage rate (Pasinetti 1977).

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Prices of capital goods (such as machines) thus depend on their production conditions, but also on the state of the distribution variables (wages and profits). This brings about serious problems for the measurement of capital. To give an idea of how prices change when distribution is modified consider the price of capital good j expressed in terms of commodity 1 (and setting w = 1) as given by the following expression (Pasinetti 1977:82):

(4) 
$$\frac{p_j}{p_1} = \frac{l_{nj} + (1+\pi) \sum_{j=1}^{n-1} a_{ij} p_i}{l_{n1} + (1+\pi) \sum_{i=1}^{n-1} a_{i1} p_i}$$

The derivative of this expression with respect to the rate of profit,  $\pi$ , is:

(5) 
$$\frac{d}{d\pi}\left(\frac{p_{j}}{p_{1}}\right) = \left[p_{1}\sum_{i=1}^{n-1}a_{ij}p_{i} - p_{j}\sum_{i=1}^{n-1}a_{i1}p_{i}\right] + (1+\pi)\left[p_{1}\sum_{i=1}^{n-1}a_{ij}\frac{dp_{i}}{d\pi} - p_{j}\sum_{i=1}^{n-1}a_{i1}\frac{dp_{i}}{d\pi}\right]$$

It will be positive or negative

$$\frac{d}{d\pi} \left( \frac{p_j}{p_1} \right) > 0 \quad \text{or} \quad \frac{d}{d\pi} \left( \frac{p_j}{p_1} \right) < 0$$

depending on two different factors. First, the sign depends on the capital intensity effect given by the comparison of the costs of production of commodity j (the capital good) and commodity 1. This comparison is contained in the first bracket of equation (5). For commodities that have a greater (lower) capital intensity than commodity 1, this effect will always be positive (negative).

The second factor can be called the price effect: it is related to the movement of all prices in the economy. This is captured in the second bracket of equation (5). The main difference with the capital intensity effect is that the price effect cannot be determined unambiguously at the level of the commodity being considered (capital good j). Now changes in the price of any commodity (including capital goods) depend on how all the prices change in the whole system. The price effect is not predictable at the level of any given industry. Thus the sign of this derivative depends on the price system of the entire economy.

There are several implications from this. The first is that it is not possible to think of a sum of money (the aggregated prices of machines) as a good representation or proxy of the collection of heterogeneous machines and tools that economists would like to consider "capital". It is now impossible to talk about the "quantity of capital" as an autonomous concept: if the price of machines and equipment depends on the distribution of income, then the *quantity of capital* depends on the distribution of income. In other terms, for every state of the distribution variables there corresponds a new set of prices and, thus, a new "quantity of capital". The reference used by advocates of the natural capital metaphor cannot provide a good foundation for the objective of measuring "nature's assets".

The second implication is that the neoclassical relation between the quantity of capital and the rate of profit is destroyed. Neoclassical theory wanted to show that marginal productivity would fall as the quantity of capital increased. In fact, the problem we are examining here reveals that not only is there no uniform relationship between the rate of profit and the amount of capital, but also the direction of causation works in the opposite direction too. The amount of capital depends on the rate of profit and not the other way around.

Finally, the third implication is that because the quantity of capital depends on its price it is not possible to rank the techniques of production in terms of their "intensity of capital": the order or ranking of these techniques of production is modified each time distribution changes. Choice of techniques becomes a problem and this affects the assumption that firms are rational agents that select the more efficient combination of 'factors of production'.

This opens the door for the phenomenon of "reswitching" (Sraffa 1960, Pasinetti 1966): if at the rate of profit  $\pi_1$  method M1 is the most profitable method of production, and if at profit rate  $\pi_2$  ( $\pi_2 > \pi_1$ ) technical method of production M2 (for producing the same commodity) becomes the most profitable one, then reswitching occurs when technique M1 becomes once again the most profitable one at an even higher rate of profit  $\pi_3$  ( $\pi_3 > \pi_2 > \pi_1$ ). The possibility of reswitching contradicts the neoclassical postulate that techniques with lower intensities of capital become eligible at higher rates of profit.

What are the implications of reswitching for the notion of natural capital? One of the most important implications concerns the issue of substitutability between man-made capital and natural capital. Using an aggregate production function where natural capital is introduced as a factor of production carries the assumption of factor substitution. In the case of man-made capital and labour, substitution takes place in accordance with movements in the relative prices of these two "production factors". What happens when natural capital is added as a third "factor of production"?

# Natural capital and weak sustainability

The idea that natural capital is a factor of production leads directly to the question of substitutability between factors. To their credit, many authors who advocate the use of the natural capital metaphor disapprove the idea of replacing natural capital by manufactured capital (Costanza and Daly 1992). However, the use of aggregate production functions implies factor substitution and this possibility lies at the heart of the notion of weak sustainability.

According to Pezzey (1992) the most commonly used definition of sustainable development (that welfare of future generations should not be less than the welfare of the current generation) can be interpreted in terms of non-declining utility and maintaining stable total capital stocks at a macroeconomic scale. This allows for the problem of weak sustainability to be examined at an aggregate level in order to determine the conditions under which these outcomes can be attained.

A strong connection can then be established with well-known problems in neoclassical growth theory when exhaustible resources are explicitly taken into account in the context of an aggregate production function. For example, Stiglitz (1974) uses a model with an aggregate production function to characterize steady state paths in economies with exhaustible natural

resources and examine the conditions under which a sustainable level of per capita consumption is feasible. Solow (1974) looks at the conditions under which a non-declining positive level of output or consumption can be sustained indefinitely when production depends on the non-renewable natural capital. Substitution between all factors of production, including man-made capital and natural capital is a central assumption to prove the validity of this condition (Hamilton 1995).

The phenomenon of reswitching has deep negative implications for neoclassical models when only two factors of production are considered. These problems do not go away when a third factor called "natural capital" is introduced. Consider for example the model in Facheux, Muir and O'Connor (1996): it is an overlapping generations model with a production function using manufactured capital, labour and natural capital. The model uses a standard Cobb-Douglas function

$$Q = f(M, L, R, t) = M^{\alpha_1} L^{\alpha_2} R^{\alpha_3} e^{\lambda t}$$

where Q is output, M is manufactured capital, L is labour, R is natural capital (used in production) and  $\lambda$  is the rate of (time invariant) technical progress. The parameters  $\alpha \mathbf{l}$ ,  $\alpha \mathbf{2}$  and  $\alpha \mathbf{3}$  designate the respective output elasticity of the three inputs, manufactured capital, human capital and natural capital. As in any Cobb-Douglas function the elasticity of substitution between the three forms of "capital" is equal to unity (that is,  $\alpha \mathbf{l} + \alpha \mathbf{2} + \alpha \mathbf{3} = \mathbf{l}$ ). And as in standard definitions of weak sustainability manufactured capital and natural capital capital undistinguishable (Ekins et al 2003).

But how exactly should agents in the model decide to replace one form of capital for another? Can they select more or less "natural-capital intensive" technologies? The aggregate models that incorporate natural capital do not specify microeconomic (behavioural) rules for the substitution between man-made capital, labour and natural capital. Clearly substitution cannot be assumed to take place in accordance with the rules set forth in neoclassical capital theory because, among other things, we would re-encounter the problem of reswitching. Modellers working with natural capital have not explicitly discussed this problem. Thus, the models assume substitutability but do not specify just how agents should go about in the process of substituting natural capital for the other two forms of capital.

It can be stated that behavioural rules for capital substitution are not specified in the models because like their relatives in neoclassical growth theory, their objective is to derive macroeconomic conditions for certain types of growth paths. For example, the Hartwick-Solow condition states that investing (natural capital) resource rents in producing man-made capital is a sufficient condition for weak sustainability. In other words, the problem at hand is to define macroeconomic conditions that are required to maintain natural capital stock intact or how consumption (and utility) can be sustained indefinitely.<sup>7</sup> Unfortunately, the building blocks of these models are flawed because there is no unambiguous measure of man-made capital. The foundations that a policy-relevant model should possess are lacking.

Aggregate models using the notion of natural capital to examine conditions of weak sustainability cannot serve the purpose for which they were created. Those conditions cannot

<sup>&</sup>lt;sup>7</sup> This explains why Cabeza Gutés (1996) concludes that the notion of weak sustainability "can be presented as a direct application of the savings-investment rule from growth theory with exhaustible resources".
be related to decentralized market economies and the problems associated with the measurement of man-made capital do not allow us to rely on those models for policy making. For example, through the assumption of perfect substitutability of factors of production, the metaphor of natural capital is directly related to the notion of biodiversity offsetting. There are many definitions of biodiversity offsetting, but they all involve different mechanisms that allow for full compensation for negative impacts on biodiversity through other investment projects. It is a notion closely related to the goal of "no net loss" when it comes to negative impacts on biodiversity. There are many problems associated to this idea but our analysis uncovers a new difficulty: reswitching means that using a monetary measure for the components of man-made and "natural capital" can lead to erroneous choices and mistakes in the attempt to achieve offsetting and "neutral impact" schemes.<sup>8</sup> The entire discussion on natural capital in the context of so-called weak sustainability becomes meaningless.

This is not the only problem. Most of these models use the notion of a "representative agent". For example, Pezzey (2001) uses a representative agent model with an aggregate production function to examine weak sustainability conditions and the role of policy instruments. He recognizes that the model's simplifying assumptions limit the policy relevance of its results and states that the representative agent framework obscures the interaction of separate generations. On the other hand, Faucheux et al (1996:529) affirm that we can think of these models as "expressions of social choices".

But the problem with the "representative agent" is not that it obscures relations between generations. And this notion cannot be used to model expressions of social choices. The difficulty is that the aggregation process needed to arrive at this fiction does not conserve the properties of individual rationality that are ascribed to the representative agent. This is a fiction that has been logically discredited by the simple fact that the weak axiom of revealed preferences does not hold for market excess demand functions. In addition, the Sonnenschein-Mantel-Debreu theorem has revealed that aggregation of preferences in the context of general intertemporal systems only conserves continuity, Walras' law and homogeneity of degree zero of the excess demand functions. These properties are not enough to provide a suitable structure to the market excess demand function. Models relying on the fiction of a representative agent are misleading in macroeconomics and are deceptive in environmental policy making.

The critique raised by academics in Cambridge University gave a devastating blow to the school of neoclassical economics that relies on an aggregate production function. In a well-known article Samuelson (1966) conceded defeat and accepted the arguments of the critics to the neoclassical school stating that "If all of this causes headaches for those nostalgic for the old time parables of neoclassical writing, we must remind ourselves that scholars are not born to live an easy existence".<sup>9</sup> Advocates of the natural capital metaphor need to stop living in a world of parables and metaphors and should derive the logical consequences of the critique to the marginalist version of neoclassical economic theory.

<sup>&</sup>lt;sup>8</sup> One important question relates to the negative incentives that this creates for the reduction of negative environmental impacts when an investor can simply "compensate" or "offset" it. The problem of reswitching exacerbates this problem.

<sup>&</sup>lt;sup>9</sup> One key result of the Cambridge controversies is that the use of time-preference, "patience" and "time" did not allow for escape the criticism addressed by the Sraffian model (the same applies to the approach à *la* Böhm-Bawerk on the "average period of production" to characterize methods of production). In none of these cases is there a concept of capital that is independent of the rate of profit.

# SECTION III. General equilibrium theory: disequilibrium and scarcity

The critique outlined in the previous section is addressed to neoclassical capital theory based on long-term stationary equilibrium using an aggregate production function and assuming the existence of a uniform rate of profit throughout the economy. But what happens if we move into the realm of general inter-temporal equilibrium models where the frame of reference is the short term and there is no uniform rate of profit? This is a relevant question since work on natural resource management has already begun using applied general equilibrium systems (Jorgenson and Wilcoxen 1990, Persson and Munasinghe 1995, Conrad 1999).<sup>10</sup>

In general equilibrium theory there is no aggregate measure of capital and every capital good has its own rate of return. The different versions of the Arrow-Debreu model accommodate a large number of producers and each one may use any of a number of capital goods (and intermediate inputs) as it maximizes its profit function on its production possibility set. Each capital good has its own price and ecause each different capital good has its own rate of return, there is no reference to a thing called "capital" or to a uniform rate of profit. According to Hahn (1975, 1982) general equilibrium theory is unaffected by the Sraffa-based critique that led the attack on marginalist theories.<sup>11</sup> That is a debatable assessment: both Garegnani (2011) and Schefold (2005) have shown that the Sraffa-based critique is also relevant for inter-temporal general equilibrium models. Schefold in particular has proven that reswitching has negative implications for the stability of equilibrium. This makes the Arrow-Debreu model of little use when seeking for good theoretical foundations for the natural capital approach.

There are other serious problems affecting general equilibrium models that are of great relevance to the natural capital metaphor, especially from the vantage point of the valuation of the stock of natural capital and the flow of ecosystem services. Perhaps the most important problem is that Arrow-Debreu general equilibrium models have not been able to yield good results when it comes to stability theory (i.e., the formation of equilibrium prices). This has deep implications for the objective of putting a monetary value on natures "assets".

In the late fifties Arrow and Hurwicz (1958) and Arrow, Block and Hurwicz (1959) were able to prove stability theorems in a general equilibrium framework, but only after extreme conditions were assumed: gross substitutability and the weak axiom of revealed preferences at the market level.<sup>12</sup> The first involves rather strange economies that are difficult to imagine, while the second involves a contradiction because the weak axiom of revealed preferences is not valid at the market level. Scarf (1960) showed through a counter-example that conjecturing about the generality of stability in GE models was unjustified. Stability has remained an intractable problem for general equilibrium theory. Efforts to build better tâtonnement and non-tâtonnement models (where trading takes place at disequilibrium prices) in a general

<sup>&</sup>lt;sup>10</sup> For a detailed critique of computable general equilibrium models, see Taylor and Arnim (2006), Ackerman and Gallagher (2004) and Stanford (2003).

<sup>&</sup>lt;sup>11</sup> Garegnani (1976) attributes this feature to a change in the notion of equilibrium and the abandonment of the long-term perspective where all individual profit rates converge to a uniform rate. These changes are due to "weaknesses in the dominant theory of distribution and, in particular, of the conception of capital it relies on. The attempt to overcome the deficiencies of the received notion of capital (...) provides the main explanation of the move towards short-period equilibria and their sequence in time."

<sup>&</sup>lt;sup>12</sup> It is important to note that both *tâtonnement* and *non-tâtonnement* models require the presence of a auctioneer, an agent that announces prices, calculates market excess demands and adjusts prices (according to the law of supply and demand) in a centralized manner. This of course belies a good representation of a decentralized market economy.

equilibrium context failed to provide a satisfactory answer to the question of equilibrium price formation.

This is highly relevant for any discussion about market-based policy instruments. In neoclassical economics only in equilibrium are prices a signal of scarcity and only in that case can it be said that there is an efficient allocation. In disequilibrium there are positive excess demands and therefore prices do not reflect in any sense scarcity. This has deep implications for the valuation techniques of "natural capital", a point to which we return below.

In 1974 a new and even more serious problem surfaced affecting every aspect of general equilibrium theory, including the disaggregated measure of all capital goods. The Sonnenschein-Mantel-Debreu theorem of 1974 (Sonnenschein 1973, Debreu 1974, Mantel 1974) showed that for a continuous function that is homogeneous of degree zero and that respects Walras' law, there is an economy with at least as many agents as goods such that for prices bounded away from zero the function is the aggregate demand function of the economy. This means that the assumptions that specify well-behaved demand functions at the microeconomic level do not carry over to the aggregate level. In other terms, the market demand curve does not necessarily have a downward slope. This poses a serious problem: stability results (attaining an equilibrium price vector) will not be able to be attained unless *ad hoc* restrictions are imposed on the excess demand functions.

The SMD theorem applies even under extreme conditions. Mantel (1976) was able to demonstrate that even with homothetic preferences the conclusion of the SMD theorem is verified. And Kirman and Koch (1986) showed that even if we assume that collinear endowments (fixed income distribution) the theorem still holds. Price formation processes become anarchic and will not necessarily lead to equilibrium (efficient) allocations. The traditional interpretations of a price formation mechanism simply fall apart under the impact of the SMD theorem. This explains why Mas-Collel, Whinston and Green (1995) aptly describe this theorem as the "Anything Goes Theorem".

How does this affect the natural capital approach? First, the SMD theorem shows that the market excess demand curve has no structure and therefore implies that the law of supply and demand does not apply to the market demand curve. This means that, for example, the demand curve for natural capital that Pascual and Muradian (2010) borrow from Farley (2008) does not have the negative slope indicated in their diagram. That curve may show segments with a positive slope and this throws any conceivable economic adjustment process in disarray. For example, it is conceivable that as the price of natural capital increases its demand could also rise.

Second, if stability is not an intrinsic property of equilibrium then we need to focus on disequilibrium prices. But the nature of these prices is very different from equilibrium prices: in disequilibrium agents are aware of arbitraging opportunities and adapt their plans to take advantage of them. Disequilibrium prices may convey the weight of market power and strategic behaviour of different agents, but these prices are not signals of scarcity or of market efficiency. In other terms, disequilibrium prices do not denote in any meaningful way what is the real scarcity of natural capital. These prices are not a good reference for environmental policy. As Rizvi (2006) has pointed out, "observations on market prices alone do not restrict in any meaningful way the sort of economy that could have generated them".

# **SECTION IV** The fallacy of measurement

There is another angle to the problems associated with the NC metaphor: how exactly are the values of natural capital measured and in what units of account? The valuation techniques used in the context of the natural capital approach have serious limitations that have been identified by many ecologists (Chee 2004 and references therein). Here we highlight the inconsistencies and shortcomings of these valuation techniques from the standpoint of economic theory.

TEEB (2010a) contains a description of methods commonly used to determine the value of natural capital and of the flows of ecosystem services. It also classifies valuation techniques into three categories: direct market valuation, revealed preferences and stated preferences approaches. The first group includes market and cost-based approaches, as well as the production function approach.<sup>13</sup> The second involves a travel cost method and a hedonic pricing scheme to estimate (monetary) values for NC and ecosystem services. The third category comprises techniques that simulate markets and demand for ecosystem services.

The valuation techniques used in the context of the natural capital approach yield monetary values or prices. But, once again, these are not equilibrium prices: they are affected by distortions, rigidities and imperfections existing in the real economy. Because they are disequilibrium prices, it is not possible to assume that they embody accurate information about scarcity or efficiency. The data they generate may lead to gross misallocation of resources and cannot provide reliable guidance for environment policy-making.

Several valuation techniques involve aggregating individual preferences and estimating demand functions. But aggregation of individual preferences is not a valid procedure: a well-known fact is that the weak axiom of revealed preferences (due to Samuelson 1938) is not valid at the market level. This result was strengthened by the Sonnenschein-Mantel-Debreu theorem we examined before. Strong restrictions are needed to justify the hypothesis that a market demand function has the characteristics of a consumer demand function (Shafer and Sonnenschein 1982). Using valuation techniques that ignore this result will inevitably yield useless or misleading information on the value of "natural capital".

One final consideration is that all of the valuation techniques used in the NC approach rely on a partial equilibrium frame of reference and thus ignore the interdependencies that make price formation so complex and unpredictable. Relying on partial equilibrium to measure "natural capital" leads to invalid results because it rules out income effects, as well as repercussions of disequilibria across markets. In this context, policy-makers will receive misleading information concerning the value and the demand curve for "natural capital". In the rest of this section we examine three more specific problems associated with the valuation techniques used by advocates of the NC metaphor.

<sup>&</sup>lt;sup>13</sup> The many authors that are involved in TEEB-related exercises seem to ignore that the controversy over capital theory is not a simple theoretical discussion that has relevant empirical implications for a discussion on valuation of "natural capital". For example, in a study of thirty-two input-output matrices from an OECD database Han and Schefold (2005) found evidence of reswitching or reverse capital deepening. Estimating production functions for natural capital and its components is a misleading enterprise.

# Reswitching and discount rates

Because the notion of natural capital involves a stream of ecosystem services across time, discount rates are used in calculations involving net present value. Whether one should use discount rates at all in choosing between alternatives in environmental policies is an open and delicate question involving crucial ethical issues (Ackerman and Heinzerling 2004). In addition, the choice of the relevant discount rate is a well-known: selecting a low versus a high discount rate has been discussed in many contexts, from climate change to life insurance.

Furthermore, there is a difficulty that has been neglected in most of the literature and that is highly relevant in the context of our previous analysis on capital theory. Baumol (1997) presents a model to assess a project entailing both economic benefits and environmental costs. Determining the net present value of the investment project and its costs involves "a standard reswitching phenomenon and may, perhaps, represent one of the more persuasive illustrations of the significance of reswitching in practice" (Id.:49)

Baumol's model can be applied *ceteris paribus* to a vast array of cases. It was originally developed by comparing the net costs and benefits of a project in the context of a stream of services. The model involves multiple solutions for a project's internal rate of return. The crucial equations show that at very low interest rates (high discount factors) and at very high interest rates (low discount factors) the project must be rejected. The reason for this is that at very high interest rates only the immediate cost and amenity loss will matter and the project will not be justified. On the other hand, when the interest rate is very low the discount factor approximates unity and "the loss of amenity value for the indefinite future becomes overwhelming" so that the project also fails the cost-benefit test and must be rejected.

Thus, only for intermediate values of the interest rate is the project acceptable. We have here a case of reswitching where multiple solutions exist for the project's rate of return. The project is to be rejected at low interest rates, approved at intermediate interest rates and, then once again, rejected at higher interest rates. And if we relax the assumption that the value of the stream of net benefits and net losses does not change (for example if there is a growing population that demands more of the amenity value), the reswitching phenomenon can become even stronger. According to Baumol (1997: 55)

"The fruitful debate on reswitching offered substantial illumination to capital theory, its original domain. (We) show that analytic tools that played an important role in the reswitching discussion also shed light on other economic issues. (...) This is so because of the reswitching phenomenon, the possibility that both a low and a high discount rate can yield the same present-value figure for a given project."

The consequences of reswitching are as serious for cost benefit analysis as they are for capital theory. It is not possible to have a monotonic ordering of projects as a function of discount rates. This is due to the possibility, for example, that a project that was approved at a given discount factor  $D_1$  but was discarded at a higher discount factor  $D_2$  ( $D_2 > D_1$ ) may return to be approved at a new discount factor  $D_3$  that is even higher ( $D_3 > D_2 > D_1$ ). Discount rates are not an infallible technical tool for the valuation of 'natural capital' and its stream of ecosystem services.

The question of reswitching should not be underestimated. It arises in the context of environmental policy choice and thus is a real possibility in the context of valuation of 'natural capital'. Viscusi and Zeckhauser (1976) discuss the possibility of reswitching while applying a Markovian model to three cases where policy choices need to be made. The first case involves uncertainty as a significant ingredient, while the second deals with situations where there is the possibility of irreversibility. Because of reswitching these authors recognize that "in deterministic policy contexts there may be no unambiguous way to ascertain whether one policy is more future-minded than another" (Ibid: 98):

"Policy A may be preferred to policy B when payoff streams are discounted at rate  $r_1$ , and B preferred to A when  $r_2$  is used. Yet policy A would once more be preferred at rate  $r_3$ , where  $r_3 > r_2 > r_1$ . Situations in which there is a second reversal in project preference will be referred to as instances of 'reswitching'. For such situations, it is not possible to state which of two projects is favoured as the discount rate is lowered and the future is in effect given greater weight."

The problem of reswitching will haunt anyone who believes there is a possibility for putting a monetary value on the components of so-called "natural capital".

# Uncertainty

Uncertainty, we are told, must be taken in a sense radically distinct from the familiar notion of risk (Knight 1964: 19). According to Knight's classic formulation risk is a quantity susceptible of measurement, while uncertainty is essentially unmeasurable. Risk involves knowing the probability distribution of events but in many cases we are unable to calculate probability distributions (in fact, we may even be unable to describe them adequately).

The valuation of so-called natural capital is affected by both risk and uncertainty. In the first case, it is possible to calculate probabilities of potential outcomes. And this information may be thought of relevance in various valuation techniques. However, in the case of uncertainty the impact on valuation is devastating. Although some authors working on valuation techniques do recognize the difference between risk and true uncertainty, the use of these techniques ignores the radical difference between them. Some authors are quite candid about the choice they must make in order to put a "total economic value" on the components of "natural capital". Pascual and Muradian (2010) acknowledge the essential difference but conclude that they will use the term uncertainty "to refer to the one commonly used in economic valuation of the environment, i.e., the conflated risk and uncertainty notion". Of course, while blending these notions makes some problems more tractable, this does not mean that the analysis gains in rigour.

An ideal and well-behaved universe where probability distributions are known may go well with the valuation techniques for 'natural capital'. But it is very different from the real world where uncertainty commands a dominant position. In the real world the dynamic processes that help form new states are led by self-reinforcing or cumulative dynamics, lock-in situations, non-linear developments, irreversibility, recursive loops and complex interdependencies. Radical uncertainty makes contingent valuations of NC worthless.

Estimating risk requires a sample of the universe that is being analysed. We may be able to calculate probabilities for events in horse racing, but we do not have anything comparable for estimating the net present value of a stream of "ecosystem services" that goes into the distant future. It makes sense to recall what Keynes (1937:213-4) had to say about this essential difference between events where probability calculus can be applied and those that belong to the realm of uncertainty.<sup>14</sup>

# Valuation in financial markets

Adherents to the NC approach have repeatedly made references to the similarities between "natural capital" and "financial capital". Thus natural capital is the stock that provides interest for human welfare. In the terms of the Natural Capital declaration (<u>http://www.natural</u> <u>capitaldeclaration.org</u>) "neither of these services, nor the stock of natural capital that provides them, are adequately valued in terms comparable to manufactured and financial capital."

But pricing financial assets is not an easy operation, as traders and regulators of financial markets well know. This is evident in the case of derivatives such as futures, options, interest rate swaps, forward currency contracts and credit default swaps. The bigger the gap between a financial asset such as any of these derivatives and its underlying asset, the riskier adequate pricing becomes. Also, where the opacity of financial assets and/or the complexity of financial innovations increase, the more difficult the task of "price-discovery" becomes in a financial market. It is therefore inaccurate and misleading to affirm that we can go ahead in valuing natural capital in a similar way to financial assets.

Financial markets and institutions have expanded significantly since the early 1970s. Deregulation has accompanied this process and global markets have become increasingly integrated. In addition, financial innovations have augmented the scale of complexity and opacity in many types of transactions. Two good examples of this are securitization and the generalization of over-the-counter transactions with derivatives. Securitization was considered an efficient method to hedge risk as assets were repackaged and sold in the international financial market. In fact, risk diversification does not eliminate risk and OTC transactions increased opacity. In the end, the market freeze that struck financial markets in the midst of the crisis was due to uncertainty about fair asset prices (Easley and O'Hara 2010). Unless one is a firm believer in Fama (1970) and the "efficient market hypothesis" (i.e., that market prices always fully reflect available information) it is clear that financial markets do not offer a good reference for something like valuation of "natural capital" for environmental policy.

In fact, this is the reason why financial markets are marked by greater instability. The global financial crisis that erupted in 2007 is a reminder that the hypothesis of efficiency, rationality and self-regulating markets is inadequate, especially when applied to financial markets. There are multiple signs of this in the banking and financial sectors.<sup>15</sup> Financial instability is at the heart of modern and well-developed financial markets. The methodologies that led to placing

<sup>&</sup>lt;sup>14</sup> As Brady (2012) has shown Keynes made several critical breakthroughs in his *Treatise on Probability* that are relevant in the discussion on valuation of "natural capital". The problems identified by Keynes involve intractable difficulties for any attempt to measure in monetary terms the components of NC and their flow of services.

<sup>&</sup>lt;sup>15</sup> An interesting example for this discussion on natural capital concerns the deregulation of the commodity futures markets in the 1980s and 1990s that culminated with the Commodities Futures Modernization Act of 2000. This led to legalized speculative over the counter trading in derivatives (Stout 2011) that brought about greater instability in commodity markets.

so many bad bets on derivatives in over-the-counter transactions and that led to the 2007 crisis cannot be used to provide adequate prices on "natural capital".

# Concluding remarks

Metaphors and analogies are often used to provide insights on complex phenomena. But using metaphors requires caution. What is gained in ease of communication may be lost in precision. Above all, those who rely on metaphorical thinking should be aware that similarities involved in metaphors are not indicators of identity or of causal relations. Drawing policy conclusions from metaphors can be a risky proposition if their limitations are not well understood.

The proposition that natural capital can be thought of as another form of "capital" is used as a metaphor to try to quantify the value of "natural assets" and of the flow of "ecosystem services". This metaphor is presented by its advocates as having firm bases in economics, especially in the concept of capital in marginalist neoclassical theory. We have shown that the metaphor of "Nature as Capital" does not stem from any scientific enterprise and does not respond to technical imperatives. Our analysis reveals that the metaphor does not have rigorous foundations in economic theory and that it cannot provide adequate economic measurements of what are supposed to be "nature's assets". This may explain why supporters of the NC metaphor have never engaged in a detailed discussion of the concepts and the economic theory they claim to underpin their imagery.

We can now summarize our findings as follows. First, the marginalist theory of capital cannot provide a solid foundation for the natural capital metaphor. Because prices are affected by distribution, rather than the rate of profit depending on the amount of capital, it is the measured quantity of capital that depends on the rate of profit. Therefore, aggregate measures of man-made capital are not as unambiguous as advocates of the 'natural capital' metaphor would like to think.

Second, factor substitution is an essential feature of marginalist theory of capital. It is also a key element of models trying to introduce "natural capital" as a third factor of production. This is especially important in models used to analyse conditions for weak sustainability. However, the possibility of reswitching cancels out any possibility of defining behavioural rules for substitution between man-made and "natural" capital. Reswitching also has negative implications for the definition of the macroeconomic conditions that are required to put an economy on a (weak) sustainability path.

Third, general equilibrium models are equally ill suited to provide a foundation for the natural capital metaphor. Poor results in stability theory have been compounded by the negative implications of the Sonnenschein-Mantel-Debreu theorem. The traditional interpretations of a price formation mechanism simply fall apart under the impact of the SMD theorem. General equilibrium theory cannot provide the underpinnings for adequate valuations of "natural capital".

Fourth, macroeconomic models used to discuss natural capital and weak sustainability bring in a new defect as they rely on representative agents. The aggregative process behind these entities does not allow us to conserve the rationality the theory assumes exists at the level of individual agents and market demand curves do not necessarily have a downward slope. Using an inconsistent theory cannot provide useful information for policy makers on environmental degradation.

Fifth, the valuation techniques used by supporters of the NC approach can only yield current or disequilibrium prices. These prices do not provide correct scarcity signals and cannot guide the allocation of resources. In addition, these valuation techniques are based on a partial equilibrium framework that assumes away all the problems of income effects and market interdependencies. They also ignore the empirical relevance of reverse capital deepening and reswitching when using discount rates. They take no notice of the difficulties in correctly pricing financial products, especially complex derivatives, and they conflate uncertainty and risk. The valuation techniques used in the NC approach do not yield accurate measurements of environmental destruction and may lead to dangerous misallocations of resources.

Proponents of this metaphor believe that the passage from metaphor to the realm of economic analysis is automatic. A short text that exemplifies all of the shortcomings of the natural capital approach is provided by Hughes (2013) who thinks that "...by valuing natural capital in a similar way to financial, manufactured, social and human capital, we can make decisions on stewardship of the natural environment based on hard-nosed economics, and not just on the vitally important moral case for saving nature for nature's sake". We have shown that the NC metaphor is not a useful instrument for environmental conservation or for sustainability and should not be used as a signpost for policy-making. The NC metaphor cannot lead to anything resembling "hard-nosed" economic analysis.

Perhaps the most serious shortcoming of the natural capital metaphor is that it is incapable of providing information on the drivers of environmental degradation. The economic forces that lead to overinvestment, waste and large-scale environmental destruction cannot be analysed through the use of this metaphor. The role of macroeconomic and sector-level policies cannot be understood through this simple-minded discourse. Factors such as the global financial crisis, inequality or the international macroeconomic imbalances that affect the world economy today will remain invisible to followers of the "natural capital" approach. Thinking that the best way to prevent damages to the environment is by "correcting" prices to avoid externalities entails an inadequate vision of theory and real world economics. Serious work on the economic drivers of environmental degradation is urgently required and cannot be based on the flawed metaphor of natural capital.

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# Inferences from regression analysis: are they valid?<sup>1</sup>

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The focus of this paper is regression analysis. Regression analysis forms the core for a family of techniques including path analysis, structural equation modelling, hierarchical linear modelling, and others. Regression analysis is perhaps the most-used quantitative method in the social sciences, most especially in economics and sociology but it has made inroads even in fields like anthropology and history. It forms the principal basis for determining the impact of social policies and, as such, has enormous influence on almost all public policy decisions.

This paper raises fundamental questions about the utility of regression analysis for causal inference. I argue that the conditions necessary for regression analysis to yield valid causal inferences are so far from ever being met or approximated that such inferences are never valid. This dismal conclusion follows clearly from examining these conditions in the context of three widely-studied examples of applied regression analysis: earnings functions, education production functions, and aggregate production functions. Since my field of specialization is the economics of education, I approach each of these examples from that perspective. Nonetheless, I argue that my conclusions are not particular to looking at the impact of education or to these three examples, but that the underlying problems exhibited therein generally hold to be true in making causal inferences from regression analyses about other variables and on other topics.

# **Overall argument**

In some fields, regression analysis is used as an ad hoc empirical exercise for moving beyond simple correlations. Researchers are often interested in the impact of a particular independent variable on a particular dependent variable and use regression analysis as a way of controlling for a few covariates. Despite being common, in many fields such empirical fishing expeditions are frowned upon because the result of particular interest (the coefficient on the key independent variable under examination) will depend on which covariates are selected as "controls".

To the contrary, nowadays, most fields teach that one has to be serious about causal modeling in order to use regression analysis for causal inference. Causal models require certain conditions to hold for regression coefficients to be accurate and unbiased estimates of causal impact. While these conditions are often expressed as properties of regression residuals, they also may be expressed as three necessary conditions for the proper specification of a causal model examining a particular (or set of) dependent variable(s):

• All relevant variables are included in the model;

<sup>&</sup>lt;sup>1</sup> I would like to thank Jim Cobbe and an anonymous reviewer for comments on a draft of this paper. I wish to give a special thanks to Sande Milton for his insights and long-term collaboration with me on this topic.

- All variables are measured properly; and
- The correct functional interrelationships of the variables are specified.

In order to achieve proper specification, one must have a very well elaborated theory that allows one to fulfill these conditions.<sup>2</sup> The fundamental problem with regression analyses is that we do not have sufficiently complete theories in any of our fields to properly specify causal models. Regression analysis application literatures therefore generally become discussions about the degree of misspecification and its consequences. Unfortunately, regression analysis theory is very unforgiving; with just one omitted variable, all regression coefficients may be biased to an unknown extent and in an unknown direction. While researchers sometimes use ad hoc reasoning to infer the direction of bias of particular omitted variables, they do so based on its potential correlation with a particular included independent variable of interest. However, this ad hoc reasoning is not valid. The direction of bias will depend on the intercorrelation of the omitted variable with *all* the included variables. Ad hoc reasoning does not offer a clue as to how biased included coefficients are.

More to the point, we are never talking about the simple case of a single omitted variable. We are faced with multiple failures of all three assumptions: many variables are always omitted, we have little idea of how to best measure the variables we are able to include; and we have hardly any idea of their functional form. This is best illustrated by looking at concrete examples of regression analyses literatures, as I do below.

# Earnings functions

Earnings functions are used principally by economists and sociologists to investigate the determinants of earnings differences. It is probably one of the most-regressed topics of study and has been especially relevant to the economics of education as the source of rate of return to education estimates (Blaug, 1976; Psacharopoulos and Patrinos, 2004). I find earnings functions especially interesting because it is one of the few terrains where social scientists on the left and the right have competed, principally because of arguments about labor market segmentation. In economics, this was about a challenge to the neoclassical idea that there was just one big perfect labor market in which success was determined by your individual human capital characteristics. To the contrary, political economists and other critics of the neoclassical story saw an imperfect labor market with fractures (e.g., divisions into primary and secondary labor markets) and structures (e.g., large firms, unions, sexism, and racism) that greatly influenced whether an individual succeeded. In sociology, this was about a similar challenge to the idea generated from the dominant structural-functionalist theory and its derivative status attainment theory that, like economics, argued that individual success was determined chiefly by individual characteristics. To the contrary, critical sociologists, often sharing a conflict theory critique of structural functionalism, argued, like political economists, that success in the labor market was greatly determined by structural factors. Each side in this debate used regression analysis to "prove" their point of view (Klees and Milton, 1993).

More to the point here, is that there have literally been hundreds of earnings functions studies with each study using anywhere from somewhat to vastly different specifications. The three principal conditions necessary for the regression coefficients of an earnings function to be

<sup>&</sup>lt;sup>2</sup> Statistical techniques for getting around these conditions are discussed briefly in conclusion., The essential problem with them is that they can't deal with rampant misspecification.

accurate estimators of true causal impact are very far from being fulfilled. First, all relevant variables that may affect earnings can never be included. Our theories literally posit dozens of variables, and which variables are included in a particular regression study is again idiosyncratic. Examples of variables that some researchers have considered relevant are: health status, years of schooling, quality of schooling, type of schooling, cognitive ability, race, ethnicity, religion, socioeconomic status, gender, immigration status, marital status, participation in a union, job search, occupation status and differentiation, labor market segment, firm and industry characteristics, and many more. Second, we do not know the "right" way to measure most of these variables. Measurement is ad hoc and varies from study to study. Third, the functional interrelationship between variables is not known. While it is common to use the natural logarithm of income as the dependent variable, even neoclassical economists admit the basis for doing so is very weak and, in actuality, what would be needed is to specify some unknown set of complex simultaneous equations filled with variables subject to complex interactions (Blaug, 1976; Klees and Milton, 1993).

The result of this state of affairs is endless misspecification – by necessity.<sup>3</sup> Each researcher has an almost infinite array of choices in how they specify the earnings function they estimate. Each regression study is never a replication but always different from others in many respects. The upshot is each regression study is idiosyncratic. Since it is relatively easy to get significant coefficients, especially with large data sets, everyone finds their particular variable of interest to be significant. When there is controversy, everyone finds empirical evidence to support their side of the debate. Every segmentation theorist finds labor market segments to be a significant factor in determining earnings and other labor market outcomes, yet no neoclassical economist or structural functionalist sociologist ever does.

With respect to education, most everyone finds some effect of education on earnings, reports it, and sometimes uses it to estimate a rate of return. But alternative specifications always yield different results, and so the estimates are notoriously unstable and inconsistent. Hanushek (1980, p. 240) argued that

"...estimated rates of return for years of schooling particularly in regression estimates [on earnings], considering other individual differences appear very unstable: changes in sample, changes in time periods, changes in precise model specifications yield enormous changes in estimated rates of return."

The estimated impacts of education on earnings and associated rates of return are basically arbitrary, the result of ad hoc empiricism run rampant.

# **Education production functions**

Another very common use of regression analysis is to estimate what are called education production or input-output functions (Levin, 1976; Hanushek, 1986). The dependent variable usually studied is a student's score on some achievement test. The three conditions for proper specification are again impossible to fulfill. First, the array of potential independent variables is huge, including, for example: socioeconomic status, gender, race, ethnicity, age,

<sup>&</sup>lt;sup>3</sup> Imagine you collected data on the earnings of 100 people selected at random. Imagine delineating the literally dozens of factors that would explain why they had different earnings, only one factor being their educational differences. Despite sophisticated statistical techniques, it is simply impossible to accurately separate out the impact of education or any other variable from all other factors.

homework effort, computer use in the home, previous learning, ability, motivation, aspiration, peer characteristics, teacher degree level, teacher practices, teacher ability, teacher experience, class size, school climate, principal characteristics, curriculum policies, to name a few. Second, there is no agreement on how to measure most, if not all, of these variables. Third, again the possible functional interrelationships are innumerable. Contrary to the linear formulation usually run, recursive and simultaneous equation formulations with an array of interaction terms among the independent variables have been posited but little used (Levin, 1976; Hanushek, 1986).

Economists of education, sociologists of education, and other educational researchers have estimated hundreds of these functions. Again, with such an infinite array of specification choices, almost every study is unique and idiosyncratic. Hanushek (2004, 1986, 1979) has, over the long term, studied and summarized the results of such studies. Not surprisingly, he and others have found inconsistent results. However, he and the vast majority of quantitative researchers cling to the hope that improvements in models and data can eventually show some clear results. To the contrary, I see the complete indeterminacy of this form of research built into the very assumptions on which it is based.

A particularly destructive use of these functions is for so-called performance pay for teachers. The "value-added" to student achievement test score by individual teachers are ascertained through estimating an educational production function, usually using only a few control variables, with teacher effects determined by dummy variables or residuals (AERA, 2015). The problem, of course, is that with different control variables different teachers are ranked high or low and there is neither rhyme nor reason to choosing one specification over another. Yet around the U.S. teachers are being hired and fired based on these completely spurious results.

# Aggregate production functions

While many economics of education studies have looked at the impact of education inputs on student achievement and others have focused on the connection between education and earnings, as a proxy for productivity, some studies have tried to look more directly at the connection of education and productivity by looking at the effect of education on economic growth, as measured by GNP. Indeed, some of the earliest work on human capital examined the correlation between levels of education or school enrolments in a country and its GNP (Bowman, 1966; Blaug, 1970). However, correlation is not causation, and these studies were quickly dismissed as neither controlling for other differences between countries nor demonstrating which was cause and which was effect (Blaug, 1970).

The most significant early, and still widely quoted, work that tried to take a more sophisticated look at the connection between education and GNP was by Edward Denison (1961, 1967). Denison focused on a particular form of what economists call an "aggregate production function". Like an earnings function tries to look at all the variables that might affect earnings, production functions look more directly at all the variables that might affect production output in a particular industry. An "aggregate" production function, as the name implies, looks at the effect of inputs on total production output, that is, GNP. This approach, *in theory*, could get around the need to assume earnings reflect productivity by directly looking at the impact of education on output. However, Denison's famous work did *not* do this. Instead of estimating an aggregate production function, it *assumed* one of a particular form and then used

education's association with earnings as the evidence of education's impact on GNP, thus offering nothing different than the results yielded by the problematic education-earnings connection discussed above. Blaug (1970, p. 100) dismissed all this early research: "In short, we learn from international comparisons [of education and GNP]...that we do not learn from international comparisons."

Attempting to connect education directly to GNP generally fell out of favor until the late 1980s and 1990s when a few works in the area of what was called "new growth theory" signaled a broader vision of education's contribution (Romer,1986; Lucas, 1996 Psacharopoulos and Patrinos, 2004).<sup>4</sup> This vision is theoretically interesting in that education is seen not just as contributing to worker productivity but as enhancing growth through a variety of mechanisms and externalities. However, empirically these new directions have proven extremely difficult to model mathematically. Almost every researcher who attempts to estimate these connections therefore uses a different model and the results are, as one would expect, typically idiosyncratic, unstable, and inconsistent (Psacharopoulos and Patrinos, 2004). In 1970, Blaug said the "Mecca of the economics of education lies elsewhere" (p. 100), and I think that holds true today, for reasons similar to the ones I discussed for education and earnings and for educational inputs on outputs.

As I said, the results of the empirical research estimating the impacts above have been idiosyncratic, unstable, and inconsistent. The same is true for the impact of education on GNP for similar reasons. First, there is no agreement on how to measure the stock or flow of human capital in a country. Various proxies have been used but, as Psacharopoulos and Patrinos (2004, pp. 13-14) admit, such measurement may be the "weakest point" of these studies: "Such data have serious intertemporal and inter-country comparability problems, and there are data gaps often filled with constructed data based on interpolations and extrapolations."

Second, more to my general point, as Psacharopoulos and Patrinos (2004, p. 15) also admit: "Countries also differ in many other aspects than those measured by physical and human capital stock..." that can affect GNP. Estimates of aggregate production functions have literally used dozens of different variables as inputs, such as climate, latitude, access to waterways, transportation infrastructure, technological development, investment climate, cultural and political differences, fiscal and monetary policy, etc. (Stevens and Weale, 2004; Hulten and Issakson, 2007; Hulten, 2009).<sup>5</sup> Empirical studies idiosyncratically choose some of these input variables, from those available in the data set being used, and always omit many others. As Psacharopoulos and Patrinos (2004, p. 15) again admit: "These omitted variables can lead to margins of error of hundreds of per cent in accounting for differences in the economic growth path between countries."

Third, it is widely recognized by economists that the linear functional form so commonly used in regression analyses studies is not applicable to aggregate production functions. However, there is considerable debate over what functional form to use and different functional forms yield different estimates of the impact of education (and of all other inputs) on GNP (Stevens and Weale, 2004). There is even a respected school of economics that says that there is no theoretical basis for even believing that an aggregate production function actually exists. Each

<sup>&</sup>lt;sup>4</sup> Sociologists periodically study the determinants of GNP but usually without any pretense of theoretical justification for an aggregate production function (e.g., Kentor, 1998).

<sup>&</sup>lt;sup>5</sup> Tan (2014, p. 426) argues that "social, political, institutional, and cultural factors need to be considered when the impact of education" on economic growth is estimated.

good and service may have a "production function," meaning some mathematical regularity in how inputs like land, labor, capital, and technology combine to produce televisions, yachts, insurance policies, hamburgers, etc. However, since there is no physical process by which aggregate GNP is produced, nor, from this perspective, is there some way to aggregate and measure physical capital, trying to specify an aggregate production function is seen as nonsensical (Cohen and Harcourt, 2003). Guerrien and Gun (2015, p. 100) note that Paul Samuelson, Nobel laureate in economics, pointed out that aggregate production functions wrongly offer "a statistical test of an accounting identity (which is by definition always true)". They argue (p. 99) for the need "to convince everyone to definitively abandon the aggregated [sic] production functions, both in theory and practice" (also see Felibe and McCombie, 2013).

Given these fundamental problems with fulfilling the conditions for regression analysis to yield accurate estimates of causal impact (discussed earlier), it is no wonder that consistent results of the impact of education on GNP are not found. Reviews of this literature report a bewildering array of idiosyncratic methodological choices resulting in a bewildering array of different results (Stevens and Weale, 2004). Patrinos and Psacharopoulos (2004, p. 15) quote Temple and Voth (1998, p. 1359): "[A]ttempting to impose the framework of an aggregate production function is almost certainly the wrong approach for many developing countries." I would say that this is the wrong approach for any country.<sup>6</sup>

It should be noted that almost all these studies only offer some measure of the quantity of education, not its quality. In a widely quoted recent study, Hanushek and Woessmann (2008) try to remedy this by adding country average PISA test scores as a proxy for the quality of education in a country, concluding that a one standard deviation difference in test scores yields a 2 percentage point higher growth rate of GNP/capita. In the light of the foregoing problems, I find this claim completely unreasonable and its uncritical reception due to ignorance of the fundamental problems with human capital theory and empirics discussed in this paper (also see Klees, forthcoming). Hanushek and Woessmann's measures of the quantity and quality of education, choice of other inputs to control for, and choice of functional form are all idiosyncratic.<sup>7</sup>They are only one of literally thousands of reasonable alternative specifications of an aggregate production function. Different specifications will yield different results.<sup>8</sup>

# Discussion

While I have approached the examples above as an economist most interested in the impact of education, the problems are identical in looking at the impact of any of the other myriad

<sup>&</sup>lt;sup>6</sup> Levine and Renelt (1992, p. 942) discuss the "variety of economic policy, political, and institutional factors" that need to be accounted for in GNP regressions: "Given that over 50 variables have been found to be significantly correlated with growth in at least one regression, readers may be uncertain as to the confidence they should place in the findings of any one study...We find that only a few findings can withstand slight alterations in the list of explanatory variables." I would add that real world alterations are far from slight. Also see Sala-i-Martin's (1997) article on the subject, entitled "I just Ran Two Million Regressions."

<sup>&</sup>lt;sup>7</sup> Hanushek and Woessmann (2008) control for only two of the literally dozens of variables that they could have included, and these two variables reflected neoliberal development ideology (openness to trade and security of property rights).Different controls would, of course, yield very different estimates of the impact of the "quality" of education.

<sup>&</sup>lt;sup>8</sup> Hanushek and Woessmann (2015) then take these invalid estimates and make country-by-country projections decades in the future of what GNP would be if PISA results improved. Their results have no validity, depending on literally hundreds of assumptions and completely tenuous causal linkages.

independent variables in these equations. Moreover, as far as I can see, the impossibility of proper specification is true generally in regression analyses across the social sciences, whether we are looking at the factors affecting occupational status, voting behavior, etc. The problem is that as implied by the three conditions for regression analyses to yield accurate, unbiased estimates, you need to investigate a phenomenon that has underlying mathematical regularities – and, moreover, you need to know what they are. Neither seems true. I have no reason to believe that the way in which multiple factors affect earnings, student achievement, and GNP have some underlying mathematical regularity across individuals or countries. More likely, each individual or country has a different function, and one that changes over time. Even if there was some constancy, the processes are so complex that we have no idea of what the function looks like.

Researchers recognize that they do not know the true function and seem to treat, usually implicitly, their results as a good-enough approximation. But there is no basis for the belief that the results of what is run in practice is anything close to the underlying phenomenon, even if there is an underlying phenomenon. This just seems to be wishful thinking. Most regression analysis research doesn't even pay lip service to theoretical regularities. But you can't just regress anything you want and expect the results to approximate reality. And even when researchers take somewhat seriously the need to have an underlying theoretical framework – as they have, at least to some extent, in the examples of studies of earnings, educational achievement, and GNP that I have used to illustrate my argument – they are so far from the conditions necessary for proper specification that one can have no confidence in the validity of the results.

Moreover, what researchers do in practice invalidates their results even further. In theory, when using regression analysis, you are supposed to start with a complete model specification, and then take your data and estimate it, a one-shot deal. Given the indeterminacy of model specification, no one does that in practice. In his now classic article, "Let's Take the Con Out of Econometrics," Leamer (1983, p. 36) describes regression analysis in the real world and its consequences:

"The econometric art as it is practiced at the computer ... involves fitting many, perhaps thousands, of statistical models....This searching for a model is often well-intentioned, but there can be no doubt that such a specification search invalidates the traditional theories of inference. The concepts of unbiasedness, consistency, efficiency, maximum likelihood estimation, in fact, all the concepts of traditional theory utterly lose their meaning by the time an applied researcher pulls from the bramble of computer output the one thorn of a model he likes best, the one he chooses to portray as a rose."

The practical question to me then becomes whether we have learned anything from all this research? Most quantitative researchers would say they have, but I believe that such learning, if examined, would turn out to be from a subset of studies done from a perspective with which the researcher agreed. As Learner (1983, p. 37) put it: "Hardly anyone takes data analyses seriously. Or, perhaps more accurately, hardly anyone takes anyone else's data analyses seriously" (also see Learner, 2010). Hardly anyone ever uses anyone else's specification without "improving" on it, arguing explicitly or implicitly that the previous study was incorrect.

These remarks do not imply that, at least within paradigms, there is no cumulative learning from one another's arguments. Such learning does take place. However, the argument here suggests that regression-based causal inference is simply an excuse for theorizing but does not provide any valid evidence for it. There's an old saw in economics: "If you torture the data long enough, nature will confess." In reality, nature never confesses. Studies from the three examples I have chosen have commanded the attention of educators and policymakers for over 50 years, yet, in reality, I believe that this approach has no validity, providing no reliable, or even approximate, information to help a sensible allocation of societal resources.

Econometricians and other regression analysts do recognize that there are many sources for biases of regression coefficients. They spend a lot of time on ways to correct for things like sample selection bias and measurement error – without much success unless you are willing to make some heroic assumptions. But these problems are minor compared to rampant misspecification. Regression analysts have tried to deal with one misspecification problem – that of omitted variables – through the use of instrumental variables (IVs). But this generally requires accurate measurement of included variables and correct specification of functional form, none of which is ever true. Instrumental variable techniques give different results depending on the IV chosen, as well as have other problems (Heckman and Urzua, 2009; Leamer, 2010). Again, these and other techniques (regression discontinuity, differences-in-differences) require heroic assumptions to deal with any aspect of misspecification (Angrist and Pischke, 2009).<sup>9</sup>

I believe that, unfortunately, regression analysis methodology is a dead end, no better than alchemy and phrenology, and someday people will look back in wonder at how so many intelligent people could convince themselves otherwise. This is not a problem that better modeling, techniques, and data can fix.<sup>10</sup>

# Alternatives

I do not see the essence of the problem as quantification. Nor do I think it futile to try to look for causes and consequences of our practices and policies. Quantifying social phenomena clearly has its limits and, at best, yields approximations (Samoff, 1991). But cross-tabulations and correlations are useful to suggest interrelationships. As is well-known, however, any associations found may be spurious or have a myriad of alternative explanations. For example, crosstabs may reveal that, on average, women earn \$.75 compared to \$1.00 earned by men. We can unpack this some by looking at women and men working full-time, where perhaps the data show a comparison of \$.80 to the \$1.00.We can further look at college-educated women working full-time compared to men in similar circumstances, perhaps giving us a comparison of \$.90 to the \$1.00.Crosstabs can give even finer comparisons. These comparisons, despite limitations, offer real, descriptive, face valid data. Unfortunately, social sciences' hope that we can control simultaneously for a range of factors

<sup>&</sup>lt;sup>9</sup> At best, deviations from proper specification have to be very well-behaved for corrective actions to work – and they never are. Another approach for dealing with misspecification is sensitivity analysis but even the extreme form of this, advocated by Leamer (1983, 2010), that would be needed to account for the extremely wide variations in specification, are unlikely to uncover any robust regularities, as is evident from most empirical literatures (also see McAleer, Pagan, and Volker, 1985, and Saltelli, 2008).

<sup>&</sup>lt;sup>10</sup> I come to the opposite conclusion of Hendry's (1980) article, "Econometrics – Alchemy or Science?" It is interesting to note that Hendry's litany of Keynes' critique of statistical methods parallels my own. As Hendry says: "Keynes came close to asserting that no economic theory is ever testable" (p. 396) (also see Pratten, 2005).

like education, labor force attachment, discrimination, and others is simply more wishful thinking.

The problem is that the causal relations underlying such associations are so complex and so irregular that the mechanical process of regression analysis has no hope of unpacking them. One hope for quantitative researchers who recognize the problems I have discussed is the use of experimentation - with the preferred terminology these days being randomized controlled trials (RCTs). RCTs supposedly get around the issues faced by regression analysis through the use of careful physical, experimental controls instead of statistical ones. The idea is that doing so will let one look at the effect of an individual factor, such as whether a student attended a particular reading program. In order to do this, one randomly assigns students to an experimental group and control group, which, in theory, will allow for firm attribution of cause and effect. Having done this, one hopes that the difference in achievement between the groups is a result of being in the reading program. Unfortunately, it may or may not be. You still have the problem that the social and pedagogical processes are so complex, with so many aspects for which to account, that, along some relevant dimensions, the control and experimental group will not be similar. That is, if you look closely at all potentially relevant factors, control groups almost always turn out systematically different from the experimental group, and the result is we no longer have the ability to make clear inferences. Instead, we need to use some form of statistical analysis to control for differences between the two groups. However, the application of statistical controls becomes an ad hoc exercise, even worse than the causal modeling regression approach. In the latter, at least there is a pretence of developing a complete model of potentially intervening variables whereas with the former a few covariates are selected rather arbitrarily as controls. In the end, one does not know whether to attribute achievement differences to the reading program or other factors (Learner, 2010).

If we are interested in looking at quantitative data, I am afraid we are mostly stuck with arguing from cross-tabulations and correlations. This is a dismal prospect for most quantitative researchers who have spent years becoming virtuosos at data analysis and see the implications of my argument as essentially abandoning the research enterprise. Fortunately, for many of us, the research enterprise is alive and well, with a myriad of more qualitative alternative methodologies with which to investigate our educational and social world.

When I went to graduate school, introduction to research methods courses often focused on regression analysis or on Campbell and Stanley's (1963) examination of the design and analysis of quantitative experimental and quasi-experimental studies. This is still true today within certain fields and university departments. However, the past 30 years has seen a blossoming of alternative approaches to research methods, especially in education, but in other fields as well. Education has been at the forefront of such changes, largely, in my view, because many of the changes were generated within the field of program evaluation which grew, in large part, from the educational evaluations that were mandated by the U.S. Congress in the 1960s and 1970s. Many of those involved in evaluation fieldwork simply found that the quantitative approach to research and evaluation could not capture the experience of the programs they were studying and drew upon other traditions, such as in sociology or anthropology, or invented new approaches. In subsequent years, these forays yielded a wide array of alternative methods for research and evaluation (Mertens, 2015).

For a number of years, I have been fortunate to teach our department's Introduction to Research Methods course. While any grouping of methods is somewhat arbitrary and their labeling always problematic, the course is divided in three, focusing in turn on quantitative/positivist methods, qualitative/ interpretive methods, and critical/transformative methods. There is a large literature on the qualitative/quantitative debate. Some argue too much has been made of it, while others, whom I agree with, argue that there are fundamental theoretical differences in outlook that need to be considered (Smith and Hesushius 1986; Mertens, 2015). Regardless, it is clear that there are lots of qualitative alternatives to quantitative experimental and regression analysis approaches, including: case study, ethnography, grounded theory, phenomenology, narrative, and oral history, to name a few.

Additional methodological alternatives are offered by critical/transformative perspectives which come out of the array of theories in the social sciences and applied fields such as radical political economy, critical sociology, feminisms, queer theory, and others focused on issues of marginalization (Klees, 2008). These perspectives generally criticize the fundamental lack of objectivity of positivist/quantitative research and qualitative/interpretive research, arguing that there is no neutral research, and that too often such studies are done in support of dominant interests. Critical/transformative research takes an explicit position to work in the interests of marginalized people. This includes research under the labels of participatory, action, feminist, indigenous, critical, critical ethnography, and critical race (Denzin et al. 2007; Mertens 2015).<sup>11</sup>

Proponents of quantitative research recognize that some of these alternative methods exist but usually, at best, relegate them to the realm of generating ideas, not to the scientific process of building knowledge of the social world. To the contrary, many proponents of alternative methods argue that they are as or more valid, reliable, and generalizable than quantitative.<sup>12</sup> For example, Miles and Huberman (1994, p. 434) go so far as to argue;

"Qualitative studies...are especially well-suited to finding causal relations; they look directly and longitudinally at the local processes underlying a temporal series of events and states, showing how these led to specific outcomes, and ruling out rival hypotheses. In effect we get inside the black box; we can understand not just that a particular thing happened, but how and why it happened."

Similarly, strong arguments are made for the transferability and generalizability of qualitative and critical research (Donmoyer 1990; Mertens, 2015).

# Conclusions

For a number of years, a sociologist of education colleague and I taught a regression lab course. We used a good national data set and had the students spend the semester running alternative specifications of education production functions. Each class, groups came in and explained their specifications and their results. As expected, different specifications of

<sup>&</sup>lt;sup>11</sup> These classifications are not clear and neat. For example, you can do grounded theory or feminist research from positivist, interpretive, or critical perspectives. To make things more confusing still, critical paradigm researchers may avail themselves of any method, including quantitative ones.

<sup>&</sup>lt;sup>12</sup> Given the problems with the positivist paradigm, there is considerable literature examining alternative criteria for good research for the interpretive and critical paradigms (Mertens, 2015).

included variables, decisions on how to measure variables, and functional forms yielded substantially different results. Each group was asked to explain their results as if they had written them up for a journal article. My colleague used to always comment on a group's explanation of their results with: "That makes sense." And it always did. As do the articles in the literature I reviewed above. We can always make sense of our results and always do. When running regressions, we stop making the many adjustments to our regressions – that always must be made – when we get results that make sense to us. However, taking these literatures as a whole, they simply result in divergent findings, all based on reasonable – at least to some – alternative specifications of their regression equation models.

In conclusion, I wish to say I'd like to be wrong about my argument in this paper. It would be useful if the emperor's not-so-new clothes were more than the nakedness researchers seem to avoid looking at too closely. Unfortunately, theory and practice seem to strongly indicate otherwise. The theoretical conditions for regression analysis to "work" are never close to being met. And, in practice, regression analysis applications seem to result in interminable debates because specifications are so loose that researchers seem to be able to use this family of techniques to "prove" almost anything they want. Nonetheless, while we cannot find the simple cause-effect regularities that regression analysts would like to uncover, at the very least there are still many alternatives methods for investigating our educational and social world.

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# Escaping the Polanyi matrix: the impact of fictitious commodities: money, land, and labor on consumer welfare

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#### Abstract

Karl Polanyi's concept of land, money, and labor as "fictitious commodities" are found in his book The Great Transformation. He pointed out that the attempt to commodify these factors, which he said was necessary for a market economy, would demolish people, business, and nature if some mitigating steps were not taken. The impact of these fictitious commodities will be analysed in the modern market economic context to show their detrimental impact on consumer surplus and welfare. The author introduces the term "Polanyi matrix" to describe the system of unseen rules whereby land, money, and human labor are commodified, are never questioned, and are at the root of many economic problems. The author contends that commodification of land, money, and labor are not necessary for a functioning market economy, and in fact are detrimental to it. For example, land can be placed in trusts, money can be administered as a public utility, and people can reclaim sovereignty over their own labor. The concepts of socially responsible business, green economics, sustainable economics, the creative economy, natural capital, steady-state economics, caring economy, solidarity economy, cooperative economy, or any other suggested solutions, have no chance of succeeding in creating widespread prosperity or sustainability, unless the operating system of the economy can be reformed in these three crucial systemic ways. We can use Polanyi's insights to address the fundamental roots of the problem in commodity land, money, and human labor.

#### Introduction

In his classic 1944 book, The Great Transformation, Karl Polanyi provided the key to understanding how to escape from the current unsustainable economic system. He reviewed the historical transformation from a feudal society to a market society. He noted that it was necessary for all factors of production to be sold in markets in order to make them available for production. However, he made the observation that land, capital, and labor are "fictitious" commodities, since they are not "produced for sale on the market" (Polanyi, p. 72), and therefore their prices are not equilibrated by supply and demand. He pointed out that the attempt to commodify these factors would demolish people, business, and nature if some mitigating steps were not taken. The book documents the various steps undertaken by local communities, feudal landowners, the church, government, and society as a whole to compensate for the negative impacts that took place when these factors were attempted to be put into an unregulated market economy, which he believed could never actually be realized. At the time, results were described by the writer Charles Dickens and others as "satanic mills" of the industrial economy in 18<sup>th</sup> and 19<sup>th</sup> century England. Due to centuries of labor organizing, satanic mills are mostly gone in developed countries, though they still exist in developing countries. What is still with us is the attempt to treat land, money, and human beings in their labor role as commodities. In the neo-liberal dominance and globalization of the last few decades, deregulation of all three factors has taken place to a great extent. What Polanyi thought could never actually be realized has come closer to reality. The treatment of

land and money as commodities for generation of unearned income through asset bubbles has become sacred. "Flexible" labor markets are also promoted in the neo-liberal ideology of our time because they are good for business, but not necessarily for human beings who comprise labor.

In Adam Smith's "invisible hand" theory, developed further by theorists of the firm (microeconomics), firms seeking profits enter existing markets as long as economic profit<sup>32</sup> is greater than or equal to zero. Competition lowers prices and consumers benefit. If economic welfare is equated with consumption, consumer surplus and welfare are maximized. This does not take place for markets in land, money, or labor. Market entry by investors in land and money create asset bubbles and busts, as well as massive debt. Treating human labor as a market commodity results in dehumanization and deprivation through wage slavery and periodic unemployment. In order to compare market commodities with Polanyi's "fictitious commodities" we can use the concept of consumer surplus to analyse benefits or losses to consumers. It is the attempt to commodify land, money, and human labor which is at the root of many economic problems, and this the author is calling the "Polanyi Matrix", the system of unseen rules we live by creating much of the misery and degradation around us. Commodification of land, money, and labor are not necessary for a functioning market economy, and in fact are detrimental to it. For example, land can be placed in trusts, money can be administered as a public utility, and people can reclaim sovereignty over their own labor.

The concepts of socially responsible business, green economics, sustainable economics, the creative economy, natural capital, steady-state economics, caring economy, solidarity economy, cooperative economy, or any other suggested solutions, have no chance of succeeding in creating widespread prosperity or sustainability, unless the operating system of the economy can be reformed in these three crucial systemic ways. We can use Polanyi's insights to address the fundamental roots of the problem in commodity land, money, and human labor. We will start with a discussion of competitive markets for products resulting in consumer benefits, and then analyse each of the fictitious commodities from the standpoint of consumer surplus. Then we will consider remedies for the commodification of land, money, and labor. Next we will look at the impact of the 2008 financial crisis on the three false commodities. Finally we will look at various economic solutions proposed and document how they fall short due to the lack of consideration of Polanyi's insight.

# Microeconomics, consumer surplus, and fictitious commodities

The principle of consumer surplus is associated with neo-classical economics, "laissez-faire", and Adam Smith's "invisible hand". The concept is that individuals expressing their market preferences and maximizing their utility will draw forth resources into production at the lowest possible price. Firms will enter the market and compete for market share until prices are bid down to the lowest level, and normal profit is zero<sup>33</sup>. This will maximize consumer surplus, and therefore consumer welfare because it will provide the greatest amount of products at the lowest possible price, thereby maximizing consumption, which supposedly maximizes human utility. Details can be found in any microeconomics textbook.

 <sup>&</sup>lt;sup>32</sup> Economic profit is defined as revenue minus alternative returns foregone by using the chosen inputs.
 <sup>33</sup> Normal profit is an economic condition occurring when the difference between a firm's total revenue and total cost is equal to zero (<u>http://www.investopedia.com/terms/n/normal\_profit.asp</u>)

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Prerequisites for microeconomics are: perfect competition (free entry and exit), a homogeneous product, factor mobility, and perfect information. There are a large number of firms so each firm is a price taker, and Pareto optimality is enforced (no one is supposedly made worse off). As long as there are economic profits, then firms will enter the market, increase the supply of products, and push prices down until there are no economic profits. This circumstance has a resemblance to reality in the case of some industries, but is completely untrue in the case of the fictitious commodities. Although Polanyi did not discuss consumer surplus, we can use this concept to compare the consumer welfare of products "produced for sale on the market", with the three fictitious commodities as defined by Polanyi.

Figure 1 Increasing consumer surplus with increased supply and subsequent drop in prices



On a chart of consumer surplus, dropping prices are depicted as rising consumer surplus. The supply curve moves to the right from S to S' as manufacturers can produce a larger quantity at a lower price. This drops the equilibrium price from P1 to P2. The consumer surplus, which is the area above the line P1-S moves to the P2-S' line, and therefore consumer surplus is increased by the area of a+b.

Figure 2 Rising Consumption increases utility



When the price of Good B goes down, this is depicted as a change in the budget line (B1 to B3 above), allowing greater consumption of the item, or more money available for other items. Also the utility curve moves to the right (I1, I2, I3), increasing the consumer's utility for every reduction in price.

# The invisible hand and computer chips

We are first looking at items produced for sale on the market, and in particular a competitive market. The conditions for maximizing consumer surplus are approached in some industries. The most obvious one is the microelectronic industry, where Moore's law has prevailed for many decades since first stated in 1965<sup>34</sup>, doubling computing power at the same price every 18-24 months. Competition between Intel, Samsung, Qualcomm, Micron, etc. is fierce, dropping prices, while improving performance. The Top 10 manufacturers in 2013 from Wikipedia with market share are:

1	Intel Corporation	USA	14.8
2	Samsung Electronics	🛤 South Korea	10.5%
3	<u>Qualcomm</u>	USA	5.5%
4	Micron Technology	USA	4.5%
5	<u>SK Hynix</u>	South Korea	4.2%
6	Toshiba Semiconductor	<ul> <li>Japan</li> </ul>	3.9%
7	Texas Instruments	USA	3.6%
8	Broadcom	USA	2.6%
9	STMicroelectronics	France	2.5%
10	Renesas Electronics	Japan	2.5%

No company has a majority of market share, so monopoly is avoided, and the industry remains competitive. Competition may not be perfect but it is substantial, and entry and exit is limited mainly by investment capital. Many of the products are homogenous and interchangeable, for example processors on mother boards. Globalization has resulted in extreme factor mobility as companies move factories and resources around the world for the most favourable location, mainly to reduce labor costs. Information may not be perfect, but the technology for producing microelectronics is widespread. Therefore the pre-requisites for maximizing consumer welfare are present in microelectronics and the facts support it.

Many electronics-based products have declined in price. According to Yahoo finance<sup>35</sup> the following reductions have occurred: televisions (down 77.9 percent); computers (down 88.3 percent); audio equipment (down 39.3 percent); and videocassettes, video discs and other media, including rentals (down 20.4 percent). Over the last decade they also document a 6.6 percent drop in the price of new cars and trucks, 44.4 percent drop in the price of toys, 11

<sup>&</sup>lt;sup>34</sup> Moore, Gordon E. (1965). <u>"Cramming more components onto integrated circuits"</u> (PDF). <u>Electronics</u> <u>Magazine</u>. p. 4. Retrieved 2006-11-11.

<sup>&</sup>lt;sup>35</sup> http://finance.yahoo.com/news/pf article 106259.html

percent drop in clothes, and the cost of a timepiece fell 6.2 percent. Reducing prices result in individuals having greater income to spend on other items, which from a purely consumption standpoint increases their welfare. In these cases the "magic of the market" actually works to create greater consumption and prosperity. Polanyi conceded that even though commodification of labor imposed severe cultural and social costs to workers and their families, it also contributed to economic "improvement" and growth.

# The three false commodities and consumer surplus

# Land and money supply curves

Now we will look at consumer welfare in the case of the three false commodities. In contrast with computer chips, land and money do not fulfil any of the criteria of competitive market goods as there is not free entry and exit, a homogeneous product, factor mobility, or perfect information. Firms may be price makers, not takers, and Pareto optimality cannot be enforced, as they are zero-sum games with winners and losers. They are dominated by economic rents (unearned incomes), rather than economic profits, and when firms enter the market they do not increase the supply of the product and bid the price down. Instead they increase demand for a relatively fixed supply driving prices up, resulting in capital gains until prices crash. This phenomenon has been known back to the days of the "tulip mania" in Holland in 1636.

These items are assets with vertical or near vertical supply curves, subject to bubbles, which reduce consumer surplus and result in less societal welfare. Compare the action of demand on land and money to that of computer chips. Since land or financial products have limited supplies, as demand increases, prices are bid up in a bubble (see figure 3). Consumer surplus is reduced by area a + b, and consumer welfare is therefore decreased. This is true for currency, stocks, bonds, commodities, or any other financial asset since a limited number of shares are available, and for land, since it is fixed in quantity. Prices continue to rise until factor payments and interest crowd out the rest of the economy and asset prices crash.





#### Labor and consumer surplus

In the case of labor, it is an entirely different problem. There is free entry and exit, a relatively homogeneous product, factor mobility, and reasonably good, though not perfect information.

Some explanation is called for. Free entry and exit means that human beings are generally free to enter the labor market at the wage offered if there is a job opening, and free to leave. There are no structural barriers to entering the labor market as long as they have the skills required. Labor is fairly homogenous for any particular job as laborers generally have similar skills within any particular profession or trade. There are certainly outliers, but human beings generally have two arms, two legs, and a brain, so are homogenous for their labor. Good information means that people are free to move to seek higher prices for their labor. Good information means that people are informed about what the job entails and what the wages will be prior to agreeing to supply labor to the labor market. So labor fulfils most of the criteria for market entry by a firm, where we are comparing the supply of a firm's product to a product market with the supply of person's labor to a labor market.

Labor fulfils the criteria of the invisible hand and laissez faire almost perfectly. Competition due to market entry by new laborers does cause the price of labor supply to be bid down, just as market entry by firms increases supply of products and thereby lowers prices. Lowering the price of labor thereby lowers the price of products using labor, and increases consumer surplus. Therefore treating labor as a market commodity increases consumer welfare.

There is only one problem with this equation, which is that the price of labor as a factor of production is simultaneously the wage of labor. Therefore the "magic of the market" in labor results in driving down the income of labor. Labor in their role as consumers desiring low prices, are directly in conflict with labor in their role as workers desiring high wages. Also, in the case of recession due to the collapse of asset bubbles in commodity land and money, massive unemployment results, which would result in homelessness and starvation if not for social or governmental provision of emergency measures such as unemployment insurance, welfare, food stamps, housing, etc. Workers' labor may be for sale, but workers and their families are not. It is labor as a social being that makes the commodity function of labor contrary to its economic function. Perhaps the best illustration of this is WalMart, where the company has perfected the ability to exclude unions, maintain employees at minimal wages, and keep their working hours below what requires the payment of benefits. Walmart has what is called an extremely "flexible" labor market, meaning it is extremely market driven with little government interference. Employees are trained how to obtain government benefits like food stamps and welfare, demonstrating Polanyi's point that unregulated labor markets would cause starvation and deprivation without social measures provided by government, or other non-market institutions. Walmart also follows Henry Ford's famous policy of turning employees into customers, only in reverse. Henry Ford paid his workers high wages so they could afford to buy cars, while Walmart keeps their employees so poor, they can only afford to shop at Walmart.

# The three false commodities in the financial crisis

Fred Block states "The theory of market self-regulation rests on the pretence that the supply and demand for these fictitious commodities will be effectively equilibrated by the price mechanism just as if they were true commodities. But as Polanyi insists: 'in regard to labor, land, and money such a postulate cannot be upheld. To allow the market mechanism to be sole director of the fate of human beings and their natural environment, indeed, even of the amount and use of purchasing power, would result in the demolition of society...no society could stand the effects of such a system of crude fictions even for the shortest stretch of time unless its human and natural substance as well as its business organization was protected against the ravages of this satanic mill" (Polanyi, p. 73).

Buying and selling property and financial assets simply transfers money from one person to another, while inflating asset values, allowing banks to collect greater interest, and contributes nothing to the economy. No good or service is produced. We might call it anti-free enterprise or anti-capitalism because it siphons money from real production to speculation. The entire Finance, Insurance and Real Estate (FIRE) sector is a parasite on the productive part of the economy. In 2008, we nearly had the demolition of the economy, if not society, as Secretary of the Treasury Hank Paulsen warned Congress that without non-market government intervention to the tune of a \$700 billion taxpayer bailout, there would be economic Armageddon. As President Obama told the leaders of Wall St. in 2009, "My administration is the only thing between you and the pitchforks."<sup>36</sup> In order to demonstrate the impact of the three false commodities in the financial crisis we can look at asset prices for land and money, and unemployment for labor during the financial crisis.

# Land as a false commodity

When looking at real estate prices it is important to remember that rising house prices reflect rising land values, not buildings. Buildings depreciate as they wear out. Their replacement price may increase due to the normal inflation rate, and their value may be maintained by investments in repair and maintenance, but buildings do not increase in value on their own. By contrast, land values are what are subject to rising real prices.

There are three important problems with land as a market commodity. First, periodic booms and busts of land prices due to speculation are extremely disruptive of the economy and cause recessions and massive unemployment. The crash of 1929 was due to an asset bubble including real estate, and it is widely understood that the "sub-prime mortgage crisis", i.e.; a land bubble, was behind the global financial crisis in 2008, as real estate prices rose to unheard of levels historically, and then crashed.

Second, land prices rise faster than incomes driving average wages to subsistence due to mortgages and rents crowding out other expenditures by average wage earners. Housing costs rise to unaffordable levels, and households respond by putting both partners to work, increasing working hours, or working multiple jobs so they become trapped by debt peonage. Third, rising land values, or the economic rent of land, are generated by society and not by the individual land owner. Therefore society has the right to recapture it. Allowing some members of society to benefit from social progress and leaving others behind is inequitable. So there is a moral component as well, since landowners accumulate capital gains from land while doing nothing to earn them. "Did you ever consider the full meaning of the significant fact that as progress goes on, as population increases and civilisation develops, the one thing that ever increases in value is land?" (George, 1887)

Furthermore, it is a zero-sum game. For every person who makes unearned gains from land someone else must pay. Rising prices and rent benefit land owners, while renters must pay

<sup>&</sup>lt;sup>36</sup> http://abcnews.go.com/blogs/politics/2009/04/obama-to-banker/

ever increasing rents. These factors sum up the essential problems with the privatization of land rent, and commodification of land.

# Boom and bust

#### Figure 4 Case-Shiller index



Using the 2014 table for the Case-Shiller index for real, inflation-adjusted home prices<sup>37</sup>, we find that the index went from 124.41 in Feb 1, 1997 to 217.31 Dec. 1, 2005. This is an average annual increase in the real price of 6.52% per year, and a total increase of 75% in 8 years. The graph above in figure 4 shows the same trend up to 2012. The repeating booms and busts in real estate, especially the land boom leading to the global financial crisis in 2008, demonstrate the diametric opposite of the invisible hand leading to consumer welfare. In this case rising demand and competition lead to higher prices, rather than lower. Investors attempt to purchase a limited supply of land as the price is rising, in order to receive the "free-lunch" of rising home prices, or capital gains. Those who purchase at the peak and suffer the drop in value, end up "underwater", with their mortgage higher than the property is worth at that time. It may be many years before they recover the purchase price if ever.

Furthermore, this is a worldwide phenomenon as demonstrated by Gavin Putnam in his article entitled, "*From the Subprime to the Terrigenous: Recession Begins At Home*"<sup>38</sup>, revealing the

<sup>&</sup>lt;sup>37</sup> <u>http://www.multpl.com/case-shiller-home-price-index-inflation-adjusted/table?f=m</u>

<sup>&</sup>lt;sup>38</sup> http://blog.lvrg.org.au/2009/06/from-subprime-to-terrigenous-recession.html#terrig.seq

drop in housing prices in 31 countries during 2006-2008. See figure 6. Putnam summarizes the drop in property values followed by a recession in each of these countries in figure 5.



# Figure 5 Fall in property, then recession (from Putnam)


## Figure 6 Fall in property price worldwide<sup>39</sup>

#### Group 1: Parallel recessions

The following table is in chronological order, sorted first by the fall in property values (V), then by the fall in turnover (T or t), then by the onset of recession (R or r).

Year Quarter	1	20 2	05 3	4	1	200	96 3	4	1	200	97 3	4	1	200	98 3	4	2009 1
USA Denmark Ireland Greece Latvia S. Korea Hungary UK New Zealand France Lithuania Estonia Finland S. Africa Luxembourg Australia Spain Sweden Hong Kong Iceland Singapore**			Т		T	=	V T		V V T T	V V T T t	/ v v v v T		R R R V V V V V V V V V V	R R R VI	R R R R R R	r R r	r
Canada Slovenia Belgium Ukraine Czech Rep. Norway Slovakia Russia Bulgaria Malta							t						Т	V V T T T T	V V V V V T T	r R R R V R V r	r R

In figure 7 we compare median monthly income with monthly mortgage payments required for the median house or flat in eight capital cities to test for affordability. This does not include insurance or property taxes. Average (mean) incomes are often skewed upwards due to some very high incomes, therefore median income is a better statistic. The last year of available data is 2011. We will assume a typical home or flat owner finances 95% of the value and calculate the mortgage payment at 5% interest on a 30 year mortgage. We find that people would need to spend 51.9% to 108.8% of their monthly income to pay for a house and 43.3% to 81.4% of their income for a flat (unit). Using the standard limit of 30% of income, houses and flats are unaffordable in every major city in Australia for an individual wage earner. Treating land as a commodity is definitely not helping Australians to own property.

<sup>&</sup>lt;sup>39</sup> <u>http://www.globalpropertyguide.com/real-estate-house-prices/A</u>

House Transfers	Median Prices June 2011	95% mortgage	Monthly mortgage at 5% interest for 30 yrs	2010-11 monthly median income, single wage earner	30% of monthly median income	% of median income needed to pay mortga ge or rent	Monthly Income gap needed to pay mortgage or rent at 30% of income
Sydney	\$595,000	\$565,250	\$3,034	\$2,788	\$837	108.8%	\$2,198
Melbourne	\$501,000	\$475,950	\$2,555	\$3,198	\$959	79.9%	\$1,596
Brisbane	\$442,000	\$419,900	\$2,254	\$3,498	\$1,049	64.4%	\$1,205
Adelaide	\$395,000	\$375,250	\$2,014	\$3,014	\$904	66.8%	\$1,110
Perth	\$485,400	\$461,130	\$2,475	\$3,027	\$908	81.8%	\$1,567
Canberra	\$540,000	\$513,000	\$2,754	\$4,144	\$1,243	66.5%	\$1,511
Darwin	\$500,000	\$475,000	\$2,550	\$3,907	\$1,172	65.3%	\$1,378
Hobart	\$330,000	\$313,500	\$1,680	\$3,236	\$971	51.9%	\$709
Units (flats)	Dec, 2011						
Sydney	\$445,000	\$422,750	\$2,269.41	\$2,788	\$837	81.4%	\$1,433
Melbourne	\$425,000	\$403,750	\$2,167.42	\$3,198	\$959	67.8%	\$1,208
Brisbane	\$375,000	\$356,250	\$1,912.43	\$3,498	\$1,049	54.7%	\$863
Adelaide	\$320,000	\$304,000	\$1,631.94	\$3,014	\$904	54.1%	\$728
Perth	\$395,000	\$375,250	\$2,014.42	\$3,027	\$908	66.5%	\$1,106
Canberra	\$420,000	\$399,000	\$2,141.92	\$4,144	\$1,243	51.7%	\$899
Darwin	\$395,000	\$375,250	\$2,014.42	\$3,907	\$1,172	51.6%	\$842
Hobart	\$275,000	\$261,250	\$1,402.45	\$3,236	\$971	43.3%	\$432

## **Figure 7** Housing affordability in Australian cities<sup>40</sup>

#### **Privatization of rent**

The three causes of commodity land increasing in value are 1) natural features and proximity to amenities; 2) Public investment in infrastructure: transportation, education, police, fire, parks, etc.; 3) Economic development and population growth in the vicinity. So when we look at a real estate bubble, we are really looking at a land bubble. "Flatters, Henderson, and Mieszkowski [1974], and Stieglitz [1977] have shown that in a simple spatial economy, where

<sup>&</sup>lt;sup>40</sup> Median Wages from: ABS 5673055003\_1A, Median Houses ABS 6416.0 and domain.com.au Unit prices from: <u>http://www.genworth.com.au/docs/lender-resource-centre/rp-data-2012-report\_low-res.pdf</u> amortization from: <u>http://www.amortization-calc.com/</u>

the spatial concentration of economic activity is due to a pure local public good and where population size is optimal, aggregate land rents equal expenditure on the pure public good. This result has been dubbed the Henry George Theorem (HGT)..." (Arnott and Stieglitz, 1979, p. 471-471).

There is a moral argument that homeowners do nothing to earn their capital gains, which are all created by society, and therefore society is entitled to the gain through taxation or value recapture. This was a well-known principle historically, although forgotten by most neoclassical economists. The first economists known as the physiocrats in France advocated this principle. Ricardo's principle of economic rent on agricultural land is based on it. Thomas Paine wrote a famous essay to the French government entitled "Agrarian Justice" advocating the government to collect "ground rent". Adam Smith remarked that landlords grow richer in their sleep without working, risking or economizing, and therefore land rent belonged to the public. John Stuart Mill said the "unearned increment" belongs to the public. Cherbuliez, Hilditch, and Proudhon were other advocates. Henry George was the most famous advocate for collecting land taxes. But the desire for unearned capital gains has trumped them all, violating the basic principle of "free markets" that competition will drive down prices and consumers will benefit. Clearly the opposite is true for land. Land markets are anti-capitalist driving prices up and harming consumer welfare.

## Solutions to commodity land

Treating land as a market commodity destroys consumer welfare, and creates boom-bust economic cycles. For a sustainable economy, land must be removed as a commodity leading to asset bubbles. There are several ways this could be done. Municipalisation, taxation on economic rent of land, and community land trusts. In Singapore, Hong Kong, and formerly in Canberra, Australia all land is owned by the city and leased out on long term contracts such as 99 year leases. Covenants restricting profit on resale would be necessary to avoid turning land into a commodity.

Arnott and Stieglitz sum up another solution to the land problem as follows: "...since a confiscatory tax on land rents is not only efficient, it is also the 'single tax' necessary to finance the pure public good" (Arnott and Stieglitz, 1979, p. 471-471). If holding costs are less than the annual capital gains, then financiers will continue to speculate on land and housing. If land tax or capital gains taxes removed the unearned income from real estate, then land would no longer be subject to speculation or bubbles.

Another viable method of removing land from the market is to place land in community land trusts, where land is owned by a non-profit organization, and is leased to homeowners who own the buildings. Limited or shared-return contracts on homes prevent homeowners from capitalizing land prices into their house prices when sold. They are most often used for affordable housing, but can also be used for commercial, industrial, or agricultural uses.

These three methods essentially remove land from speculative markets, and remove the distortionary impact of land from markets of all goods and services containing a land component.

## Money as a false commodity

"...the market administration of purchasing power would periodically liquidate business enterprise, for shortages and surfeits of money would prove as disastrous to business as floods and droughts in primitive society." (Polanyi, 1944, p. 73).

The administration of purchasing power, in other words creation of the money supply, is currently regulated by quasi-private central banks such as the US Federal Reserve Bank, but is mainly controlled by private banks. According to a recent publication by the Bank of England, private banks create 97% of the money supply through the issuance of loans to borrowers<sup>41</sup>. "This confirms that banks create money when they grant a loan: they invent a fictitious customer deposit, which the central bank and all users of our monetary system, consider to be 'money', indistinguishable from 'real' deposits not newly invented by the banks. Thus banks do not just grant credit, they create credit, and simultaneously they create money" (Warner, 2014, p.74). "Central banks increase money supply by purchasing government bonds with money created for that purpose" (Farley et al, 2013), so-called monetizing the debt. This central bank money is often called "vertical money", while money created by the banking system is called "horizontal money". So the entire money supply is essentially administered by the market as Polanyi stated as a requirement of a market society. The money supply is very flexible as determined by demand for credit, but every dollar creates interest payments to banks, which increases the cost of items purchased such as homes, and decreases consumer welfare.

Market administration of the money supply and other financial products also violates the theory of consumer welfare, the invisible hand, and the magic of the market. Greater demand for currencies, credit, and financial products, rather than leading to market entry and lower prices, leads to rising asset prices until the bubble bursts. Hyman Minsky called the end stage of financial capital the "Ponzi stage", where there are no more productive investments available to be made in the economy, so finance goes mainly into speculative activities, driving up asset prices (Minsky, 1992, p. 9). While asset prices are rising, banks are willing to extend rising amounts of credit, increasing the money supply, but when asset prices are falling banks call in loans and reduce lending, thereby shrinking the money supply. This is referred to as pro-cyclical monetary policy, which exacerbates recessions and unemployment due to shrinking demand in times of falling asset values. What is needed is counter-cyclical monetary policy. Some control is exerted by central banks through adjustment of reserve rates, interest rates and open market operations, but they don't directly control the money supply.

Since monetary policy is out of the hands of governments, in order to address monetary contraction they often respond with Keynesian expansionary fiscal policies to address monetary contraction. That is the only tool they have at their disposal. By additional spending or reduction of taxes, governments are able to inject more money into the economy, and counteract some of the pro-cyclical trends of bank money. This goes directly against the principle of market administration of purchasing power, and is an example of one of the mitigating responses to pure neo-liberal, laissez-faire policies that Polanyi devotes his entire book to explaining. "Grave evils would be produced in this fashion unless the tendencies

<sup>&</sup>lt;sup>41</sup> <u>http://www.bankofengland.co.uk/publications/Documents/quarterlybulletin/2014/qb14q1prerelease</u> moneycreation.pdf

inherent in market institutions were checked by conscious social direction made effective through legislation" (Polanyi, 1944, p. 129).

There is also a biophysical limitation to the infinite emission of credit by private banks. All bank credit is issued with interest compounded continuously or annually as a condition of loans. Therefore, the money supply must continually expand and the economy must grow in order to obtain the funds to pay back interest on the entire money supply. This creates a perpetual growth imperative, which has resulted in the 2014 ecological footprint measured at 50% overshoot of the planet's biocapacity<sup>42</sup>. "Robbed of the protective covering of social institutions...Nature would be reduced to its elements, neighborhoods and landscapes defiled, rivers polluted, military safety jeopardized, the power to produce food and raw materials destroyed (Polanyi 1944, p. 73). And thus it is today as planetary overshoot has already exceeded boundaries for biodiversity and the nitrogen cycle (Rockström 2009, p. 472), and the climate destabilizes from excess greenhouse gasses.

Marion King (M.K.) Hubbert identified the basic problem with our current debt and interest based monetary system in his essay entitled, "Exponential Growth as a Transient Phenomenon in Human History".

#### Figure 8 Hubbert growth curves



Three types of growth (Hubbert, 1974b, fig. 1]

"The lower curve represents the rise, culmination, and decline in the production rate of any non-renewable resource such as the fossil fuels, or the ores of metals...The third (upper) curve is simply the mathematical curve of exponential growth. No physical quantity can follow this curve for more than a brief period of time. However, the sum of money, being of a non-physical nature and growing according to the rules of compound interest at a fixed interest rate, can follow that curve indefinitely." Since the entire economy is based on fossil fuel extraction, the growth curve of oil must track the growth curve of the money supply at compound interest. However, conventional world oil peaked in 2005, and more expensive, harder to extract, unconventional oil is nearing its peak as well. Hubbert predicted a "cultural crisis" when the curve for oil and curve for money diverged, which could be argued happened in 2008 when the GFC occurred. Debt has increased worldwide in order to continue the

<sup>&</sup>lt;sup>42</sup> <u>http://www.footprintnetwork.org/en/index.php/GFN/page/world\_footprint/</u>

pursuit of growth in the real economy, but in actuality these debts can never be paid. Many countries have total debt/GDP ratios over 100% including Japan which is approaching 400%.

## Fictitious money in the financial crisis



## Figure 9 Dow-Jones Average 2002-2013

The value of stocks on the Dow-Jones Industrial Average fell in half during the financial crisis in 2008. The recovery in stock value since then has been due mainly to the huge influx of money to banks from the Federal Reserve's quantitative easing program, and the repurchase of their own stocks by corporations. It does not reflect the real value of productive enterprises. Due to the commodity nature of money, the value of stocks are disconnected from the real world. The value of stocks is primarily determined by the availability of credit to purchase them, as quantitative easing to banks simply provided additional resource for them to invest in financial assets and drive up their prices.



## Figure 10 Price of gold in dollars<sup>43</sup>

Dow Jones Industrial Average Last 10 Years http://www.macrotrends.org/1358/dow-jones-industrial-average-last-10-years

<sup>43</sup> http://goldprice.org/gold-price-chart.html

The value of money itself can be measured in foreign exchange rates or the value of currency in gold or other commodities, which fluctuate widely, and has no relationship to a manufactured commodity. The value of the US dollar in gold fluctuates widely, and reflects the flood and droughts of money that Polanyi referred to.

Over time, due to our debt-interest based money system, the value of money declines, making products more expensive. This perpetual decline in the value of money has resulted in one dollar in 2014 being worth the equivalent of four cents in 1913 (bls.gov), a total depreciation in 100 years of 25 times its value, or -3.16% per year compounded annually. The most stable currency in the world at the time, the Deutch Mark was worth 20 cents (pfennigs) in 2001, based on a 1950 starting point of one Deutch Mark<sup>44</sup>. This constant reduction in the value of money due to the debt-interest money system, results in perpetual price inflation, which is accounted for in economic models as an annual rate of 2-3% inflation. For consumer welfare prices should be going down, not up. Constantly depreciating currency results in prices always rising and labor needing to constantly sell itself for higher wages on labor markets.

Since land is usually appreciating faster than wages, this creates a treadmill for the average worker trying to keep up with constant consumer price and land price inflation. If the value of money was constant, then competition in real commodities would decrease prices benefitting consumers. In reality economists look at deflation with horror as it decreases demand and results in recession, and makes loans harder to repay due to increasing value of money compared to income. Since nearly the entire money supply is issued through commercial loans, it would create default and monetary crisis if there was constant price deflation. By using the concept of "real" prices adjusted for monetary inflation, we can disaggregate the portion of prices due to increased product costs, and the portion due to currency depreciation. Since nominal prices of microelectronics have declined without adjusting for "real" prices, this is an even more impressive achievement. It means that the prices of microchips have declined faster than prices have increased due to currency deflation. So if prices are inflating at 3.16% per year due to currency depreciation, that means that the price of microchips are declining in real terms faster than 3.16% per year.

## Solutions to commodity money

The fact that 97% of the money supply is created by interest bearing loans is the crux of the problem. It adds the cost of interest to everything, and causes a growth imperative due to the need for economic expansion to provide the money for interest payments. Many economists and writers over the years have recommended 100% reserve requirements, most recently in an IMF working paper by Jaromir Benes and Michael Kumhof, called "The Chicago Plan revisited". This refers to the plan by U of Chicago economists led by Frank Knight in the 1930's and also supported by Irving Fisher and Henry Simons, to require 100% reserve requirements on all bank loans, which would transfer to government the function of creating the money supply through 100% vertical money, ideally interest free, by spending it on public goods. Lincoln's Greenbacks were a successful example of interest-free government money. Greenbacks were created interest-free to pay Civil War soldier's wages. With 100% reserves banks would just become intermediaries between savers and borrowers.

<sup>&</sup>lt;sup>44</sup> <u>http://margritkennedy.de/media/pre\_moneypres\_56.pdf</u>

Several intermediate steps have been proposed. The JAK bank in Sweden issues loans interest free which would decrease the perpetual growth imperative, and perpetual inflation. However, the JAK Bank lends out only money it has on hand and does not add to the money supply. Coincidentally, JAK stands for Jord, Arbete, Kapital in Swedish - or Land, Labour, Capital in English. Public Banks such as the Bank of North Dakota have been proposed as a way to transfer some seigniorage to the public sector. Banks are able to create credit while states are prohibited by the US Constitution. The Bank of North Dakota receives all deposits of state revenues, and uses them to leverage many loan programs for agriculture, industry, commerce, and student loans. Even more decentralized would be a system of municipal public banks creating credit for their infrastructure needs, at minimal interest, and paid back by tax money, with revenue going to the municipality instead of to banks. A voluntary decentralized approach is also possible. Around 70-80% of all bank loans in the US are for mortgages. If non-profit financial institutions were formed to take on the financing of nonmarket land in community land trusts, then we could begin to escape the Polanyi matrix. These banks could establish credit on the basis of the JAK bank which takes equity in properties instead of interest, and can begin to remove the use of money as an extractive commodity.

For the problem of monetary speculation, a financial speculation tax or Tobin Tax has been proposed worldwide as a means to reduce the level of speculation in financial assets, but would not change the creation of money. To eliminate the commodity nature of money, the creation of the money supply would have to be transferred to government, and maintained at a stable price level.

## Labor as a false commodity in the financial crisis

The third of Polanyi's false commodities, labor, is a special case. As factors of production, people are no longer human beings, but are commodities bought and sold in labor markets, so called wage slaves, or "human capital". Without unionization or government intervention, labor is generally at a disadvantage to the owners of business, except in the case of highly skilled labor, as labor is normally overabundant, and easily reproducible by a very enjoyable process. In Polanyi's words, "Robbed of the protective covering of cultural institutions, human beings would perish from the effects of social exposure; they would die as the victims of acute social dislocation through vice, perversion, crime, and starvation (Polanyi, p. 73). As a factor of production, it is in the interest of business to minimize wages to labor. As consumers, laborers benefit from low prices resulting from reduced labor costs, which act directly against their interests as employees seeking higher wages.

There are several specific results of labor as a market commodity. During bust cycles, labor is hit with unemployment, leading to many of the social consequences pointed out by Polanyi. Due to globalization, labor has not been compensated for its increased productivity contribution since 1975 in the US. Also, as a result of treating labor as a market commodity, Polanyi predicted starvation and crime would result, without social intervention. We can evaluate these results in light of the 2008 financial crisis.

## **Boom bust cycles**





Sources: U.S. Bureau of Labor Statistics, Haver Analytics, National Bureau of Economic Research

As shown in the graph above the unemployment rate during the financial crisis rose from 4-9.1%. Many analysts feel that this number is grossly understated, as US unemployment figures do not include discouraged workers, who have used up unemployment benefits, and who are now most likely living on welfare and food stamps.

As proof that unemployment statistics are not accurate, analysts point to the labor force participation rate (figure 12), which cannot hide discouraged workers, and note that it is at its lowest level since 1978 when far fewer women were in the workforce as a percentage of the population. The explanation given by US officials is the retirement of "baby-boomers" and their withdrawal from the workforce, but that is blatantly false as the over-55 age category is the only one where the workforce has not declined, as seen in figure 13.

So we can see the impact of the financial crisis on labor, seven years after it began. When unemployment results, such as from the financial crisis, human beings will starve without other means of support. So the idea that labor should be managed only subject to the price mechanism in "flexible" labor markets is untenable as Polanyi contended.

<sup>&</sup>lt;sup>45</sup> <u>https://www.frbatlanta.org/cenfis/publications/notesfromthevault/1110.aspx</u>





Figure 13 Over 55 labor participation rate<sup>47</sup>



http://www.zerohedge.com/news/2014-01-10/people-not-labor-force-soar-record-918-million-partici 46 pation-rate-plunges-1978-level <sup>47</sup> https://research.stlouisfed.org/fred2/series/LNS11324230

# Wages not commensurate with labor productivity

Figure 14 Productivity and wages<sup>48</sup>

# Since 1979, productivity has risen eight times faster than pay

Disconnect between productivity and typical worker's compensation, 1948–2013



Since 1975 workers have received almost none of the gains of increased productivity, which has increased by 143% since around 1975 (figure 14). In other words, productivity has more than doubled, while workers received none of the gains. This can be explained by the deindustrialization of the US economy, as heavy industries followed by manufacturing in general were exported to Asia. Due to this trend there was a huge decrease in unionization which went from 39% in 1940 to around 10% in 2014. During the same period there was a trend toward part-time work and contract labor, mergers and acquisitions, with downsizing and layoffs. The Reagan revolution and Republican "Contract with America" both served to remove power from the working class and transfer it to corporations. One of Reagan's first acts as President was to break the Pilots and Air Traffic Controllers strike (PATCO), replacing them all with military personnel. That was the final nail in the union coffin. The Democratic Party in 1992 through the Democratic Leadership Conference chose to seek the same corporate and Wall St. money as the Republicans, and from that point on effectively stopped serving the working class. All these factors led to the reduction of bargaining power and political power on the part of labor, and can help explain the stagnating real wages during this period of time.

<sup>&</sup>lt;sup>48</sup> <u>http://www.epi.org/productivity-pay-gap/</u>

## Crime and starvation

Subject to an unregulated free market for labor, Polanyi believed that "workers would die as the victims of acute social dislocation through vice, perversion, crime, and starvation". The US has the most "flexible" labor market of all OECD countries, meaning the freest market for labor, with the least intervention by government or social institutions, as defined by Polanyi. "Essentially, to get high ratings, a country must have low marginal tax rates, a low minimum wage, a high degree of flexibility in hiring and firing, a small amount of centralized collective bargaining, and low unemployment benefits" (Lawson, Robert A. & Bierhanzl, 2004 p. 122). Advocates of laissez-faire "free market" policies believe that the threat of starvation will motivate people to seek employment. The number of people on food assistance reached an all-time high after the 2008 financial crisis, and currently in the U.S. around one of six people in the country (14% as of Jan 13, 2015, US Dept. Ag<sup>49</sup>), and one in five of children (US Census Bureau, Jan. 28, 2015)<sup>50</sup> are on the Supplemental Nutritional Assistance Program (SNAP), formerly known as food stamps. Polanyi's claim that without mitigating social institutions, an unregulated free market in labor would result in starvation is proven to be true. Without SNAP these people would starve.

Figure 15 Incarceration rate<sup>51</sup>

#### FIGURE 6.

#### Incarceration Rates in OECD Countries

With an incarceration rate of 710 inmates per 100,000 residents, the United States stands in stark contrast to the typical incarceration rate of 115 among OECD nations.



Sources: Glaze and Herberman 2013; Walmisley 2013; authors' calculations. Note: All incarceration rates are from 2013, with the exception of the rates for Canada, Greece, Israel, the Netherlands, Sweden, Switzerland, and the United States; of these countries, all rates are from 2012, with the exception of Canada, whose rate is from 2011–12. The incarceration rate for the United Kingdom is a weighted average of England and Wales, Northern Ireland, and Scotland. For more details, see the technical appendix.

HAMILTON BROOKINGS

<sup>&</sup>lt;sup>49</sup> <u>http://www.statisticbrain.com/food-stamp-statistics/</u>

<sup>&</sup>lt;sup>50</sup> http://www.census.gov/newsroom/press-releases/2015/cb15-16.html

<sup>&</sup>lt;sup>51</sup> http://www.brookings.edu/~/media/multimedia/interactives/2014/10 facts crime/crimeFig6.png



Figure 16 Increase in incarceration with neo-liberal era<sup>52</sup>

The United States has 5% of the world population and 25% of the prison inmates. The incarceration rate is more than double all other OECD countries. It is noteworthy that the era of neo-liberal supremacy began with the election of Ronald Reagan in 1980, and accelerated with the collapse of the Soviet Union in 1991. Policies in the US became more consistent with unregulated markets than ever before, in accord with Margaret Thatcher's claim that "There is No Alternative" (TINA), to the "Washington Consensus" of privatization, liberalization, free trade, free movement of capital, structural adjustment, and all the other policies promoted by market fundamentalists since 1980. There was an inflection point in 1980 when US incarceration rates began to increase drastically (figure 16). Before that time the US was in line with other OECD countries. This increased incarceration was not a result of an increase in violent crime as the violent crime rate dropped during this same period. Although it cannot be directly blamed on flexible labor markets, this increase in incarceration rate is consistent with Polanyi's claim that treating labor as a market commodity will result in "crime and starvation".

#### Solutions to commodity labor

One of the responses to critiques such as Polanyi's of labor as a market commodity was Marx's prescription of a "dictatorship of the proletariat", and state ownership of the "means of production". It turns out that one dictatorship is no better than another. Also owning the means of production does not necessarily eliminate land or money as commodities, although presumably putting labor in charge of managing industrial production would give them more

<sup>&</sup>lt;sup>52</sup> <u>http://staticd.discourse.net.s3.amazonaws.com/site/wp-content/uploads/2013/01/US\_incarceration\_timeline-clean-fixed-timescale.svg\_-400x267.png</u>

sovereignty over their work lives. In reality during Soviet communism, laborers remained commodities ruled by party elites. More recently the Mondragon cooperatives have demonstrated a more cooperative form of labor management, still within the market system, but with good results. In the US Louis Kelso originated the idea of Employee Stock Ownership Plans (ESOPs), which would ideally turn all employees into capitalists by giving them a share of stock in the company. This has had limited success. Many ideas for returning power to workers have been proposed in recent years. Community land trusts often employ development and construction companies for housing construction and renovation. Therefore, combining community land trusts with worker-owned construction companies is feasible. Gar Alperovitz has promoted many structural reforms including, "the traditional radical principle that the ownership of capital should be subject to democratic control" (Alperovitz, 2013). This refers to worker ownership or participation in their own workplaces, a very different proposition than state communism or state capitalism. Democratizing the workplace is a great unfinished business of society.

What few reformers have advocated directly is to remove labor as a factor of production sold in labor markets. It is probably a lack of imagination that prevents us from imagining an economy where labor consists of human beings doing meaningful work in alignment with their skills and interests. Aboriginal and tribal people managed to do it, through non-simultaneous reciprocity. Even in feudal times according to Polanyi, labor was tied to feudal estates and was remunerated according to social relationships, not according to labor markets. Surely we can find an approach embodying something like Sen's capabilities approach that respects the humanity of labor, while still remunerating them for their work. We need to find a way for laborers to gain control of their lives and work according to their capabilities, instead of simply selling their labor in markets.

#### Evaluating proposed economic reforms

There are many current proposals for a "new economics" that will supposedly solve our economic problems. These include Corporate Social Responsibility (CSR)<sup>53</sup>, Green Economics<sup>54</sup>, Sustainable Economics<sup>55</sup>, Natural Capital<sup>56</sup>, Creative Economy<sup>57</sup>, Caring Economy<sup>58</sup>, and Solidarity Economy<sup>59</sup>. These are non-solutions since they don't address the three false commodities comprehensively, or at all in most cases. By contrast, Herman Daly has proposed the following ten policies for a steady state economy<sup>60</sup> which begin to address the Polanyi matrix:

- 1. Cap-Auction-trade
- 2. Ecological Taxes
- 3. Min/Max Income
- 4. Flexible work time
- 5. Trade Regulation
- 6. Reformed WTO

<sup>&</sup>lt;sup>53</sup> <u>https://www.ceres.org/</u>

<sup>&</sup>lt;sup>54</sup> <u>http://www.wri.org/blog/2011/04/qa-what-green-economy-0</u>

<sup>55</sup> http://www.sustainable.org/economy

<sup>&</sup>lt;sup>56</sup> http://www.naturalcapitalproject.org/

<sup>57</sup> http://creativeconomy.britishcouncil.org/

<sup>58</sup> http://caringeconomy.org/

<sup>&</sup>lt;sup>59</sup> <u>http://socialeconomy.itcilo.org/en</u>

<sup>60</sup> http://steadystate.org/top-10-policies-for-a-steady-state-economy/

- 7. 100% reserve banking
- 8. Public Trusts
- 9. Stable Population
- 10. Reformed GDP

These tenets come the closest to addressing all three false commodities. Cap-auction-trade and ecological taxes put prices on environmental costs and operate within the market framework. Public Trusts for natural resources remove them from the market and could create a non-market mechanism for use of land and resources. 100% reserve requirements, we have already noted, removes the power of banks to create the money supply as a commodity for their profit, and returns the function to government, which it could use as a public utility for the greater good. Min/max income, flexible work time, trade regulation, and reformed GDP could all help to address the commodity nature of labor sold in labor markets. It doesn't create an alternative mechanism for human beings to actualize their potential and receive a livelihood without selling their labor, but it points in that direction.

## Conclusion

Commodity land, money, and labor remain a largely unseen matrix as they have been part of the market economy since "The Great Transformation" from a feudal to market society. Market competition raises the price of land and money through increased demand for fixed assets, rather than lowering it through increasing supply as in the case of microchips or other competitive product. Commodity labor in flexible labor markets normally results in wages being driven down due to the oversupply of labor. Labor is also subject to periodic unemployment and loss of income during recessions resulting from booms and busts in commodity land and financial products. Therefore commodifying land, money, and labor reduces consumer surplus, and lowers economic welfare. It is entirely unnecessary, as all three can be managed outside of markets. None of the major proposals for economic reform address the three false commodities identified by Polanyi, except for Daly. Combining solutions developed in this article and Daly, by implementing 100% reserve requirements and public banks, community land trusts, and housing companies set up with worker ownership and management, all three fictitious commodities could be addressed. Since 70-80% of commercial loans are for real estate, this would comprise a huge portion of commodity money, land, and labor. Only by addressing the Polanyi matrix can the fundamental problems of inequality, poverty, environmental destruction, boom-bust monetary cycles, exorbitant housing prices, and many other problems be solved.

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# The petition against the Smoot-Hawley Tariff Bill: what 1,028 economists overlooked

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#### Abstract

On May 30, 1930, Swathmore economics professor Clair Wilcox presented President Herbert Hoover with a petition, signed by 1,028 U.S. economists, denouncing the proposed Smoot- Hawley Tariff Bill. Citing a panoply of reasons, they strongly urged him to veto the Bill. Hoover ignored their plea, signing the bill into law on June 17, 1930. This paper examines this important rite of passage of the U.S. economics profession. In short, it marked the beginning of a collective conscience among U.S. economists. It will be argued, however, that it also highlights an important intellectual disconnect between the government and the economics profession in favor of the former. More specifically, it will be shown that for untold reasons, the economics profession had overlooked both the underlying rationale of, and the solution of proposed the Smoot-Hawley Tariff Bill. In short, the profession saw it as a tariff act like all others, while the government, notably the Republican Party saw it as a stop-gap measure aimed at dealing with the growing problem of electrification-based overproduction and excess capacity that ironically constituted the cornerstone of Paul Douglas' and other signatories' thinking and writings at the time.

JEL Codes N12, N42, N62

Keywords Smoot-Hawley Tariff Act, U.S. economists, petition

#### 1. Introduction

As Frank Whitson Fetter pointed out in 1942, the economists' statement in opposition to the Hawley- Smoot tariff bill was a unique document for a number of reasons (Fetter 1942). For one, no pronouncement by American economists had ever attracted the public attention that this received. More importantly, it signaled the unanimous opposition of economists and the economics profession to the tariff bill pending in Congress. Put differently, the economics profession spoke with one voice. But more importantly, it marked the coming of age of the U.S. economics profession in policy matters, one that would be accentuated with the rise of Keynesian macroeconomics and the various ensuing debates (fiscal versus monetary policy).

The petition has aged well and has gone on to inspire other similar efforts aimed at influencing the trade debate. For example, in 2007 a petition of economists against protectionism was organized by the Club for Growth. According to the Editors of the Econ Journal Watch, it was inspired by the 1930 petition, and like it, gathered 1,028 signatures (Econ Journal Watch, 2007).

In this paper, we argue that while it represented a triumph of sorts for the U.S. economics profession, the petition was fundamentally flawed, owing to a disconnect between the underlying principles of the Bill as seen by the architects and sponsors of the Bill, and the 1,028 originators and signatories. In short, it will be argued that the Smoot-Hawley Tariff Act and, to a lesser degree, the Fordney-McCumber Tariff Act of 1922 represented a new

generation of tariff bill, one whose goals were macroeconomic- and growth-related, and not revenue – and/or sectorally – related as had been the case in the past. Ranking Republican and Chairman of the Senate Finance Committee, Senator Reed Smoot was adamant: rising unemployment owed to rising imports and the introduction of mass production techniques.<sup>61</sup> Higher across-the-board tariffs would secure a larger share of the U.S. domestic market for U.S. firms, thus increasing employment, output, profits and hence overall welfare. Throughout the election campaign and ensuing legislative debate, the Republicans were unwavering: higher tariffs for everyone would bring/restore prosperity.

For reasons that will be discussed, the 1,028 signatories overlooked this, and treated the Bill like any and all others. The ultimate irony, however, lies in the fact that the principles upon which the Bill was based (rising excess capacity due to technological change) were shared by Paul Douglas, Rexford G. Tugwell and to a lesser degree, Irving Fisher, all of whom had written extensively on the effects of mass production on America's ability to produce wealth.

The paper is organized as follows. To begin with, we present the petition in its entirety, along with the names of its originators. We then examine the underlying rationale for the Bill according to the Republicans. This is then followed by a discussion of what the 1,028 economists thought the Republicans were doing. Lastly, we examine the possible reasons for this disconnect.

## 2. The Republican Party and the 1,028 economists: a fundamental disconnect

The petition read:

"We are convinced that increased restrictive duties would be mistake. They would operate, in general, to increase the prices which domestic consumers would have to pay. By raising prices they would encourage concerns with higher costs to undertake production, thus compelling the consumer to subsidize waste and inefficiency in industry. At the same time they would force his to pay higher rates of profit to established firms which enjoyed lower production costs. A higher level of duties, such as is contemplated by the Smoot- Hawley bill, would therefore raise the cost of living and injure the great majority of our citizens.

Few people could hope to gain from such a change. Miners, construction, transportation and public utility workers, professional people and those employed in banks, hotels, newspaper offices, in the wholesale and retail trades and scores of other occupations would clearly lose, since they produce no products which could be specially favored by tariff barriers. The vast majority of farmers would also lose. Their cotton, pork, lard and wheat are

<sup>&</sup>lt;sup>61</sup> There is no evidence that imports of either manufactures and/or food had increased. It is our belief that Smoot resorted to this tried and true strategy, largely for political effect. Most ranking Republicans alluded to mass production and growing excess capacity.

export crops and are sold in the world market. They cannot benefit, therefore, from any tariff which is imposed upon basic commodities which they produce.

They would lose through the increased duties on manufactured goods, however, and in a double fashion. First, as consumers, they would have to pay still higher prices for the products, made of textiles, chemicals, iron and steel, which they buy. Second, as producers their ability to sell their products would be further restricted by the barriers placed in the way of foreigners who wished to sell manufactured goods to us.

Our export trade, in general, would suffer. Countries cannot permanently buy from us unless they are permitted to sell to us, and the more we restrict the importation of goods from them by means ever higher tariffs, the more we reduce the possibility of our exporting to them. This applies to such exporting industries as copper, automobiles, agricultural machinery, typewriters and the like fully as much as it does to farming. The difficulties of these industries are likely to be increased still further if we pass a higher tariff.

There are already many evidences that such action would inevitably provoke other countries to pay us back in kind by levying retaliatory duties against our goods. There are few more ironical spectacles than that of the American government as it seeks, on the one hand, to promote exports through the activity of the Bureau of Foreign and Domestic Commerce, while on the other hand, by increasing tariffs it makes exportation ever more difficult.

We do not believe that American manufacturers in general need higher tariffs. The report of the President's Committee on Recent Economic Changes has shown that industrial efficiency as increased, that costs have fallen, that profits have grown with amazing rapidity since the end of the World War. Already our factories supply our people with over 96 per cent of the manufactured goods which they consume, and our producers look to foreign markets to absorb the increasing output of their machines. Further, barriers to trade will serve them not well, but ill.

Many of our citizens have invested their money in foreign enterprises. The Department of Commerce has estimated that such investments entirely aside from war debts amounted to between 12,555,000,000 and 14,555,000,000 on Jan. 1, 1929. These investors, too, would suffer if restrictive duties were to be increased since such action would make it still more difficult for their foreign debtors to pay them the interest due them.

America is now facing the problem of unemployment. The proponents of higher tariffs claim that an increase in rates will give work to the idle. This is not true. We cannot increase employment by restricting trade. American industry in the present crisis might well be spared the burden of adjusting itself to higher schedules of duties. Finally, we would urge our government to consider the bitterness which a policy of higher tariffs would inevitably inject into our international relations. The United States was ably represented at the world economic conference which was held under the auspices of the League of Nations in 1927. This conference adopted a resolution announcing that the time has come to put an end to the increase in tariffs and move in the opposite direction.

The higher duties proposed in our pending legislation violate the spirit of this agreement and plainly invite other nations to compete with us in raising further barriers to trade. A tariff war does not furnish good soil for the growth of world peace.

## Originators and First Signatories

Paul H. Douglas, Professor of Industrial Relations, University of Chicago. Irving Fisher, Professor of Economics, Yale University. Frank D. Graham, Professor of Economics, Princeton University. (Trade theorist) Ernest M. Patterson, Professor of Economics, University of Pennsylvania. Henry R. Seager, Professor of Economics, Columbia University. (Student of Patten, UPenn) Frank W. Taussig, Professor of Economics, Harvard University (Trade theorist) Clair Wilcox, Associate Professor of Economics, Swarthmore College."

## 2.1 Smoot-Hawley: a new generation of tariff policy

As was the case then, today much controversy continues to surround tariff policy in the Hoover era. The quintessential question is: what prompted the Republican Party to introduce a second upward tariff revision, six years after the prohibitive Fordney-McCumber Tariff Act of 1922. For example, Irwin (2011) attributes the Smoot-Hawley Tariff Bill to a combination of weak agricultural prices and political strategy (e.g. ploy) on the part of the party. Beaudreau (2014) on the other hand points to the growing problem of excess capacity in East-Coast manufacturing, which prompted Reed Smoot and Joseph Grundy, two ranking Republican Party leaders and policy stalwarts, to propose higher tariffs as a means of securing a greater share of the U.S. domestic market for U.S. firms. Higher tariffs on agricultural products, he argued, were thrown in for good political measure, especially in light of the farmer's plight and former President Calvin Coolidge's vetoing (2X) of the highly popular McNary-Haugen Farm Relief bill. This section draws extensively on his work, chronicling the debate over the proposed upwards tariff revision.

Beaudreau (2014) puts the genesis of the Bill in the developments in late-1927/early-1928, specifically in growing unemployment. Consider, for example, the following remarks made by Senator Smoot in the Senate, in response to claims by Democrats that unemployment was on the rise in 1927.

"Senator Smoot insisted that the picture drawn by the Democrats on Monday, when the Senate passed the Senate resolution, was much overdrawn. He admitted that some unemployment existed, but insisted that it did not compare with that of 1920 and 1921 when the Republicans came into power after eight years of Democratic administration. As for one reason for a degree of unemployment, Senator Smoot referred to large importations of foreign merchandise that have been steadily reaching American shores in spite of the Republican protective tariff... These imports have a tendency to supplant large quantities of American goods, despite the tariff, thus slowing down many American industries. There also was an over-supply or over-production in many lines, Senator Smoot contended, and over-production or underconsumption in the textiles industries. A slow-down of many industries helps to increase industrial unemployment, and the result is immediately felt in the lowering of the consuming power of the wage earners. This has brought about what may be called an oversupply or overproduction existing in many lines; and we might add that mass production has cut a great figure in the amount of production in the United States in special lines" (*New York Times*, March 8, 1928, p.1).

According to him, the U.S. economy in 1928 was characterized by oversupply, the chief culprits being higher imports and mass production. This was a recurrent theme in the debate over the proposed Smoot-Hawley Tariff Bill. For example, at Hearings in the House of Representatives on the proposed tariff bill in February 1929, over-production was a recurrent theme. The *New York Times* reported: "Most of the petitioners for large basic industries have admitted states of over- production or over-capacity for meeting domestic demand. Some estimated excess facilities at as much as 25 percent" (*New York Times*, February 17, 1929, p.1).

Calls for higher tariffs on manufactures came from the industrialized North-East. Leading the charge was Joseph A. Grundy, President of the Pennsylvania Manufactures Association and a long- time Republican. Grundy played an instrumental role in Hoover's victory at the 1928 Republican National Convention in Kansas City. According to Harold U. Faulkner: "The Smoot-Hawley Tariff was an administrative measure put through the party machine and no single person was more active than Joseph R. Grundy, president of The Pennsylvania Manufacturers Association, who became Senator in December 1929" (1950, 342). His political agenda was limited to one item: a general upward tariff revision including manufactures.

Unfortunately, there is no record of the rate hikes Grundy had in mind. Were they to rise by 10 percent, 20 percent, or 100 percent? When the Hawley Tariff Bill was put before the House in early 1998, Grundy was unhappy with the proposed rate revisions. The *New York Times* reported:

"The dissatisfaction in highly protected industry because the bill does not increase rates on manufactured products is apparent from a statement of Joseph R. Grundy, president of the Pennsylvania Manufacturers' Association. Mr. Grundy had an interview with President Hoover not long ago, and while he would not comment on what took place behind the doors of the President's office, the impression was created that he had yielded to Mr. Hoover's desire that he should not insist on higher duties on industrial products than the bill was then expected to provide. Today Mr. Grundy said: 'The few rises that are in the bill fall short of meeting the requirements, which the past seven years of Pennsylvania's industries show, along the lines indicated in the Republican platform adopted at Kansas City" (*New York Times*, May 9, 1929, p.3).

Other leading tariff protagonists included Pennsylvania Governor John S. Fisher and Samuel M. Vauclain, president of the Baldwin Locomotive Works of Philadelphia. On September 5, 1929, in a meeting with President Hoover, Fisher expressed his concerns over increasing pressure to amend the tariff bill.

"Earlier in the day President Hoover heard Representative Albert Johnson of Washington vigorously oppose the Senate Tariff bill, while two others, Governor Fisher of Pennsylvania and John E. Edgerton of New York, president of the National Manufactures Association, voiced protests against administrative features of the bill. Governor Fisher said that the American valuation plan was essential to a sound tariff bill and that protection could not be given to one group alone, but must be extended to the entire country. 'During the campaign we preached protection for the East, West and all parts of the country,' Governor Fisher said. 'We in Pennsylvania are for a tariff that will afford protection for all of our industries. We expect agricultural protection, but we are not going to stand for recognition of any section to the disadvantage of another'" (*New York Times*, September 6, 1929, p.1).

In his January 1928 address to The Chicago Association of Credit Men, Vauclain pointed out that high wages and high tariffs were essential for the preservation of prosperity in America.

"Wages should not be governed solely by supply and demand, he asserted, but should be placed at a level which would enable workers to buy the necessities of life. It is the wage-earner who constitutes the great majority of our population, he said. These people are the spenders of the nation and upon their ability to spend freely the general business of our country depends. Foreign importations should be avoided by all, he said. We may profit individually by buying foreign goods at less than American manufacturers can produce, but the injurious consequences to general business more than offset the selfish gain, he declares. A protective tariff is necessary if we are to have full dinner pails for our boys during 1928 and the years to come, he insisted" (*New York Times*, January 24, 1928, p.3).

This brings us to the question of why? Why did the Party see a need to raise tariffs on manufactures. Beaudreau (1996, 2014) attributed the over-production and excess supply referred to by Senator Reed Smoot and other Republicans, to electrification which he shows contributed to tractorization and motorization of the U.S. economy (Bresnahan and Trajtenberg 1995, Helpman and Trajtenberg). Drawing from a number of sources, he presented estimates of excess capacity in manufacturing in the 20-25 percent range. What is particularly noteworthy is the fact that the ensuing tractorization and motorization of the farm and transport sector contributed/exacerbated the agricultural surpluses that followed the end of WWI. Fossil fuels had all but displaced agriculture-based feedstocks (hay and oats) in transport.

America's new greater capacity to generate wealth and need for more control over the domestic market through the use of tariffs was raised by Secretary of Labor, John J. Davis at the 1928 Kansas City Republican convention. For example, on June 11, he highlighted the successes and impending dangers facing U.S. industry.

"Summing it all up, the protective tariff, limited immigration, exclusion of child labor, general watchfulness of women in industry, the eight-hour day, collective bargaining coupled with conciliation and arbitration of injunction, are the important things in which the American laboring man is very much interested. Industrial competition among the countries of the world has caused fundamental changes in American industry that have vastly increased output and at the same time, relatively decreased the cost of production in practically all lines of endeavor. Thus, in meeting the competition from countries were lower standards of living obtain, the mechanization of industry has been brought about a practical industrial revolution in our country. The American workers are the highest paid in the world; the American standard of living surpasses that of any country; but even with this enviable record of progress, the mechanization of industry and the development of rapid power machinery processes have displaced many veteran workers and others, necessitating their engaging in other activities. To maintain high wages, it is absolutely necessary to have a high protective tariff, a tariff that protects" (Washington Post, June 12, 1928, p.4).

The role of over-production and excess capacity in the thinking of Reed Smoot and hence in the drafting of the SHTA can be seen from the following quote taken from Merton Merrill's biography:

On his return to Utah in August 1932, in preparation for his final battle in political life, Smoot advised his people that it had been the common attitude in 1930 to attribute the depression to unwise governmental policies, with the Smoot-Hawley act specified. Lest there were some obsessed with heresy, he declared, "To hold the American tariff policy, or any other policy of our government, responsible for this gigantic deflationary move is only to display one's ignorance of its sweeping universal character." He found that "The world is paying for its ruthless destruction of life and property in the World War and for its failure to adjust purchasing power to productive capacity during the industrial revolution of the decade following the war." (Merrill, 1990, 340)

At the Kansas City convention, Ranking Republican Charles E. Hughes praised the merits of an upward tariff revision against a background of greater efficiency, lower costs and greater output.

"I shall not review at any length the results of the Republican tariff policy. Mr Hoover did that in his speech at Boston. Let me recall to you what he said. 'Every argument urged by our opponents against the increased duties in the Republican tariff act has been refuted by actual experience. It was contended that our costs of production would increase. Their prophecy was wrong for our costs have decreased. They urged that the duties which we proposed would increase the price of manufactured goods; yet prices have steadily decreased. It was urged that, by removing the pressure of competition of foreign goods, our industry would fall in efficiency. The answer to that is found in our vastly increased production per man in every branch of industry, which indeed is the envy of our competitors"(*New York Times,* October 24, 1928, p.5).

The key point is in the last sentence where Hughes invokes the "vastly increased production per man in every branch of industry". Put differently, higher tariffs, by increasing domestic firms' market share, would allow firms to "slide" down their new, lower average cost curve. Greater market share would lower costs and ultimately, prices.

This not only confirms Faulkner's view that the SHTA was an administrative measure put through the party machine, but provides a rationale, namely over-production. Ranking Republicans and party officials were unanimous in their desire to see a major upward revision in tariffs, thus limiting imports and providing the American producer with a larger market. Furthermore, it dovetailed with the long-held Republican belief in the "protective tariff to be a fundamental and essential principle of the economic life of this nation" (*New York Times*, June 24, 1928, p.13). In the text of the Republican Platform adopted by the Kansas City Convention, the tariff figures prominently, mostly as a tool of stabilization policy and overall economic growth.

"We reaffirm our belief in the protective tariff as a fundamental and essential principle of the economic life of this nation. While certain provisions of the present law require revision in light of changes in the world competitive situation since its enactment, the record of the United States since 1922 clearly shows that the fundamental protective principle of the law has been fully justified. It has stimulated the development of our natural resources, provided fuller employment at higher wages through the promotion of industrial activity, assured thereby the continuance of the farmer's major market and further raised the standard of living and general comfort and wellbeing of our people. The great expansion in wealth of our nation during the past fifty years and particularly in the last decade could not have been accomplished without a protective tariff system designed to promote the vital interests of all classes" (*New York Times,* June 15, 1928, p.8).

Joseph R. Grundy, President of the Manufacturers Association	"We therefore advocate a general revision of the tariff upward, that every section of the country with all labor in every industry and business may reap the benefit and enjoy profitable prosperity" ( <i>New York Times</i> , June 11, 1928, p.1).
James J. Davis, Secretary of Labor	"An utmost economic call for increased duties and that the present unemployment was due to low tariff schedules which permit cheaply made goods to come in and cripple American industry" (New York Times, June 11, 1928, p.1).
William M. Butler, Chairman of the Republican National Committee	"has favored revision of the tariff for some time and while he is not impressing his personality on the platform drafters, it is believed that his views and those of others in the East will be taken into consideration. The compromise suggestion that the farm group offered for increased duties on farm-products and the decreasing of the free list has been met by the manufacturing interests with a willingness to cooperate with the farmers provided the industrial schedules are also considered favorably" ( <i>New York Times</i> , June 11, 1928, p.1).
S.D. Fess, Senator from Ohio	"even should the convention decide that there should not be a general revision of the tariff favored in the platform, any tinkering with the tariff, such as the change in the farm schedules, inevitably would lead to a general revision."
George H. Moses, Permanent Chairman of the Republican National Conference	"In seeking for an economic policy for the United States, we know that our people will not turn to the party which clings to the fetich of free trade. In seeking for a policy to make the tariff effective for every interest in the United States, we know our people will not turn to the party whose strength and weakness alike lie in its sectional character" (Congressional Record, May 29, 1928.p. 10624).

## **Table 1** Kansas City Republican Party tariff positions

Table 1 presents a compendium of tariff positions/opinions expressed by leading members of the Republican Party at the Kansas City convention.

## 2.2 What the 1,028 economists thought the Republicans were saying and doing

It is our view that the various rationales invoked in the petition were orthogonal to the concerns raised by Ranking Republicans, and therefore, were orthogonal to the goals and aspirations of the Bill itself. Perhaps the best example of this is found in the last paragraph, where the question of unemployment is raised. Specifically, it states:

"America is now facing the problem of unemployment. The proponents of higher tariffs claim that an increase in rates will give work to the idle. This is not true. We cannot increase employment by restricting trade. American industry in the present crisis might well be spared the burden of adjusting itself to higher schedules of duties."

As shown, the unemployment that led to the SHTA was in 1927/1928, not in 1930 after the Stock Market Crash. As Reed Smoot had pointed out, the growing unemployment at the time (1927) owed to growing imports and mass production. Hence, rather than being cyclical-based, the unemployment in question was growth-based and was attributed to technological change.

## 2.3 The question of higher prices

This question illustrates well the disconnect between the Republican Party and the 1,028 signatories, and between the Republican Party and the economics profession in general. In keeping with general trade theory, a tariff in general results in a higher price, as domestic producers increase output along the convex section of their marginal cost curve. The Smoot-Hawley Tariff Bill, by increasing tariffs on virtually all goods, would raise the cost of living.

This stands in stark contrast with opinions expressed by ranking Republicans who argued that higher tariffs, by securing a larger share of the U.S. market for domestic firms, would result in lower prices as firms traveled down their non-convex average cost curve. In many regards, they anticipated, by roughly half a century, the economies of scale, product differentiation literature of the1980s, where firms operate on the downward sloping section of the average cost curve. Surprisingly, not one of the originators or signatories acknowledged nor understood this argument, preferring to revert to standard trade results.

## 2.4 The plight of the farmers

The plight of the farmers is a question that is closely tied to the question of tariffs and prices. According to the petition, the Smoot-Hawley Tariff Bill would be harmful to farmers as it would increase the price of manufactures, and not offer any real relief for agricultural markets. Most observers agreed that because the U.S. imported little-to-no hay and cereals (wheat, barley, oats), higher tariffs on agricultural product would have little effect on farm revenue. As such, the debate focused on the question of prices for non-agricultural goods. These included the goods farmers consumed directly as well as the goods which served as inputs into agriculture.

The 1,028 signatories were of the view that the proposed higher tariffs would increase prices of manufactures and farm inputs, which would be prejudicial to farmers. Hence, in keeping with the Haugen-McNary Farm Relief Bill, lower, not higher tariffs would be beneficial to farmers. Not surprising, the Republicans argued the contrary, for the reasons raised above. In short, higher tariffs would be beneficial to farmers as they would result in lower, not higher prices.

#### 2.5 Reciprocity and international relations

One of the more interesting aspects of the SHTA was its insular nature. In short, ranking Republicans had chosen to play a Nash strategy with the Rest of the World (ROW), despite growing unrest abroad over the proposed upward tariff revision. Could they not foresee the consequences of their actions? The 1,028 economists were adamant: yet another round of tariff hikes would most certainly result in a tariff war, especially with Europe. There are, we believe, a number of factors that could explain their seemingly irrational behavior. The first is a combination of a strong desire on the part of the Republican party to act, combined with a dearth of knowledge and policy instruments. They sensed there was a problem, but had no clue as to the cause, nor of what constituted a proven solution. As Reed Smoot pointed out in his biography, the problem was the failure to "adjust purchasing power to capacity." The second is the past as prologue. The prohibitive tariff hikes contained in the Fordney-McCumber Tariff Act of 1922 were met with little in the way of reactions on the part of European countries. Perhaps this owed to the fact that they had a political debt towards the

U.S. for having helped them defeat Germany in World War I. The third was the changing nature of international trade. The 1910s and 1920s witnessed the emergence of the horizontal multinational firm, mostly American, which localized production facilities in foreign markets, thus rendering them immune to trade sanctions. For example, France could not slap a tariff on General Electric products as most of them were being produced in France or Europe in general. In short, the U.S. exported modernity via foreign direct investment, rendering conventional tariff policy ineffective.

## 3. Connections/disconnections

Thus far, we have shown the existence of a fundamental disconnect between the Republican Party and the economics profession. One could go as far as to argue that the two were speaking a different language, a different economics language. However, we maintain that this is not a completely accurate characterization of the state of the economics profession in the 1920s, and of the very thoughts/beliefs of many of the signatories. In this section, we show that paradoxically, many of the latter held views/positions similar to the Republican Party. For example, the chief author of the petition, University of Chicago professor Paul Douglas also maintained that the U.S. economy in the 1920s was characterized by significant excess capacity.

## 3.1 Paul Douglas and excess capacity

The chief author and instigator of the petition, Paul Douglas was a professor of industrial relations at the University of Chicago. Throughout his career, he devoted much time and effort to the question of wages and productivity, specifically to the relationship between the two. In short, he felt that while productivity had increased markedly in the 1920s, wages had lagged behind, making for a disequilibrium. For example, in a paper entitled: "The Modern Technique of Mass Production and Its Relation to Wages," published in 1927, he referred to the 29 percent increase in the volume of physical production from 1919 to 1926.

Year	Fed. Reserve	Day- Thomas	Harvard
1010	100	100	100
1920	102		106
1921	79	80	82
1922	103		110
1923	120	122	130
1924	112		120
1925	125		133
1926	129		

## TABLE 2. INDEXES OF PRODUCTION IN MANUFACTURING 1010-1026 (1010-100)

Douglas was primarily concerned with the widening wage-productivity gap, specifically with the apparent failure of wages to track productivity. These concerns are also found in the writings of another signatory of the petition, Columbia university economics professor Rexford G. Tugwell, who like Douglas described the "revolution underway in U.S. industry." Foremost among the "technical causes" of increased productivity, he argued, was "the bringing into use of new and better power resources more suited to our technique, more flexible and less wasteful; and continued progress in the technique of generating and applying power" (Tugwell 1927, 180) – in short, the electrification of U.S. industry.

"The electrification of industry has now progressed to the extent of between 55 and 60 per cent completion. So widespread an adoption of this new flexible means of moving things cannot have taken place without numerous secondary results in lowered costs, improvements in quality, and a heightened morale among workers. For the new power is not only cheaper to use; it is also cleaner, more silent and handier. On the whole, the electrification of industry must be set down as the greatest single cause of the new industrial revolution" (Tugwell 1927, 182).

## 3.2 Irving Fisher and electrification

Irving Fisher, like Paul Douglas and Rexford Tugwell, felt that changes in technique had served to increase vastly America's capacity to produce, echoing the views of the ranking members of the Republican Party. On the day after the crash (October 24, 1929), he declared:

"The stock market rose after the war above the pre-war level by 50-100 percent because of war inflation and that since, it has doubled because of increasing prosperity from less unstable money, new mergers, new scientific management and the new policy of waste saving."



CHART 15.—Total primary factory power applied through electric motors increased, 1919-1927, from 55 per cent to 78 per cent. Inset: 50 per cent purchased, 28 per cent made locally.

Source: Fisher (1930).

## In The Stock Market Crash and After, published in 1930, he was more explicit:

"But after 1919, something happened. The implications of which are not yet sufficiently gauged. It was of enough significance to cause President Hoover's Committee on Recent Economic Changes to remark that 'acceleration rather than structural change is the key to an understanding of our recent economic developments.' The committee added: 'But the breadth and the tempo of recent developments gives them new importance.' What happened was indicated by the fact that in the United States, 8.3 million workers produced in 1925 one quarter more than 9 million workers turned out in 1919. The new indexes of the Federal Reserve Board measuring production record this gratifying advance which reflects an increase in the American standard of living... The general volume of production had increased between 1919 and 1927 by 46.5 percent; primary power by 22 percent, and primary power per wage earner by 30.9 percent (between 1919 and 1925), and productivity per wage worker by 53.5 percent between 1919 and 1927" (Fisher, 1930, 120).

However, unlike Tugwell and Douglas, he stopped short of invoking this manifold increase in productivity as the cause of the structural weakness in the 1927-1928 period.

## 3.3 How did they miss it?

Our findings raise an important question, namely how and why did 1,028 leading U.S. economists misunderstand and misinterpret the well-known and well-publicized intentions of the Republican Party in its proposed Smoot-Hawley Tariff Bill? As we have shown, many of its guiding principles can be found in the writings of the petition's instigators and signatories. We submit two reasons, namely incomplete information and the state of flux in which economics as a whole found itself in, in the 1920s. By incomplete information, it should be understood the failure of the instigators to fully comprehend and appreciate the economic reasoning/logic behind the Bill. For example, if Paul Douglas had followed the electoral and legislative debate over the proposed tariff bill, from late 1927 on, he would have discovered a set of principles that were largely consistent with his own work at the time. Ibid for Rexford G. Tugwell.

Second, the idea/theory that the U.S. economy was "operating" significantly below capacity in the 1920s was a new one. Most subscribed to Say's Law, according to which supply creates its own demand. Hence, the many references to unused capacity and mass production-based unemployment were orthogonal to the dominant ideology at the time. Combined with what was an incomplete knowledge and appreciation of the Bill's stated goals, it comes as little surprise that 1,028 leading U.S. economists misunderstood and misinterpreted the intentions of the Republican Party in its quest to raise tariffs in 1928.

## 4. A more appropriate petition

With the benefit of hindsight, it is clear that the 1,028 instigators and signatories to the petition had failed to appreciate the subtleties of the act, making for a situation in which its bases

were orthogonal to those of the actual petition. This then raises the following counterfactual, namely had this not been the case (i.e. had they been aware of the intentions and underlying principles), what would their petition have resembled. To this end, we drafted what we believe would have been a more appropriate petition, which follows:

"Owing to the genius of its entrepreneurs, engineers and scientists, the U.S. has benefited from a manifold increase in its ability to generate wealth and so increase the standard of living. Unfortunately, for reasons that are not fully understood at the present moment, markets (income and expenditure) have not followed suit, resulting in a situation of generalized excess capacity, especially in manufacturing and agriculture.

The Republican Party is currently proposing yet another upward tariff revision in the hope of securing a greater share of the domestic market for U.S. firms, arguing that higher utilization rates will decrease prices and hence benefit consumers. Unfortunately, such a policy, while appealing to the layman, is based on faulty logic and cannot bring long-run prosperity to the U.S. Its basic flaw is to assume that our trading partners will not react. While Europe did not react to Fordney-McCumber Tariff Act of 1922, owing no doubt to the goodwill from our involvement in the Great War, we believe that it will react, closing markets to U.S. exports. U.S. firms will be no further ahead, as imports and exports contract.

Beggar-thy-neighbor policies have failed and will continue to fail in the long run, and the proposed Smoot-Hawley Tariff Bill is an example of such a policy. We believe that the Republican Party should examine other options to firm up markets, thus allowing the country to exploit its newly-found potential."

## 5. Conclusions

In this paper, we have argued that the Smoot-Hawley Tariff Act of 1930 represented a new generation of tariff bill, one whose objectives were macroeconomic and growth enhancing in scope. Throughout its electoral and legislative history, the Bill sought to correct the growing disparity between America's ability to produce wealth and its ability to take goods off of the market. According to its architects, it would raise firms' sales, profits and dividends. Moreover, it would do so at significantly lower prices as firms would "slide down" their new downward sloping average cost curves. While farmers' incomes would be no higher, their real incomes would stand to rise as the price of manufactures and farming inputs would fall as a result.

As we have shown, the 1,028 originators and signatories of the petition overlooked most – if not all – of this. To them, the Smoot-Hawley Tariff Bill was just another of a long-line of tariff bills with sectoral or revenue-based goals/ambitions. Ironically, many of those who opposed the bill ultimately shared its underlying principles. This led us to the conclusion that the 1,028 economists had overlooked the essence of the debate leading up to and including the final passage of the Bill.

In short, the Smoot-Hawley episode is a case in which the thinking, or policy making, was ahead of the theory. The issues that Reed Smoot and other members of the Republican Party were grappling with were absent from mainstream economics, making for the disconnect identified here. As it turned out, this disconnect would go on to characterize the second policy attempt at closing the gap between actual and potential GDP, namely the National Industrial Recovery Act. And once again, the profession condemned the Act.

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# When the model becomes the message – a critique of Rodrik

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## Introduction

Economics is perhaps more than any other social science model-oriented (see Morgan, 2012; Arnsperger and Varoufakis, 2006). There are many reasons for this – the history of the discipline, having ideals coming from the natural sciences (especially physics), the search for universality (explaining as much as possible with as little as possible), rigour, precision, etc.

Many mainstream economists want to explain social phenomena, structures and patterns, based on the assumption that the agents are acting in an optimizing (rational) way to satisfy given, stable and well-defined goals.

The procedure is analytical. The whole is broken down into its constituent parts so as to be able to explain (reduce) the aggregate (macro) as the result of interaction of its parts (micro).

Building their economic models, modern mainstream (neoclassical) economists ground their models on a set of **core assumptions (CA)** – describing the agents as "rational" actors – and a set of **auxiliary assumptions (AA).** Together **CA** and **AA** make up what I will call the **ur-model (M)** of all mainstream neoclassical economic models. Based on these two sets of assumptions, they try to explain and predict both individual (micro) and – most importantly – social phenomena (macro).

The core assumptions typically consist of:

CA<sub>1</sub> Completeness – rational actors are able to compare different alternatives and decide which one(s) he prefers

CA<sub>2</sub> Transitivity – if the actor prefers A to B, and B to C, he must also prefer A to C.

CA<sub>3</sub> Non-satiation - more is preferred to less.

CA<sub>4</sub> *Maximizing expected utility* – in choice situations under risk (calculable uncertainty) the actor maximizes expected utility.

CA<sub>5</sub> Consistent efficiency equilibria – the actions of different individuals are consistent, and the interaction between them result in an equilibrium.

When describing the actors as rational in these models, the concept of rationality used is *instrumental rationality* – choosing *consistently* the preferred alternative, which is judged to have the best consequences for the actor *given* his in the model exogenously given wishes/interests/goals. How these preferences/wishes/interests/goals are formed is not considered to be within the realm of rationality, and *a fortiori* not constituting part of economics proper.

The picture given by this set of core assumptions (rational choice) is a rational agent with strong cognitive capacity that knows what alternatives he is facing, evaluates them carefully, calculates the consequences and chooses the one – given his preferences – that he believes has the best consequences according to him.

Weighing the different alternatives against each other, the actor makes a consistent optimizing (typically described as maximizing some kind of utility function) choice, and acts accordingly.

Beside the core assumptions (**CA**) the model also typically has a set of auxiliary assumptions (**AA**) spatio-temporally specifying the kind of social interaction between "rational actors" that take place in the model. These assumptions can be seen as giving answers to questions such as:

AA<sub>1</sub> who are the actors and where and when do they act
AA<sub>2</sub> which specific goals do they have
AA<sub>3</sub> what are their interests
AA<sub>4</sub> what kind of expectations do they have
AA<sub>5</sub> what are their feasible actions
AA<sub>6</sub> what kind of agreements (contracts) can they enter into
AA<sub>7</sub> how much and what kind of information do they possess
AA<sub>8</sub> how do the actions of the different individuals/agents interact with each other.

So, the ur-model of all economic models basically consist of a *general* specification of what (axiomatically) constitutes optimizing rational agents and a more *specific* description of the kind of situations in which these rational actors act (making **AA** serve as a kind of specification/restriction of the intended domain of application for **CA** and its deductively derived theorems). The list of assumptions can never be complete, since there will always be unspecified background assumptions and some (often) silent omissions (like closure, transaction costs, etc., regularly based on some negligibility and applicability considerations). The hope, however, is that the "thin" list of assumptions shall be sufficient to explain and predict "thick" phenomena in the real, complex, world.

These economic models are not primarily constructed for being able to analyze *individuals* and their aspirations, motivations, interests, etc., but typically for analyzing *social* phenomena as a kind of equilibrium that emerges through the interaction between individuals. Employing a reductionist-individualist methodological approach, macroeconomic phenomena are, analytically, given *microfoundations*.

Now, of course, no one takes the ur-model (and those models that build on it) as a good (or, even less, true) representation of economic reality (which would demand a high degree of appropriate conformity with the essential characteristics of the real phenomena, that, even when weighing inn pragmatic aspects such as "purpose" and "adequacy", it is hard to see that this "thin" model could deliver). The model is typically seen as a kind of "thought-experimental" bench-mark device for enabling a rigorous mathematically tractable illustration of how an ideal market economy functions, and to be able to compare that "ideal" with reality. The model is supposed to supply us with analytical and explanatory power, enabling us to

detect, describe and understand mechanisms and tendencies in what happens around us in real economies.

Based on the model – and on interpreting it as something more than a deductive-axiomatic system – predictions and explanations can be made and confronted with empirical data and what we think we know. If the discrepancy between model and reality is too large – "falsifying" the hypotheses generated by the model – the thought is that the modeler through "successive approximations" improves on the explanatory and predictive capacity of the model.

When applying their preferred deductivist thinking in economics, mainstream neoclassical economists usually use this ur-model and its more or less tightly knit axiomatic core assumptions to set up further "as if" models from which consistent and precise inferences are made. The beauty of this procedure is of course that *if* the axiomatic premises are true, the conclusions necessarily follow. The snag is that if the models are to be *relevant*, we also have to argue that their precision and rigour still holds when they are applied to real-world situations. They often don't. When addressing real economies, the idealizations and abstractions necessary for the deductivist machinery to work simply don't hold.

If the real world is fuzzy, vague and indeterminate, then why should our models build upon a desire to describe it as precise and predictable? The logic of idealization, that permeates the ur-model, is a marvellous tool in mathematics and axiomatic-deductivist systems, but, a poor guide for action in real-world systems, in which concepts and entities are without clear boundaries and continually interact and overlap.

Being told that the model is rigorous and amenable to "successive approximations" to reality is of little avail, especially when the law-like (nomological) core assumptions are highly questionable and extremely difficult to test. Being able to construct "thought-experiments" depicting logical possibilities doesn't – really – take us very far. An obvious problem with the mainstream neoclassical ur-model – formulated in such a way that *realiter* is extremely difficult to empirically test and decisively "corroborate" or "falsify". Such models are from a scientific-explanatory point of view unsatisfying. The "thinness" is bought at too high a price, unless you decide to leave the intended area of application unspecified or immunize your model by interpreting it as nothing more than two sets of core and auxiliary assumptions making up a content-less theoretical system with no connection whatsoever to reality.

Seen from a deductive-nomological perspective, the ur-model (**M**) consist of, as we have seen, a set of more or less general (typically universal) law-like hypotheses (**CA**) and a set of (typically spatio-temporal) auxiliary conditions (**AA**). The auxiliary assumptions give "boundary" descriptions such that it is possible to deduce logically (meeting the standard of validity) a conclusion (explanandum) from the premises **CA** and **AA**. Using this kind of model economists can be portrayed as trying to explain/predict facts by subsuming them under **CA** given **AA**.

This account of theories, models, explanations and predictions does not – of course – give a realistic account of actual scientific practices, but rather aspires to give an idealized account of them.

An obvious problem with the formal-logical requirements of what counts as **CA** is the often severely restricted reach of the "law". In the worst case it may not be applicable to any real, empirical, relevant situation at all. And if **AA** is not "true", then **M** doesn't really explain (although it may predict) at all. Deductive arguments should be sound – valid and with true premises – so that we are assured of having true conclusions. Constructing models assuming "rational" expectations, says nothing of situations where expectations are "non-rational". Most mainstream economic models – elaborations on the ur-model – are abstract, unrealistic and presenting mostly non-testable hypotheses. How then are they supposed to tell us anything about the world we live in?

And where does the drive to build those kinds of models come from?

I think one important rational behind this kind of model building is the quest for rigour, and more precisely, *logical* rigour. Formalization of economics has been going on for more than a century and with time it has become obvious that the preferred kind of formalization is the one that rigorously follows the rules of formal logic. As in mathematics, this has gone hand in hand with a growing emphasis on axiomatics. Instead of basically trying to establish a connection between empirical data and assumptions, "truth" has come to be reduced to, a question of fulfilling internal consistency demands between conclusion and premises, instead of showing a "congruence" between model assumptions and reality. This has, of course, severely restricted the applicability of economic theory and models.

Not all mainstream economists subscribe to this rather *outré* deductive-axiomatic view of modeling, and so when confronted with the massive empirical refutations of almost every theory and model they have set up, many mainstream economists react by saying that these refutations only hit **AA** (the Lakatosian "protective belt"), and that by "successive approximations" it is possible to make the theories and models less abstract and more realistic, and – eventually – more readily testable and predictably accurate. Even if **CA** & **AA**<sub>1</sub> doesn't have much of empirical content, if by successive approximation we reach, say, **CA** & **AA**<sub>25</sub>, we are to believe that we can finally reach robust and true predictions and explanations. But there are grave problems with this modeling view, too. The tendency for modelers to use the method of successive approximations as a kind of "immunization", implies that it is taken for granted that there can never be any faults with **CA**. Explanatory and predictive failures hinge solely on **AA**. That the **CA** used by mainstream economics should all be held non-defeasibly corrobated, seems, however – to say the least – rather unwarranted.

Confronted with the empirical failures of their models and theories, even these mainstream economists often retreat into looking upon their models and theories as some kind of "conceptual exploration", and give up any hopes/pretenses whatsoever of relating their theories and models to the real world. Instead of trying to bridge the gap between models and the world, one decides to look the other way. But restricting the analytical activity to examining and making inferences in the models is tantamount to treating the models as self-contained *substitute systems*, rather than as *surrogate systems* that the modeler uses to indirectly understand or explain the real target system.

Trying to develop a science where we want to be better equipped to explain and understand real societies and economies, it surely can't be enough to prove or deduce things in model

worlds. If theories and models do not – directly or indirectly – tell us anything of the world we live in, then why should we waste time on them?

## The economics rules

Dani Rodrik's *Economics Rules* (Oxford University Press, 2015) is one of those rare examples where a mainstream economist – instead of just looking the other way – takes his time to ponder on the tough and deep science-theoretic and methodological questions that underpin the economics discipline.

There's much in the book I like and appreciate, but there is also a very disturbing apologetic tendency to blame all of the shortcomings on the economists and depicting economics itself as a problem-free smorgasbord collection of models. If you just choose the appropriate model from the immense and varied smorgasbord there's no problem. It is as if all problems in economics were conjured away if only we could make the proper model selection. To Rodrik the problem is always the economists, never economics itself. I sure wish it was that simple, but having written more than ten books on the history and methodology of economics, and having spent almost forty years among them "econs", I have to confess I don't quite recognize the picture.

## A smorgasbord of thought experiments

Rodrik's describes economics as a more or less problem-free smorgasbord collection of models. Economics is portrayed as advancing through a judicious selection from a continually expanding library of models, models that are presented as "partial maps" or "simplifications designed to show how specific mechanisms work".

But one of the things that's missing in Rodrik's view of economic models is the all-important distinction between core and auxiliary assumptions (on the importance on this distinction, cf. Max Albert (1994) and Hans Albert (2012[1963])). Although Rodrik repeatedly speaks of "unrealistic" or "critical" assumptions, he basically just lumps them all together without differentiating between different types of assumptions, axioms or theorems. In a typical passage, Rodrik writes (2015:25):

"Consumers are hyperrational, they are selfish, they always prefer more consumption to less, and they have a long time horizon, stretching into infinity. Economic models are typically assembled out of many such unrealistic assumptions. To be sure, many models are more realistic in one or more of these dimensions. But even in these more layered guises, other unrealistic assumptions can creep in somewhere else."

In Rodrik's model depiction we are essentially given the following structure,

A<sub>1</sub>, A<sub>2</sub>, ... A<sub>n</sub> ------Theorem,
where a set of undifferentiated assumptions are used to infer a theorem.

This is, however, to vague and imprecise to be helpful, and does not give a true picture of the usual mainstream modeling strategy, where there's a differentiation between a set of law-like hypotheses (CA) and a set of auxiliary assumptions (AA), giving the more adequate structure

or,

 $\begin{array}{c} CA_1,\,CA_2,\,\ldots\,CA_n\\ \hline \\ (AA_1,\,AA_2,\,\ldots\,AA_n) \rightarrow Theorem, \end{array}$ 

more clearly underlining the function of AA as a set of (empirical, spatio-temporal) restrictions on the applicability of the deduced theorems.

This underlines the fact that specification of AA restricts the range of applicability of the deduced theorem. In the extreme cases we get

CA1, CA2, ... CAn ------Theorem,

where the deduced theorems are analytical entities with universal and totally unrestricted applicability, or

AA<sub>1</sub>, AA<sub>2</sub>, ... AA<sub>n</sub> -----Theorem,

where the deduced theorem is transformed into an untestable tautological thought-experiment without any empirical commitment whatsoever beyond telling a coherent fictitious as-if story.

Not clearly differentiating between CA and AA means that Rodrik can't make this all-important interpretative distinction, and so without warrant is able to "save" or "immunize" models from almost any kind of critique by simple equivocation between interpreting models as empirically empty and purely deductive-axiomatic analytical systems, or, respectively, as models with explicit empirical aspirations. Flexibility is usually something people deem positive, but in this methodological context it's more troublesome than a sign of real strength. Models that are compatible with everything, or come with unspecified domains of application, are worthless from a scientific point of view.

# Pseudo-pluralism

The proliferation of economic models during the last twenty-thirty years is presented by Rodrik (2015:8-17) as a sign of great diversity and abundance of new ideas:

"Rather than a single, specific model, economics encompasses a collection of models ... Economics is in fact, a collection of diverse models that do not have a particular ideological bent or lead to a unique conclusion ...

The possibilities of social life are too diverse to be squeezed into unique frameworks. But each economic model is like a partial map that illuminates a fragment of the terrain ...

Different contexts ... require different models ... When models are selected judiciously, they are a source of illumination ...

The correct answer to almost any question in economics is: It depends. Different models, each equally respectable, provide different answers."

But, again, it's not, really, that simple.

Just as Colander, Holt, and Rosser (2004) argued, Rodrik also wants to propogate the view that mainstream economics is an open and pluralistic "let one hundred flowers bloom" science.

But in reality it is rather "plus ça change, plus c'est la même chose".

Why? Because almost all the change and diversity that Rodrik applauds only takes place within the analytic-formalistic modeling strategy that makes up the core of mainstream economics. All the flowers that do not live up to the precepts of the mainstream methodological canon are pruned. You're free to take your analytical formalist models and apply it to whatever you want – as long as you do it (Colander 2004:492) "with a careful understanding of the strengths of the recent orthodox approach and with a modeling methodology acceptable to the mainstream." If you do not follow this particular mathematical-deductive analytical formalism you're not even considered doing economics. "If it isn't modeled, it isn't economics." This isn't pluralism. It's a methodological reductionist straightjacket.

So, even though we have seen a proliferation of models, it has almost exclusively taken place as a kind of axiomatic variation -- where the core assumptions (CA) are usually untouched -within the standard "ur-model", which is always (following an unwritten, but impregnable rule) used as a self-evident bench-mark. Seen from the perspective presented here, that is actually just another variant of theory immunization. When the preferred axiomatic specification fails (we obviously don't have a case of perfect competition (auxiliary assumption AAi)) – just switch from AA<sub>i</sub> to AA<sub>j</sub> (e. g. monopolistic competition).

In Rodrik's (2015:71) world, "newer generations of models do not render the older generations wrong or less relevant," but "simply expand the range of the discipline's insights". I don't want to sound derisory or patronizing, but although it's easy to say what Rodrik says, we cannot have our cake and eat it. Analytical formalism doesn't save us from either specifying the intended areas of application of the models, or having to accept them as rival models facing the risk of being put to the test and found falsified.

The insistence on using analytical formalism and mathematical methods comes at a high cost – it often makes the analysis irrelevant from an empirical-realist point of view.

Applying closed analytical-formalist-mathematical-deductivist-axiomatic models, built on atomistic-reductionist assumptions to a world assumed to consist of atomistic-isolated entities, is a sure recipe for failure when the real world is known to be an open system where complex and relational structures and agents interact. Validly deducing things in models of that kind doesn't much help us understanding or explaining what is taking place in the real world we happen to live in. Validly deducing things from patently unreal assumptions -- that we all know are purely fictional -- makes most of the modeling exercises pursued by mainstream economists rather pointless. It's simply not the stuff that real understanding and explanation in science is made of. Had Rodrik not been so in love with his smorgasbord of models, he would have perceived this too. Just telling us that the plethora of models that make up modern economics "expand the range of the discipline's insights" is nothing short of hand waving.

No matter how many thousands of models mainstream economists come up with, as long as they are just axiomatic variations of the same old mathematical-deductive ilk, they will not take us one single inch closer to giving us relevant and usable means to further our understanding and explanation of real economies.

## Non-transparent user's guides to models

Rodrik (2015:73) argues that "the multiplicity of models is economics' strength", and that a science that has a different model for everything is non-problematic, since

"...economic models are cases that come with explicit user's guides -teaching notes on how to apply them. That's because they are transparent about their critical assumptions and behavioral mechanisms."

That is, however, very much at odds with many economists experience from studying mainstream economic models during the last decades.

When, e. g., criticizing the basic (DSGE) workhorse macroeconomic model for its inability to explain involuntary unemployment, its defenders maintain that later "successive approximations" and elaborations – especially newer search models – manage to do just that. However, one of the more conspicuous problems with those "solutions", is that they – as e.g. Pissarides (1992) "Loss of Skill during Unemployment and the Persistence of Unemployment Shocks" – are as a rule constructed without seriously trying to warrant that the model immanent assumptions and results are applicable in the real world. External validity is more or less a non-existent *problematique*, sacrificed on the altar of model derivations. This is not by chance. These theories and models do not come at all with the transparent and "explicit user's guides" that Rodrik maintains they do. And there's a very obvious reason for that. For how could one even imagine to empirically test assumptions such as Pissarides "model 1" assumptions of reality being adequately represented by "two overlapping generations of fixed size", "wages determined by Nash bargaining", "actors maximizing expected utility", "endogenous job openings", "jobmatching describable by a probability distribution," without

coming to the conclusion that this is – in terms of realism and relevance – far from "good enough" or "close enough" to real world situations?

It's difficult to see how those typical mainstream neoclassical modeling assumptions in any possibly relevant way – with or without due pragmatic considerations – can be considered anything else but imagined model worlds assumptions that has nothing at all to do with the real world we happen to live in! There is no real transparency as to the deeper significance and role of the chosen set of axiomatic assumptions. There is no explicit user's guide or indication of how we should be able to, as Rodrik puts it, "discriminate" between the "bewildering array of possibilities" that flow out of such outlandish and *known to be false* assumptions. Theoretical models building on piles of *known to be false* assumptions are in no way close to being scientific explanations. On the contrary. They are untestable and *a fortiori* totally worthless from the point of view of scientific relevance.

## On maths and models

To Rodrik an economic model basically consists of "clearly stated assumptions and behavioral mechanisms" that easily lend themselves to mathematical treatment. Furthermore, Rodrik (2015:31-32) thinks that the usual critique against the use of mathematics in economics is wrong-headed. Math only plays an instrumental role in economic models:<sup>62</sup>

"First, math ensures that the elements of a model ... are stated clearly and are transparent... The second virtue of mathematics is that it ensures the internal consistency of a model – simply put, that the conclusions follow from the assumptions."

What is lacking in this overly simplistic view on using mathematical modeling in economics is an ontological reflection on the conditions that have to be fulfilled for appropriately applying the methods of mathematical modeling.

Using formal mathematical modeling, mainstream economists like Rodrik sure can guarantee that the conclusion holds given the assumptions. However, there is no warrant that the validity we get in abstract model worlds automatically transfer to real world economies. Validity and consistency may be good, but it isn't enough. From a realist perspective both relevance and soundness are *sine qua non*.

In their search for validity, rigour and precision, mainstream macro modelers of various ilks construct microfounded DSGE models that standardly assume rational expectations, Walrasian market clearing, unique equilibria, time invariance, linear separability and homogeneity of both inputs/outputs and technology, infinitely lived intertemporally optimizing representative household/ consumer/producer agents with homothetic and identical preferences, etc., etc. At the same time the models standardly ignore complexity, diversity,

<sup>&</sup>lt;sup>62</sup> One might also note that often equations have to be rigged in order to solve in mainstream economics, as Steve Keen and many others have demonstrated.

uncertainty, coordination problems, non-market clearing prices, real aggregation problems, emergence, expectations formation, etc., etc.

Behavioural and experimental economics – not to speak of psychology – show beyond any doubts that "deep parameters" – peoples' preferences, choices and forecasts – are regularly influenced by those of other participants in the economy. And how about the homogeneity assumption? And if all actors are the same – why and with whom do they transact? And why does economics have to be exclusively teleological (concerned with intentional states of individuals)? Where are the arguments for that ontological reductionism? And what about collective intentionality and constitutive background rules?

These are all justified questions – so, in what way can one maintain that these models give workable microfoundations for macroeconomics? Science philosopher Nancy Cartwright (2012:28) gives a good hint at how to answer that question:

"Our assessment of the probability of effectiveness is only as secure as the weakest link in our reasoning to arrive at that probability. We may have to ignore some issues to make heroic assumptions about them. But that should dramatically weaken our degree of confidence in our final assessment. Rigor isn't contagious from link to link. If you want a relatively secure conclusion coming out, you'd better be careful that each premise is secure going in."

In all those economic models that Rodrik praise – where the conclusions follow deductively from the assumptions – mathematics is the preferred means to assure that we get what we want to establish with deductive rigour and precision. The problem, however, is that what guarantees this deductivity are as a rule the same things that make the external validity of the models wanting. The core assumptions (CA), as we have shown, are as a rule not very many, and so, if the modelers want to establish "interesting" facts about the economy, they have to make sure the set of auxiliary assumptions (AA) is large enough to enable the derivations. But then -- how do we validate that large set of assumptions that gives Rodrik his "clarity" and "consistency" outside the model itself? How do we evaluate those assumptions that are clearly used for no other purpose than to guarantee an analytical-formalistic use of mathematics? And how do we know that our model results "travel" to the real world?

On a deep level one could argue that the one-eyed focus on validity and consistency make mainstream economics irrelevant, since its insistence on deductive-axiomatic foundations doesn't earnestly consider the fact that its formal logical reasoning, inferences and arguments show an amazingly weak relationship to their everyday real world equivalents. Although the formal logic focus may deepen our insights into the notion of validity, the rigour and precision has a devastatingly important trade-off: the higher the level of rigour and precision, the smaller is the range of real world application. So the more mainstream economists insist on formal logical validity, the less they have to say about the real world. The time is due and over-due for getting the priorities right.

## The empirical turn

Rodrik maintains that "imaginative empirical methods" – such as game theoretical applications, natural experiments, field experiments, lab experiments, RCTs – can help us to

answer questions concerning the external validity of economic models. In Rodrik's view they are more or less tests of "an underlying economic model" and enable economists to make the right selection from the ever expanding "collection of potentially applicable models". Writes Rodrik (2015:202):

"Another way we can observe the transformation of the discipline is by looking at the new areas of research that have flourished in recent decades. Three of these are particularly noteworthy: behavioral economics, randomized controlled trials (RCTs), and institutions ... They suggest that the view of economics as an insular, inbred discipline closed to the outside influences is more caricature than reality."

I beg to differ. When looked at carefully, there are in fact few real reasons to share Rodrik's optimism on this "empirical turn" in economics.

Field studies and experiments face the same basic problem as theoretical models – they are built on rather artificial conditions and have difficulties with the "trade-off" between internal and external validity. The more artificial conditions, the more internal validity, but also less external validity. The more we rig experiments/field studies/models to avoid the "confounding factors", the less the conditions are reminiscent of the real "target system". You could of course discuss the field vs. experiments vs. theoretical models in terms of realism – but the nodal issue is not about that, but basically about how economists using different isolation strategies in different "nomological machines" attempt to learn about causal relationships. I have strong doubts on the generalizability of *all three* research strategies, because the probability is high that causal mechanisms are different in different contexts and that lack of homogeneity/stability/invariance doesn't give us warranted export licenses to the "real" societies or economies.

If we see experiments or field studies as theory tests or models that ultimately aspire to say something about the real "target system", then the problem of external validity is central (and was for a long time also a key reason why behavioural economists had trouble getting their research results published).

The increasing use of natural and quasi-natural experiments in economics during the last couple of decades has led, not only Rodrik, but several other prominent economists to triumphantly declare it as a major step on a recent path toward empirics, where instead of being a deductive philosophy, economics is now increasingly becoming an inductive science.

In randomized trials the researchers try to find out the causal effects that different variables of interest may have by changing circumstances randomly – a procedure somewhat ("on average") equivalent to the usual ceteris paribus assumption).

Besides the fact that "on average" is not always "good enough", it amounts to nothing but hand waving to *simpliciter* assume, without argumentation, that it is tenable to treat social agents and relations as homogeneous and interchangeable entities.

Randomization is used to basically allow the econometrician to treat the population as consisting of interchangeable and homogeneous groups ("treatment" and "control"). The

regression models one arrives at by using randomized trials tell us the average effect that variations in variable X has on the outcome variable Y, without having to explicitly control for effects of other explanatory variables R, S, T, etc., etc. Everything is assumed to be essentially equal except the values taken by variable X.

In a usual regression context one would apply an ordinary least squares estimator (OLS) in trying to get an unbiased and consistent estimate:

$$Y=\alpha+\beta X+\epsilon\text{,}$$

where  $\alpha$  is a constant intercept,  $\beta$  a constant "structural" causal effect and  $\epsilon$  an error term.

The problem here is that although we may get an estimate of the "true" average causal effect, this may "mask" important heterogeneous effects of a causal nature. Although we get the right answer of the average causal effect being 0, those who are "treated" (X=1) may have causal effects equal to -100 and those "not treated" (X=0) may have causal effects equal to 100. Contemplating being treated or not, most people would probably be interested in knowing about this underlying heterogeneity and would not consider the OLS average effect particularly enlightening.

Limiting model assumptions in economic science always have to be closely examined since if we are going to be able to show that the mechanisms or causes that we isolate and handle in our models are stable in the sense that they do not change when we "export" them to our "target systems", we have to be able to show that they do not only hold under *ceteris paribus* conditions and *a fortiori* only are of limited value to our understanding, explanations or predictions of real economic systems.

Real world social systems are not governed by stable causal mechanisms or capacities. The kinds of "laws" and relations that econometrics has established, are laws and relations about entities in models that presuppose causal mechanisms being atomistic and additive. When causal mechanisms operate in real world social target systems they only do it in everchanging and unstable combinations where the whole is more than a mechanical sum of parts. If economic regularities obtain they do it (as a rule) only because we engineered them for that purpose. Outside man-made "nomological machines" they are rare, or even non-existent.

I also think that most "randomistas" really underestimate the heterogeneity problem. It does not just turn up as an *external* validity problem when trying to "export" regression results to different times or different target populations. It is also often an *internal* problem to the millions of regression estimates that economists produce every year.

Just as econometrics, randomization promises more than it can deliver, basically because it requires assumptions that in practice are not possible to maintain.

"Ideally controlled experiments" tell us with certainty what causes what effects – but only given the right "closures". Making appropriate extrapolations from (ideal, accidental, natural or quasi) experiments to different settings, populations or target systems, is not easy. "It works there" is no evidence for "it will work here". Causes deduced in an experimental setting still

have to show that they come with an export-warrant to the target population/system. The causal background assumptions made have to be justified, and without licenses to export, the value of "rigorous" and "precise" methods – and "on-average-knowledge" – is despairingly small.

So, no, I find it hard to share Rodrik's and others enthusiasm and optimism on the value of (quasi)natural experiments and all the statistical-econometric machinery that comes with it. We are still waiting for the export-warrant. As Jakob Kapeller (2013:210) argues – following the argumentation in Hans Albert (2012[1963]) – is the experimental turn no reason to think that mainstream economics has left its Model Platonism behind

"Taking assumptions like utility maximization or market equilibrium as a matter of course leads to the 'standing presumption in economics that, if an empirical statement is deduced from standard assumptions then that statement is reliable' ...

The ongoing importance of these assumptions is especially evident in those areas of economic research, where empirical results are challenging standard views on economic behaviour like experimental economics or behavioural finance ... From the perspective of Model-Platonism, these research-areas are still framed by the 'superior insights' associated with early 20th century concepts, essentially because almost all of their results are framed in terms of rational individuals, who engage in optimizing behaviour and, thereby, attain equilibrium. For instance, the attitude to explain cooperation or fair behaviour in experiments by assuming an 'inequality aversion' integrated in (a fraction of) the subjects' preferences is strictly in accordance with the assumption of rational individuals, a feature which the authors are keen to report ...

So, while the mere emergence of research areas like experimental economics is sometimes deemed a clear sign for the advent of a new era ... a closer look at these fields allows us to illustrate the enduring relevance of the Model-Platonism-topos and, thereby, shows the pervasion of these fields with a traditional neoclassical style of thought."

Regarding game theory, yours truly remembers when back in 1991, earning my first Ph.D. with a dissertation on decision making and rationality in social choice theory and game theory, I concluded (Syll 1991:105) that

"...repeatedly it seems as though mathematical tractability and elegance – rather than realism and relevance – have been the most applied guidelines for the behavioural assumptions being made. On a political and social level it is doubtful if the methodological individualism, ahistoricity and formalism they are advocating are especially valid."

This, of course, was like swearing in church. My mainstream neoclassical colleagues were – to say the least – not exactly überjoyed. Listening to what one of the world's most renowned game theorists, Ariel Rubinstein, has to say on the (rather limited) applicability of game theory

(Rubinstein 2012), basically confirms my doubts about how well-founded is Rodrik's "optimism":

"I believe that game theory is very interesting. I've spent a lot of my life thinking about it, but I don't respect the claims that it has direct applications.

The analogy I sometimes give is from logic. Logic is a very interesting field in philosophy, or in mathematics. But I don't think anybody has the illusion that logic helps people to be better performers in life. A good judge does not need to know logic. It may turn out to be useful – logic was useful in the development of the computer sciences, for example – but it's not directly practical in the sense of helping you figure out how best to behave tomorrow, say in a debate with friends, or when analysing data that you get as a judge or a citizen or as a scientist ...

*Game theory is about a collection of fables.* Are fables useful or not? In some sense, you can say that they are useful, because good fables can give you some new insight into the world and allow you to think about a situation differently. But fables are not useful in the sense of giving you advice about what to do tomorrow, or how to reach an agreement between the West and Iran. The same is true about game theory."

So – contrary to Rodrik's optimism – I would argue that although different "empirical" approaches have been – more or less – integrated into mainstream economics, there is still a long way to go before economics has become a true empirical science.

# The behavioural challenges

How would people react if a renowned physicist, say, Richard Feynman, was telling them that sometimes force is proportional to acceleration and at other times it is proportional to acceleration squared?

I guess they would be unimpressed. But actually, what Rodrik does in amounts to the same strange thing when it comes to theory development and model modification.

In mainstream neoclassical theory preferences are standardly expressed in the form of a utility function. But although the expected utility theory has been known for a long time to be both theoretically and descriptively inadequate, neoclassical economists all over the world gladly continue to use it, as though its deficiencies were unknown or unheard of.

What Rodrik and most other mainstream economists try to do in face of the obvious theoretical and behavioural inadequacies of the expected utility theory, is to marginally mend it. But that cannot be the right attitude when facing scientific anomalies. When models are plainly wrong, you'd better replace them!

As Matthew Rabin & Richard Thaler (2001: 230) have it:

"It is time for economists to recognize that expected utility is an exhypothesis, so that we can concentrate our energies on the important task of developing better descriptive models of choice under uncertainty."

In a similar vein, Daniel Kahneman (2011) maintains that expected utility theory is seriously flawed since it doesn't take into consideration the basic fact that people's choices are influenced by *changes* in their wealth. Where standard microeconomic theory assumes that preferences are stable over time, Kahneman and other behavioural economists have forcefully again and again shown that preferences aren't fixed, but vary with different reference points. How can a theory that doesn't allow for people having different reference points from which they consider their options have a (typically unquestioned) axiomatic status within economic theory?

Much of what experimental and behavioural economics come up with, is really bad news for mainstream economic theory, and to just conclude, as Rodrik (2015:204) does, that these

"...insights from social psychology were subsequently applied to many areas of decision making, such as saving behavior, choice of medical insurance, and fertilizer use by poor farmers..."

sounds, to say the least, somewhat lame, when the works of people like Rabin, Thaler and Kahneman, in reality, show that expected utility theory is nothing but transmogrifying truth.

To Rodrik, mainstream economics is nothing but a smorgasbord of "thought experimental" models. For every purpose you may have, there is always an appropriate model to pick.

But, really, there has to be some limits to the flexibility of a theory!

If you freely can substitute any part of the core and auxiliary sets of assumptions and still consider that you deal with the same – mainstream, neoclassical or what have you – theory, well, then it's not at theory, but a chameleon.

The big problem with Rodrik's cherry-picking view of models is of course that the theories and models presented get totally immunized against all critique. A sure way to get rid of all kinds of "anomalies", yes, but at a far too high price. So people do not behave optimizing? No problem, we have models that assume satisficing! So people do not maximize expected utility? No problem, we have models that assume ...

A theory that accommodates for any observed phenomena whatsoever by creating a new special model for the occasion, and *a fortiori* having no chance of being tested severely and found wanting, is of little real value.

## Conclusion

If we cannot show that the mechanisms or causes we isolate and handle in our models are stable, in the sense that what when we export them from are models to our target systems they do not change from one situation to another, then they only hold under ceteris paribus conditions and a fortiori are of limited value for our understanding, explanation and prediction of our real world target system.

But how do mainstream economists react when confronted with the monumental absence of empirical fit of their economic models? Well, they do as they always have done – they use one of their four pet strategies for immunizing their models to the facts:

(1) Treat the model as an axiomatic system, making all its claims into tautologies – "true" by the meaning of propositional connectives.

(2) Use unspecified auxiliary ceteris paribus assumptions, giving all claims put forward in the model unlimited "alibis".

(3) Limit the application of the model to restricted areas where the assumptions/hypotheses/axioms are met.

(4) Leave the application of the model open, making it impossible to falsify/refute the model by facts.

Sounds great doesn't it?

Well, the problem is, of course, that "saving" theories and models by these kind of immunizing strategies are totally unacceptable from a scientific point of view.

If economics has nothing to say about the real world and the economic problems out there, why should we care about it? As long as no convincing justification is put forward for how the inferential bridging between model and reality de facto is made, economic model building is little more than hand waving.

The real economic challenge is to face reality and still try to explain why economic transactions take place – instead of simply conjuring the problem away by assuming rational expectations, or treating uncertainty as if it was possible to reduce it to stochastic risk, or by immunizing models by treating them as purely deductive-axiomatic systems. That is scientific cheating. And it has been going on for too long now.

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