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# Economizing on Scientific Debate

*By Olivier Godechot*

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***Le négationnisme économique. Et comment s'en débarrasser* (Economic Denialism, and How to Get Rid of It) has sparked lively discussion in France. What can we draw from this book for a reflection on social science methods and the terms of scientific debate?**

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Reviewed: Pierre Cahuc and André Zylberberg, *Le négationnisme économique. Et comment s'en débarrasser*, Paris, Flammarion, 2016, 256 p., 18 €.

The book *Le négationnisme économique. Et comment s'en débarrasser* by Pierre Cahuc and André Zylberberg has sparked a heated debate in the French press and academy. Its aim is to uncover the inadequacies, knowledge gaps, errors, and denials on the part of researchers and public actors who criticize economic science, and especially those who criticize the latter's dominant current, the so-called mainstream economics. Its main targets are critical economists, particularly "heterodox" or "appalled economists," but also intellectual figures from other scientific disciplines (Pierre Bourdieu, Michel Onfray, Dominique Méda, Axel Kahn), CEOs (Jean-Louis Beffa, Louis Gallois), politicians (Michel Rocard, Daniel Cohn-Bendit, Barbara Romagnan), and newspapers (*Alternatives économiques*).

## A Game of Opposition

This critique of critique adopts the strategy of the counter-example: It purports to disqualify the critics of economic science by highlighting in each case a finding of economic science that is endowed with a scientific authority presented as indisputable. For skeptics to be convinced,

however, these exemplary findings cannot rest on the theoretical assumptions of their authors (as is generally the case in formal theoretical models). Consequently, the book relies on the new experimental findings of economic science derived from the “credibility revolution,”<sup>1</sup> forcefully claiming that economics has now become an “experimental science” modeled on the medical sciences. Just as medical researchers can determine the effectiveness of a drug—regardless of their prior opinion on the matter—by comparing the outcome of patients randomly allocated to two groups, one taking the treatment and the other a placebo, so too can economic science determine the validity of a theoretical mechanism and the effectiveness of an economic policy by means of an experimental approach which similarly compares a treated group and a control group.

The authors mention a few experiments based on this canonical experimental approach, such as the Perry Preschool Program and the Moving to Opportunity experiment. However, they rely mainly on empirical methods—the so-called “natural experiments”—that draw on this approach without being able to respect all of its methodological protocol. In natural experiments, it is not researchers who determine and control the allocation of individuals to control and treated groups, but social life that produces similar situations: contiguous territories subject to different policies, selection thresholds distributing affected and unaffected groups on a razor’s edge, temporal variation in the application of a measure, etc.

For example, the increase in the minimum wage in New Jersey, by contrast with Pennsylvania where it remained stable, led to a rise in employment (Chapter 1). The abolition of subsidized loans in 1985 in France reallocated credit in favor of the most profitable projects and revealed the positive impact of finance on growth (Chapter 3). The tax-free fiscal year of 1987 in Iceland, subsequent to the introduction of the withholding tax on income, led to an increase in time worked, thus showing that taxation does discourage economic activity (Chapter 4). The comparison of European regions on either side of the threshold for receiving European aid strongly nuances the notion that public expenditure favors economic development (Chapter 5).

Throughout the five central chapters (on industrial policy, finance, taxation, public expenditure, and working time), the authors contradict, by means of (quasi-experimental) economics articles, the arguments of those who favor industrial policy, strict control of finance, working time reduction, or the increase in tax-financed public expenditure. The book’s vulgarizing approach is welcome. Moreover, its tone is rather pro-market—a point that in itself would merit further discussion. But the book must be evaluated in light of its primary objective, one that is above all epistemological: To denounce the obscurantist imposture of the critique of economic science and to re-establish the authority of true science.

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<sup>1</sup> Joshua Angrist and Jörn-Steffen Pischke, “The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con Out of Econometrics”, *The Journal of Economic Perspectives*, 2010, 24 (2), pp. 3-30.

Yet beyond the oppositions regarding the merits or limits of free-market economics *vs.* state interventionism, orthodoxy *vs.* heterodoxy, experiments *vs.* theory, pluralism *vs.* unity, the authors make several questionable points that, far from stimulating scientific debate, effectively paralyze it: offensive connotations, poorly supported allegations of science denial, biased accusations of ignorance, and recourse to arguments from authority.

## The Terms in Debate

The use of the French term “*négationnisme*” is not a mere editorial decision. The word appears 58 times in the book. *Négationnisme*, however, is not an ordinary concept. In law, the notion refers to the denial of crimes against humanity, a denial that is condemned in France by the Gayssot Law. While the crime against humanity is the most severely sanctioned crime, *négationnisme* is its most serious intellectual counterpart. In the social sciences, the French “*négationnisme*” (unlike the English “denialism”) is exclusively reserved for the ideological and complicit denial of mass massacres.<sup>2</sup> In addition to legal references, a Google Scholar query for this keyword mainly returns references to the denial of the Jewish genocide, as well as some references to the denial of the Rwandan and Armenian genocides, massacres by Japanese troops, colonial crimes, or the repression of 17 October 1961 in France. The authors do not ignore that fact, and the reader is warned from the first pages: The phenomenon about to be denounced will be very different. *Négationnisme* is redefined as the denial of abundantly documented scientific knowledge (p. 6).

Yet the terms of natural language, unlike those of mathematical language, cannot be redefined with the wave of a magic wand. The denotation may change, but the connotation remains. It hurts, it demeans, it offends. And yet, the French language includes words that allow for designating the phenomenon criticized in the book without mobilizing the opprobrium linked to mass massacres: for instance, economic negation or denegation. One could also invent, on the model of “climate skepticism,” neologisms such as “science skepticism” or “economic skepticism.”

Are the texts and ideas being criticized the mark of an economic skepticism similar to climate skepticism or creationism, which unfortunately pollute public debate to the detriment of true scientific knowledge? The accusation of science skepticism is serious and should not be formulated lightly. It implies specific conditions, such as common knowledge, a very broad consensus in the research community, very strong and convincing evidence for a body of

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<sup>2</sup> Thus, Robert Proctor uses the English term denialism in his book on tobacco. See Robert Proctor, *Golden Holocaust: Origins of the Cigarette Catastrophe and the Case for Abolition*, University of California Press, 2011. This term was mistakenly translated as *négationnisme* in the French version.

knowledge, but also deniers who deny, dissimulate, or distort such knowledge and its evidence with arguments that are truncated or made in bad faith, who do not publish on these issues in established scientific outlets, and who are very widely recognized among specialists as fake scientists, etc. The additional difficulty, if one adopts the perspective of the science historian Thomas Kuhn, is that researchers who challenge the dominant paradigm during an episode of scientific revolution can be regarded as science skeptics. The line is difficult to draw between the inspired methodical doubt of the revolutionary genius and the pathological doubt of the ideologue.

## The Crime of Ignorance

The book, however, does not generally meet the conditions for making this accusation. It fails to highlight, in the writings targeted for criticism, blatant denials of solidly established and consensual scientific findings. The alleged offenses, as evidenced by quotations taken mostly from interviews and essays aimed at the general public, and not from more nuanced and documented academic works, are at worst either abusive generalizations or crimes of ignorance. Those who overlook this or that economics article regarded as important by the authors are accused of economic skepticism. To ignore scientific works is certainly regrettable, but it is the most common aspect of scientific life. In any given scientific field, hundreds of texts are published every day, and it is cognitively impossible to master all the information. A scientist will always ignore a text that another will consider important. This is eminently reversible—and, unsurprisingly, the book has been criticized in particular for its ignorance in matters of epistemology.<sup>3</sup> In addition, an empirical finding is valid only under the assumption that the studied phenomenon is adequately measured. An article can be overlooked because it has failed to convince some readers due to the measure's limitations, even as it has convinced others. Rational discussion serves to determine the scope and limitations of articles without the need for anathemas.

Finally, to justify their very personal selection of empirical studies that allow them to contradict the critique of economics, the authors do not hesitate to supplement their argumentation with the most external hierarchical symbols of scientific reputation: classification of journals, impact factor, researchers ranking, John Bates Clark medal, Nobel Prize. The point is not to deny any informative value to such signs, but to recall that if they are needed to gain adherence, then they function as arguments from authority. One should be able to convince in a rational debate without resorting to this.

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<sup>3</sup> See André Orléan, “Quand Messieurs Cahuc et Zylberberg découvrent la science,” *Alternatives économiques*, 12 September 2016.

## The Challenge Of and To the 35-Hour Workweek

The pages on the “35-hour workweek” illustrate these four limitations. The accusations against those who defend the job-creating effects of working time reduction are out of hand (“They are completely lost in economic denialism,” p. 74). Here, however, the consensus among specialists on the issue is rather that the 35-Hour Workweek laws did create jobs, even if uncertainty remains as to the underlying mechanisms (pure working time reduction, reorganization of work, wage moderation, or decrease in compensation costs). These conclusions are based on scientific articles that use methods comparing treated and control groups that are thoroughly consistent with those promoted in the book. They are credible provided, as always, that the measurement assumptions are met (in this case the absence of selection bias).

Faced with these studies, the book *Le négationnisme économique* brandishes a similar type of article comparing the departments of Alsace-Moselle (treated group), where working time reduction was found to be lower given the possibility of including two local holidays in the count, to the rest of France (control group).<sup>4</sup> This last article, which had the advantage of subtracting the effects of the decrease in compensation costs, showed that working time reduction does not create jobs. But it remained cautious, its finding being valid, here again, only so long as measurement assumptions (i.e., absence of unobserved heterogeneity<sup>5</sup>) were actually verified. In fact, it was criticized precisely on this point, a criticism that was ignored in the book. In order to reinforce the authority of this article and make its omission seem even more culpable, the authors do not hesitate to mention its publication in “academic journals worthy of the name” (p.73), suggesting, perhaps, the indignity of the journals that published the previous studies.

Yet, it turns out that the findings of this article—as often happens in science—suffer from two flaws<sup>6</sup>: erroneous computer coding and failure to take into account the impact on working time of cross-border workers (who are numerous in Alsace-Moselle). The correction of these errors yields far less convergent and convincing results, the significance of which is currently under discussion.<sup>7</sup> In retrospect, while the authors may be right that there is no impact of the 35-hour

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<sup>4</sup> Matthieu Chemin and Étienne Wasmer, “Using Alsace Moselle Local Laws to Build a Difference in Differences Estimation Strategy of the Employment Effects of the 35 Hour Workweek Regulation in France,” *Journal of Labor Economists*, 2009, 27 (4), pp. 487-524.

<sup>5</sup> To determine the impact of working time reduction on employment, the article hypothesizes that the model has taken into account all factors in the differentiated evolution of the two geographical areas, and that the differences observed cannot be attributed to other unobserved variables.

<sup>6</sup> Olivier Godechot, “L’Alsace-Moselle peut-elle décider des 35 heures?” *Notes et documents de l’OSC*, 2016-04, 2016 and “Can We Use Alsace-Moselle for Estimating the Employment Effects of the 35-Hour Workweek Regulation in France?”, Mimeo.

<sup>7</sup> See Matthieu Chemin and Étienne Wasmer, “Réponse à ‘L’Alsace-Moselle peut-elle décider des 35 heures?’” Mimeo, 2016.

workweek once the decrease in compensation costs has been accounted for, they are very imprudent in making believe that science has definitively settled the question. Thus, the culpable ignorance of researchers, journalists, and deputies who discuss the effects of working time reduction without taking this study into account appears less severe.

This discussion can be read as a plea for academic caution and courtesy, which are often dull, formulaic, and also hypocritical when one considers the intensity of underlying convictions. Yet, this mode of expression serves a function: It is that which best allows for the rational evaluation of arguments in a scientific debate.

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