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James R. Wible, *The Economics of Science: Methodology as if Economics Really Mattered.* Routledge, (1998, Paperback), 2014, 288 pp. $24 Hardback.

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Book Review: Is Science Self-Corrective

James R. Wible's *T he Economics of Science: Methodology and Epistemology as if Economics Really Mattered*, originally published in 1998, has been reprinted in paperback by Routledge in 2014. This essential work has been the most significant work focusing the attention of the philosophers of science on the economic character of scientific research.

Wible's works on Milton Friedman, John Dewey, Charles Sander Peirce, and Nicholas Rescher (1982, 1984, 2008, 1994) have long been well-known. His *The Economics of Science*, a seminal piece of scholarship in the field of economics of scientific knowledge, has also been well-known and reviewed a number of times by prominent economic philosophers (Sent 2000, Hands 2000, Diamond 2006). I would like to use this opportunity to inform the readers of this journal about the reprint of Wible's *The Economics of Science* and re-assess his work from the viewpoint of the theory of intellectual path dependency in economics (Yalcıntas, 2016, Forthcoming). My intention here is to pick up one of the issues in the philosophy of science, namely, self-correction in sciences and humanities, and argue, somewhat contrary to what Wible claimed in his *The Economics of Science*, that science does not always correct itself.

Self-correction in science means that errors, mistakes, and misinterpretations are automatically cleared off if and when scientists have the opportunity to exchange ideas freely. As Wible remarks, "[f]rom an economic point of view, science seems to move forward most rapidly when structured as small-numbers pluralism. Progress in science, to the extent that it occurs, is a consequence of focusing scarce scientific resources and the best scientific minds on the very best theories and their next best alternatives" (Wible 1998: 2). Although I think that scientists vigorously debate viewpoints of other scientists, I argue that sciences and humanities are *not* always self-corrective, neither in the short nor the long runs. Under the conditions of intellectual path dependence, where the standard procedures of scientific reasoning such as critical rationalism (Popper) and dialectics (Hegel) do not clear the market of ideas from the consequences of enduring errors and mistakes, errors remain uncorrected. The invisible hand in science, so to speak, does not properly work. Let me elaborate.

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James R. Wible, interim department chair and professor of economics at Peter T. Paul College of Business and Economics of the University of New Hampshire, isan eminent scholar working on the economics of science, scientific misconduct, and research ethics in economics. The readers of this journal may find it interesting to know that Wible was one of the two keynote speakers at the first-ever workshop on scientific misconduct in economics, held in Izmir on 21 and 22 August 2014. The title of the workshop was "International Workshop on Scientific Misconduct and Research Ethics in Economics." (For more information, see website of *the Research Association for Knowledge Integrity in Economics*, a scholarly network set up by a group of economists who are concerned about the unresponsiveness of the professionals in economics to the significance of the problem of research misconduct, at http://rakie.org/.) In *The Economics of Science*, Wible argues that the economics of science has begun with the general problem of misconduct in science, which he thinks is the main source for *market failure in academic economics*. In economics, market failure is characterized by a tendency towards moving away or inability to reach to a Pareto optimum state of equilibrium. According to Wible, failure in the market of ideas occurs because there is a cost for using market mechanisms in scholarly life. In other words, scholarly life is a positive transaction costs world in the sense that universities and research institutes produce ideas for science in which high transaction costs of pursuing science require institutional scientific arrangements (such as awareness against fraud and plagiarism, definite codes of behavior, etc.) to cure the negative externalities that may come about as a result of scientific effort. Universities and research institutes minimize the costs that emerge from risk aversion in scientific markets. The reason why scientific institutions exist is that the sciences have an economic dimension. This dimension is about the efficiency problem of scholarly activity.

Science is a "resource-using endeavor" (Wible 1998: 170) and resources in the scientific market are scarce. As a result, the opportunity cost of (not) using a specific resource in the market is often high. Wible argues that, under such circumstances, researchers are like entrepreneurs making a multi-period calculation of the value of each research program in each time period. This calculation involves items such as initial costs of putting the project in place and revenues to be gained from implementing the project. Researchers get involved in a research program if the net present value of the project is greater than all other projects.

Wible identifies three different mechanisms that give rise to market failure in academic scholarship:

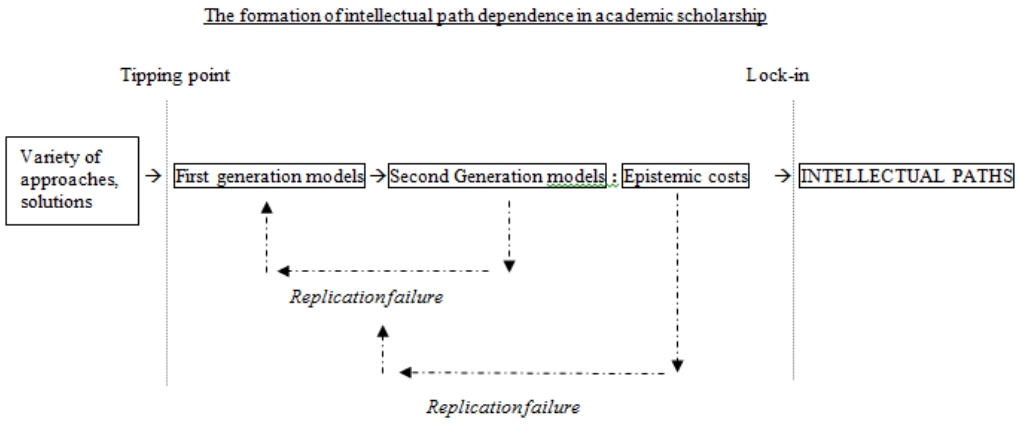
1. Fraud.
2. Plagiarism.
3. Replication failure.

"Fraud" and "plagiarism," in the sense Wible uses the terms, involve deliberate violation of scientific principles whereas replication failure is a dynamic mechanism in science in which intentions of individuals are not necessarily a constituent of the working of academic scholarship. Replication failure is the inability or unwillingness of researchers to test the result of previously published scholarly work. It is through replication that theories and research programs are checked in terms of their defensibility, consistency, and coherency. Although replication should be an essential component of scholarly work, it is not frequently handled by researchers because "an economist might allocate a larger proportion of time to producing new publishable results devoting relatively less time and effort to the tasks required for replication" (Wible 1998: 25). Replication of results is time consuming and there is no reward for scholars to repeat another's work. No significant research devotes time and effort to replicating the findings of earlier theories and research programs without compensating their economic loss. Instead,

researchers rely on the results of papers published in academic journals and they simply "reproduce" their findings without examining its significance and validity (Mirowski & Sklivas 1991, 154).

Wible reports that the reason why replication rarely takes place in scholarly life is, for instance, that processes of inference and judgement are not totally individual but rather a social phenomenon. It is difficult to replicate certain experiments without the specialized knowledge of earlier researchers. Sometimes, derived results of earlier works could be unreplicable: data may be lost, technical possibilities may not allow researchers to set up identical experimental environments, there may be informational asymmetries, and so forth and so on. Moreover, attempts by rival scientists and graduate students to criticize and publish new findings on older data as part of their routine scientific doings are construed as hostile acts.

In other words, subsequent research depends on previous studies. Findings of prior studies are used as input for upcoming research. When researchers embark on a new research project, they allocate time between replicable and unreplicable research. Authors, referees, and editors of journals often assume earlier findings are valid without retesting them in significant ways because replication takes time and this imposes significant costs on researchers. Bypassing replication generates the probability of genetic (i.e. reproductive) errors that had occurred in an earlier study but not noticed in time. In scholarly life, it is expected that such errors will be corrected as scientists do further research on the subject matter. But because of the costs of running such tests, some errors may pass unnoticed and be left uncorrected. This is a source of intellectual path dependence in which scientific markets operate in the absence of an invisible hand that could have prevented errors from happening or corrected them in the long run. Seeds of intellectual lock-in are stored in the scholarly market within the *epistemic costs* (Yalcintas 2013)organically attached to pursuing scientific research.



The Figure demonstrates a typical path formation in scholarly life. In this scenario, first generation models include influential articles whose findings, by virtue of innovative research programs, are (re-)formulated in original ways. After the first generation models, findings are popularized and spread in the market. Second generation models often take the form of textbooks and secondary or follow-up research, the findings of which are primarily borrowed from first generation models. Second generation models are so influential that they frequently cite first generation models as well as each other and, as a result, their citation

figures increase logarithmically. *Their* results are thus established in the market. The findings of the first generation models are not often replicated because of high opportunity costs of allowing time for re-testing the findings of others. Reputation and power also affect the selection process of researchers where scholars utilize the works of reputable authors to legitimize their own findings (Sterman & Wittenberg 1999). For instance, research conducted by Nobel laureates and powerful institutions with which Nobel laureates are affiliated (such as Rand Corporation, see Mirowski, 2002, 153-231) are highly credited and further research is often directed by the outcomes of such authors' work. During all of this, access to financial resources and possibilities of finding research partners play prominent roles. This intermediary phase is thus (i)economically constructed so that the growing popularity of the first and second generation models operates under increasing returns, (ii) the search for research funds is highly influential on the way further research (and its methodology) is conducted, and (iii) epistemic costs determine whether to replicate the findings of the original research program.

Now, my point is that during the process of path formation, there is no guarantee that errors, no matter how small or how big, are corrected. Who can ever know that findings and methodology of a particular piece of scholarly publication, if and when the findings and methodologies are not tested for validity, do not contain any mistakes? And who can ever assure that mistakes in the finding and methodologies, if and when they are not tested for validity, will eventually be corrected? As Wible remarks, too, "[t]he economizing of resources thus exposes science to mistakes," (1998: 31). However, he does not show how the market of ideas would be cleared off from these mistakes. Wible only claims that "[a]t some point, these mistakes will be discovered and they will have to be corrected. From an economic point of view, there needs to be a balance between resources devoted to replication in its simpler forms and innovation. Attitudes and reward structures which are skewed toward innovation may set science up for replication failures of many types. But mistakes need to be corrected or they will impede scientific progress and innovation at some point" (Wible 1998: 31).

Wible original work has been one of the best pieces of scholarship in which it is argued that economics can be used to account for the ways in which academic economists work. I fully agree with Wible that science is economically constructed. I, however, would also like to claim that the assumption that science is self-corrective is unrealistic because there are plenty of cases in the history of economics where economists are not able to correct the errors involved in models published in the past. The "Coase Theorem" is just an example where economists have used the "theorem" to mean zero transaction costs although many scholars, including Ronald Coase himself, noted that this is the opposite of what he meant (Medema 2011b, a, McCloskey 1998). Also, Steve Ziliak and Deirdre McCloskey have shown statistical significance tests are overused and misused in economics (Ziliak & McCloskey 2008); statistical significance tests are still in use. (For further examples and discussion, see the special issue of *the Review of Social Economy*, forthcoming in 2016, entitled "Scientific Misconduct and Research Ethics in Economics.") I, therefore, contend that self-correction in sciences cannot be generalized because we cannot know whether scientists, under the conditions of positive epistemic costs, are always able to correct the errors in academic scholarship. It seems more realistic to me to assume that scientists are prone to errors and errors are embedded in academic scholarship. The question is the following: *why does the nature of the processes of scientific knowledge production at universities not allow us to correct errors fully*? Although there is an enduring tendency among scholars to criticize each other's work, there have always been cases where it is epistemologically costly to lock out the sciences from erroneous

findings and methodologies. This is perhaps a reason why economists keep using invalided theories. As philosophers and historians of economics, we should rather research into specific cases in which errors are not corrected. Wible's *The Economics of Science* is a good start. His groundbreaking work is highly recommended.

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