

“The economic negationism” of Cahuc and Zylberberg: the first-order economy

By [Xavier Ragot](#)

The book by Pierre Cahuc and André Zylberberg[\[1\]](#) is an injunction to take scientific truths about economics into account in the public debate, in the face of interventions that conceal private and ideological interests. The book contains interesting descriptions of the results of recent empirical work using natural experiments for the purpose of evaluating economic policies in the field of education, tax policy, the reduction of working hours, etc.

However, assertions in the book that are at the borderline of reason ultimately make it a caricature that is probably counter-productive. More than just the debate over the 35-hour working week or France's CICE tax credit, what is at stake is the status of economic knowledge in the public debate.

1) Has economics become an experimental science like medicine and biology?

The heart of the book is the claim that economic science produces knowledge to treat social ills that is on the same scientific level as medicine. I do not believe this is true. Consider this quote from the winner of the 2015 Nobel Prize in Economics, [Angus Deaton](#):

“I argue that experiments have no special ability to produce more credible knowledge than other methods, and that actual experiments are frequently subject to practical problems that undermine any claims to statistical or epistemic superiority.”
([Deaton 2010](#))

The charge is serious; the point is not to deny the contributions of economic experiments but to understand their limitations and to recognize that there are many other approaches in economics (natural or controlled experiments constitute only a small percentage of the empirical work in economics).

What are the limits of experiments? Natural experiments serve only to measure average first-order effects without measuring secondary effects (so-called general equilibrium effects) that can significantly change the results. A well-known example: the work of the Nobel laureate Heckman (1998) in the economics of education, which showed that, at least in some cases, these general equilibrium effects significantly affect the results of experiments.

Moreover, experiments are not able to take into account the heterogeneity of the effects on populations, to accurately measure the confidence intervals, etc. I'll leave these technical discussions to the article by [Deaton](#). It should also be noted that the power to generalize from natural experiments is often weak, as these experiments are by their nature not reproducible.

Let's take an example: Cahuc and Zylberberg use the study by Mathieu Chemin and Etienne Wasmer (2009) comparing the effects of the reduction of working time between Alsace and the whole of France to identify the impact on employment of an additional reduction of 20 minutes of working time. This work finds no impact from an additional 20-minute reduction in working time on employment. Can we conclude that the transition to 35 hours, a reduction in working time more than ten times as great, has no impact on employment? Could there be interaction effects between lowering social contributions and reducing working time? I don't think it can be said that simply reducing working time creates jobs, but it seems difficult to claim scientifically that the transition to 35 hours did not create jobs based on the studies cited (the

authors also draw on the example of Quebec, where the reduction was much greater).

The economist uses data in much more diverse ways than presented by Cahuc and Zylberberg. The book does not discuss laboratory experiments conducted in economics (see Levitt and List, 2007). Further, the relationship of economics to data is undergoing change as digital distribution creates vast access to data ("big data" in short). Econometric techniques will in all likelihood make more intense use of structural econometrics. In a recent work (Challe et al., 2016), we develop, for example, a framework for using both microeconomic and macroeconomic data to measure the impact of the great recession in the US. Finally, there has been a renewal of economic history and long-series studies. The work of Thomas Piketty is an example that has not gone unnoticed. Other work, including on financial instability (especially that by Moritz Schularik and Alan M. Taylor), also uses long time periods to enhance intelligibility. In short, the relationship of data to economics involves multiple methods that can yield conflicting results.

This is no mere detail: the scientific approach of the book is reductive. The book by Zylberberg Cahuc advances a faith in the knowledge drawn from natural experiments that I don't believe has a consensus in economics.

2) How to sidestep major questions

Here is a concrete illustration of the problem with this approach. The authors render a severe verdict on France's CICE tax credit (the government's reduction of employer social charges on up to 2.5 times the minimum wage, the SMIC). The main argument is that it is well known that reducing charges in the neighbourhood of the SMIC has a much bigger impact on employment than for higher wage levels. This last point is true – but the authors are sidestepping the real issue. What is it?

The early years of the euro have seen an unprecedented divergence in labour costs and inflation between European countries. Up to the 1990s, these differences were handled over the years by devaluations / revaluations. But the single currency has made this no longer possible. The question facing economists looking at this situation is whether the euro zone can survive such misalignments (see the recent position of Stiglitz on this subject). The discussion has been focused on establishing internal devaluations in overvalued European countries and boosting wages in undervalued countries. To this end, Germany established a minimum wage, some countries cut the salaries of civil servants, while others lowered their social contributions (the CICE tax credit in France), in the knowledge that other fiscal tools are also possible (see Emmanuel Farhi, Gita Gopinath and Oleg Itskhoki, 2013). The crucial question is therefore: 1) Is an internal devaluation necessary in France, and if so how much? 2) And how could a non-recessionary internal devaluation be implemented without increasing inequality?

So there is clearly a problem if one answers these questions based on the impact of reductions of social charges near the SMIC wage level. This shows the danger of basing oneself solely on results measurable by experiments: it neglects key issues that cannot be decided by this method.

3) The problem of “Keynesianism”

The authors claim that Keynesianism provides fertile soil for negationism even while stating in the book that Keynes' recipes sometimes work, but not all the time, which any economist would acknowledge. In the absence of clarification, these remarks become problematic. Indeed, recent years (following the 2008 subprime crisis) have witnessed a return of Keynesian approaches, as can be seen in recent publications. I would go so far as to say that we are living in a Keynesian moment, with great financial instability and massive macroeconomic imbalances (Ragot, 2016).

What then is Keynesianism? (It is not, of course, fiscal irresponsibility with ever greater public debt). It is the claim that price movements do not always allow markets to operate normally. Prices move slowly, wages are downwardly rigid, nominal interest rates cannot be very negative, etc. Because of all this, there are demand externalities that justify public intervention to stabilize the economy. The French debate generates concepts like “Keynesianism” and “liberalism” that have no real meaning in economic science. It is the role of the scientist to avoid false debates, not to perpetuate them.

4) Should we listen only to researchers publishing in the top journals?

The public debate differs greatly from the scientific debate in both purpose and form. Cahuc and Zylberberg want to import the hierarchy of academic debate into the public debate. This won't work.

There will always be a need for non-academic economists to discuss economic issues. The economic situation raises problems where there is no academic consensus. The business press is full of advice from bank economists, markets, institutions and trade unions, all of whom have legitimate, though non-academic, points of view. Newspapers like *Alternatives Economiques*, quoted by Cahuc and Zylberberg, present their views, as does the *Financial Times*, which has a mix of genres. Economists without formal academic credentials play a legitimate role in this debate, even if their opinions differ from those of other researchers with longer CVs.

These contradictions are concretely lived at the OFCE, whose mission is to contribute to the public debate with academic rigor. This is a very difficult exercise; it requires knowledge of the data, the legal framework, and the academic literature produced by institutions such as the Treasury, the OECD, the IMF, and the European Commission. Knowledge of the

economic literature is essential, but it is far from sufficient to make a useful contribution to the public debate.

The willingness of economists to contribute to the public debate was exemplified in the various petitions around the El Khomry law. These petitions widely debated the effect of redundancy costs on hiring and the form of the employment contract, but not the overturning of norms (a subject that to my knowledge is impossible to evaluate rigorously) – even though this is at the heart of the debate between the government and the trade unions! It is not certain that the idea of a consensus among economists will emerge strengthened by this episode.

5) When a consensus exists in economics, do we have to listen to it?

The consensus before the subprime crisis was that financialization and securitization were factors promoting economic stabilization, because of risk allocation, etc. Microeconomic studies confirmed these intuitions, because they failed to capture the real source of financial instability, which was the correlation of risks in investor portfolios. We now know that the consensus was wrong. Some economists outside the consensus, such as Roubini or Aglietta, and some economics journalists such as *The Economist*, warned of the destabilizing effects of finance, but they were outside the consensus.

Policy (and the public debate) is forced to ask: what will happen if the consensus is wrong? It has to manage all the risks – that's its responsibility. The consensus view among economists is frequently not very informative about the diversity of viewpoints and the risks involved. The public voice of economists outside the consensus is necessary and useful. For example, the Nobel Prize in Economics was awarded to Eugene Fama and Robert Schiller, who both studied financial economics. The first asserts that financial markets are efficient, and the second that financial markets generate

excessive volatility. Newspapers carry visions outside the consensus, such as *Alternatives Economiques* in France (at least it's in the title). These publications are useful to public discussion, precisely because of their openness to debate.

In science, the diversity of methods and knowledge about methodology outside the consensus enrich the debate. For the same reason, I tended to be against the creation of a new section of heterodox economists, supported by the French association of political economists (AFEP), because I see an intellectual cost to the segmentation of the world of economists. For the same reason, giving a consensus among economists the status of truth (Cahuc, Zylberberg, p. 185) is troublesome, because it ignores the contributions of the "minority" effort.

6) "Economic negationism: radicalization of the discourse"

The authors castigate ideological criticisms of economics that are unfamiliar with the results or even the practice of economists. The science of economics has strong political implications, and is therefore always attacked when generates disturbing results. Some criticisms lower the intellectual debate to the level of personal insults. A defence of the integrity of economists is welcome, but it requires real learning and modesty to explain what is known and what is not known.

On reading the book by Cahuc and Zylberberg, it seems that the authors take up the arms of their opponents: two camps are defined (real science and deniers), doubts are planted about the intellectual honesty of pseudo-scientists outside the consensus, we proceed by amalgamation, by mixing intellectuals (Sartre) and academic economists. The very title of the book proceeds from great violence. This book is on a slippery slope in the intellectual debate that is heading towards a caricature of debate and verbal abuse. Every economist

involved in the public debate has already been insulted by people who disagree with the results presented for purely ideological reasons. Insults need to be fought, but not by suggesting that debate can be avoided due to one's academic status.

The debate in England on Brexit showed how economists and experts were rejected because of their perceived arrogance. I'm not sure that the scientistic position of the book offers a solution to these developments in the public debate. To quote Angus Deaton once again, in a recent interview he did with the newspaper *Le Monde*:

"To believe that we have all the data is singularly lacking in humility. ... There is certainly a consensus in economics, but its scope is much narrower than economists think."

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[\[1\]](#) Pierre Cahuc and André Zylberberg, *Le négationnisme économique et comment s'en débarrasser*, Paris, Flammarion, 2016.