

# About crises and methodological paradigms

by Dr Stefan Gruner

Since the seminal work on scientific paradigm shifts by Thomas Kuhn<sup>1</sup>, the terms “paradigm” and “paradigm shift” have become so popular that they are nowadays used or misused in an inflationary manner at almost any occasion. This was also the case during the most recent economic turbulences. The hard global economic crisis of 2008/09 has tossed the sciences of economics into a deep internal and external crisis.

Externally, the sciences of economics as a whole came under heavy criticism from many members of the wider public<sup>2</sup>. Without much differentiation, ordinary people who lost their jobs and money pointed their fingers at those “MBA guys” in their fine shoes and Italian suits who seem to have single-handedly messed up the global economy.

Internally, in the faculties of economics, the crisis seems to have triggered some serious infighting between different “schools” and traditions of economics who blame each other for not having foreseen the emergence of that crisis.

For Germany, in particular, this discussion is documented in a series of articles in the newspaper *Frankfurter Allgemeine Zeitung*<sup>3</sup>. Without going into the details of those articles, one can summarise the feature as follows: There is a quarrel going on (particularly in Germany) between an “old school” of economics that is more philosophically and historically orientated, and a “new school” of economics that is more mathematically or formalistically orientated. This methodological quarrel is at the same time also a fight for scarce academic tenure positions (with an additional Freudian component of academic “sons” wanting to “kill” their academic “fathers”, metaphorically speaking).

Thus, the crisis of the real economy triggered a crisis in the academic field of economics. The reasons why I mention this crisis of economics from an external perspective as an “IT guy” (in a computer science position) are the following:

First of all, the threads of IT (and its applications) are now deeply woven into the fabric of the sciences of economics, especially their new formalistic branch, with their (recently so disreputed) computer-model-based methodology. If the new school of economics were to lose the methodological dispute against the old school of “philosophical” economics in the aftermath of the current global economic crisis, then the “helper science” of IT would presumably suffer some consequences as well, be it in the form of ideological pressure on its own legitimation, or simply in the form of a shrinking demand for software and hardware equipment by the faculties and institutions of economics.

Secondly, we have already seen similar methodological “school” quarrels in two IT-related fields, namely in information science (IS) between what I call the “school of sociologists” versus the “school of technologists”, and in the field of software engineering (SE) between the “school of formalists” and the so-called “agile alliance”. Neither of these two IT-related methodological quarrels has yet ended decisively, from which I gather that the often-quoted “software crisis” has really not yet hit us hard and deep enough to enforce definitive decisions in this regard. My latest journal publication<sup>4</sup> delves deeper into this topic.

However, it is not only an external crisis that has the power to bring down the favourite methods that we invented in the lofty air of our academic methodology seminars. Some dialectic potential of evolutionary self-devaluation is already inherent in our methods themselves, which must make us very sceptical about any kind of what I like to call “method fetishism”.

<sup>2</sup> Brooks, A. Business Schools mull over Blame in Financial Crisis. <http://www.npr.org/templates/story/story.php?storyId=103719186>  
<sup>3</sup> *Frankfurter Allgemeine Zeitung*, special feature on economics: Dogmenstreit der Ökonomen. <http://www.faz.net/!RubB8DFB31915A443D98590B0D538FC0BEC/1?l~Ecommon-SThemenseite.html>

<sup>4</sup> Gruner, S. 2010. Software Engineering between Technics and Science – Recent Discussions about the Foundations and the Scientificity of a Rising Discipline. *Journal for General Philosophy of Science*, Vol. 41, No. 1, pp. 237-260, Springer Verlag.

<sup>1</sup> Kuhn, T. 2009. *The structure of scientific revolutions*. Books LLC Publ.

This is because some discovery D, made with the help of some method M, might “backfire” on the very applicability conditions of M, which must then be seen in the new light of D, which was not available before the actual application of M. Long before the notorious polemic by Paul Feyerabend against method<sup>5</sup>, this was already seen very clearly by nobody less than Martin Heidegger, who wrote as long ago as 1927 (here my translation from the German original): “Especially when a method is genuine, i.e. when it truly enables access to the objects, the progress made on the grounds of that method (...) will necessarily lead to its outdatedness”<sup>6</sup>.

In my experience, many postgraduate students are far too obsessed with

their so-called “methodology chapter” for their dissertation in the context of some particular school of thought which stipulates their so-called “research paradigm”. Following Thomas Kuhn strictly, however, we should keep in mind that we cannot simply choose between research “paradigms” in the way we can choose between tea or coffee for breakfast: new paradigms emerge from crises, and the emergences of crises are not fully under our voluntary control. If we can only make sure, following Heidegger, that our methods are “genuine” in the sense that they really provide “access to the objects”, then we no longer need to worry too much about research paradigms and methodology chapters. ☺

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<sup>5</sup> Feyerabend, P. 1975. *Against Method*. 3<sup>rd</sup> edition. Verso Publ.  
<sup>6</sup> Heidegger, M. 1975. *Die Grundprobleme der Phänomenologie*. Lecture at the University of Marburg, 1927. Frankfurt am Main: Vittorio Klostermann.