

# Backward-Oriented Economics<sup>1</sup>

Bruno S. Frey<sup>1,2</sup>

<sup>1</sup>Permanent Visiting Professor, University of Basel, Peter-Merian-Weg 6, Basel, Switzerland

<sup>2</sup>CREMA, Center for Research in Economics, Management and the Arts, Südstrasse 11, Zurich, Switzerland

## Correspondence

Bruno S. Frey, University of Basel, and, CREMA – Center for Research in Economics, Management and the Arts, Zurich.  
Email: bruno.frey@bsfrey.ch

## Abstract

Nowadays, academic journals of high standing rarely accept a conceptual idea in a paper not instantly accompanied by econometric estimates. The idea would almost certainly get rejected. Empirical validation based on past statistical data has produced an unfortunate backward orientation in economics. While one can learn from the past, this approach fails when the underlying conditions strongly change.

The paper suggests various possibilities to overcome the intense publication pressure in so-called top journals and the overemphasis on instant empirical evidence. Academia is, however, unlikely to adapt. As economics is too backward oriented, other disciplines or cranks may well dominate future economic policy.

## JEL CLASSIFICATION

A10; A11; B40; C10; C80

## 1 | PUBLICATION PRESSURE AND EMPIRICAL EVIDENCE

Today, a career in professional academic economics absolutely requires publications in accepted professional journals. The pursuit of publications in the so-called “Top Five” has become “the obsession of the next generation of economists” (Heckman & Moktan, 2020, p. 419). Economists are subject to a strong publication pressure; one may even speak of a “publish or perish culture” (Colander, 2008; Van Dalen & Henkens, 2012). The publications must be in a top scientific journal; in contrast, to write a book may even be counterproductive for an academic career.

Today, journal publications must, moreover, meet a specific formal requirement:

*A conceptual idea in a scholarly paper must be supported by empirical evidence.* To just present a conceptual idea in a paper submitted to a scholarly journal would most likely induce the reviewers to ask: “Where is the empirical evidence?” If standard econometric estimates cannot provide it, the paper is almost certainly rejected. Editors normally immediately desk-reject such papers.

<sup>1</sup> I am grateful for helpful remarks and criticisms to Andre Briviba, Martin Beglinger, Reiner Eichenberger, René L. Frey, Anthony Gullo, Beat Gygi, Margit Osterloh, Angel Serna, Lasse Steiner, David Stadelmann, Tobias Straumann, Alois Stutzer, Benno Torgler, Klaus Wellershoff and Oliver Zimmer.

These demands are close to absolute. As expressed by George Akerlof (2020, p. 411): “The market for academic research, which is in the economics journals, leaves the researchers with no chance but to foresee the dictates of editors and referees”. As a consequence, economics scholars are forced to present empirical evidence when referees and editors demand it – and this is the rule today.

The pressure to provide empirical evidence has even increased over the past years as identifying causality has become increasingly important. In many cases, panel data ranging back as far as possible serve best to deal with causality. This means that data referring to many years back, and often decades, must be used in econometric analyses. However, this pressure raises the issue how relevant the empirical evidence presented is to illuminate contemporary and future problems. Many conditions are likely to have substantially changed over the extended period the data cover. It is difficult, and in most cases, impossible, to take these changes into account in the econometric estimates. As a consequence, the pressure to provide empirical evidence is likely to reduce the extent to which an idea based on such evidence can inform us about policy options arising in the future.

An empirical orientation is certainly an improvement compared to earlier periods in economics when theoretical ideas or speculations were presented without considering whether they are empirically relevant. However, I wish to argue that *the emphasis on empirical validation has been overdone and that this has led to an unfortunate backward orientation of economic science*. Issues relating to the future have been studied too little compared to formal problems of econometric estimation using past data. This has reduced the relevance of economics to society and has opened the room for outsiders, other disciplines and cranks.

The strong pressure not only to publish in top journals but also to provide econometric evidence in the papers submitted to academic journals, has led to four reactions by scholars:

- a. Researchers who want to present an analytical idea collect past statistical data to support it. It is essential to find appropriate data (“data gold digging”) even when the data refer to many years or decades back such that the circumstances of the time can hardly be compared to today.
- b. Many scholars proceed the other way round: They start with existing empirical data and subject them to econometric analyses, or “torture”, until a “reasonable” outcome results. Such behaviour explains the popularity of collections of data covering many years and decades, such as, for instance, the German Socio-Economic Panel.
- c. Economic Researchers often consider only those estimated coefficients that are statistically significant and then provide some “theoretical” rationale. This procedure has been heavily criticized a long time ago (McCloskey, 1985; McCloskey & Ziliak, 1996; Ziliak & McCloskey, 2008). Moreover, there is the activity known as HARKing, i.e. Hypothesizing After Results are Known. This practice seems to occur widely. In many cases, econometric estimation is no longer based on any testable theory but just considers the statistically significant estimated coefficients – and then the researchers ex-post construct a theory compatible with the estimation results.  
(d) Researchers engage in subjects for which statistical data are readily available and sufficiently long to undertake acceptable econometric estimates. It is disregarded that an empirical study can be based on a variety of different sources such as case studies, historical accounts, or narratives (Shiller, 2017).

What matters for my argument is that the analyses that are successfully published are *backward oriented*. The statistical data available necessarily relate to the past, sometimes too many years or even decades back, and therefore may no longer be relevant to present-day problems and issues (see also (Mayer, 2004; Van Dalen & Klammer, 2005; Christensen & Miguel, 2018)). In contrast, it is difficult, and sometimes impossible, to test a novel idea empirically because the corresponding reality does not yet exist.

Moreover, new ideas are almost necessarily less well and stringently formulated than standard ideas, and therefore lend themselves less to empirical validation (Frey, 2003, p. 212). However, it is novel ideas that drive a subject forward. In the case of economics, it is particularly important that unconventional ideas relate to existing problems and issues in the economy and society rather than being small variations of orthodox theories.

This backward orientation due to the need to use data relating to the past is particularly acute for young scholars for three reasons. As already mentioned, young researchers in economics are subject to heavy publication pressure. It is almost impossible today to get a professorial appointment and tenure without having published in refereed, journals (Ellison, 2002a). In order to obtain a professorship at a major university, scholars must have published in one of the top five economics journals. This requirement holds today, even though major figures in our science, including Nobel Prize winners (Akerlof, 2020; Heckman & Moktan, 2020; see also Osterloh & Frey, 2015, 2020) severely criticize this practice.

The second reason why young scholars are particularly affected is that they are more likely to be creative and to present new ideas. Scientific output first increases in the mid-20s, is highest around the late 30s or early 40s, and then undergoes a slow decline with increasing age. A person's best single work tends to appear at about the same age as their maximal output (see, e.g. (Simonton, 2004; Rowena, 2016). Young scholars forced to provide evidence-based on past data may well waste those years in their career in which they potentially are the most creative.

Thirdly, researchers planning to engage in an academic career are likely to be more intrinsically motivated to apply economics to improve the social conditions obtaining in their own country and beyond (Akerlof, 2020, p. 407). If they are forced to follow a trodden path and are unable to express their own ideas without risking their career, they do not select into economic science (Colander & Klamer, 1987; Frey & Eichenberger, 1993). They are more likely to study economic sociology, political science or management.

The publication pressure does, however, not disappear once scholars are tenured. In order to maintain their position in the economic hierarchy, and to attract outside funds, also well-established economic researchers have to make a big effort to publish in top journals. This forces them to submit papers including empirical evidence, often together with younger scholars well trained in exploiting suitable data and the most advanced econometric techniques. Again, content matters less than the formal requirements and advanced identification strategies.

The backward orientation enforced by the academic publication pressure and need to provide empirical evidence is further fueled by the strongly increasing time elapsing between initial submission and final acceptance. Between 1970 and 1999, it has increased by no less than 185 percent, from 6.1 months to 17.3 months, i.e. to almost one and a half year (Ellison, 2002b). If one takes the time of research before submission into account, it is evident that the results published in academic journals often are seriously outdated. A previous dissemination as a working paper, say on SSRN or RePEc, partly solves this long delay. However, a working paper is not subject to a review by academic colleagues, and the content must therefore, be considered preliminary and unchecked. Academic acknowledgment is only attained when the paper has been published in a respectable scientific journal and therewith bolsters academic career prospects.

There are certainly many academic economists who are concerned with the future and champion novel ideas without being able to “prove” these ideas with convincing empirical evidence. They do that mainly in books where there are no referees forcing them to instantly provide empirical evidence for their ideas for the future.<sup>2</sup> However, this road is almost impossible for young scholars who have to demonstrate in scientific journals that they meet the requirements for a scientific career: formal adeptness in mathematics and particularly in econometrics.

This state of economics is the result of the presently existing institutional conditions: The crucial publication requirements demand empirical support for any conceptual idea. What is published in economics papers, therefore is backward oriented as the empirical evidence is necessarily based on data relating to the past.<sup>3</sup>

This paper proposes to allocate more room to analytical ideas without the need to immediately bring forth empirical evidence. Such a change would contribute to economics being more relevant for the future. It is, of course, not argued that publications in academic journals should never contain empirical evidence. My proposal is in line with

---

<sup>2</sup>There are hundreds of books that could be cited. It must suffice to name a few in order to indicate the flavor: Henderson (2020), Buckingham and Goodall (2019), Satz (2010). There are also famous economists using books in order to propose ideas without any instant econometric evidence or fancy identification strategies. Recent examples are Rodrik (2011), Ariely (2012), Shiller (2015), Collier (2018), Posner and Weyl (2018), Piketty (2019/2020).

<sup>3</sup>A somewhat related issue is that the data employed in econometric estimates are constantly corrected and often manipulated by governments and other institutions for their own benefit (Frey 2020, Akerlof & Shiller, 2015).

George Akerlof's (2020, pp. 405-406) plea to allow "soft" arguments rather than solely allowing "hard" arguments. Hard arguments consist of explicit mathematical models and extensive econometric estimates. The arguments are taken to be the harder, the more the math formulates existing orthodox concepts more precisely, and the more advanced econometric is used. In contrast, soft arguments may comprise words, and there is room for historical accounts, narratives, stories, stylized facts, circumstantial evidence, and case studies. In addition, insights from other social sciences such as sociology, political science, ethnology, and history should be considered. Akerlof observes "... evidence of increasing emphasis on hardness", and notes that "... PhD students are taught the hard methods of economic research, mathematical modeling and statistical analysis", leading to a bias against new ideas in economics (Akerlof, 2020, pp. 411-415; see also Akerlof & Shiller, 2010, 2015; Colander, 2019). It has previously been observed that only three percent of economics graduate students at US universities surveyed find it "very important to have a thorough knowledge of the economy" (Colander & Klamer, 1987, p. 100). Rather than content, what counts is knowledge of formal methods.

## 2 | HOW MUCH CAN WE LEARN FROM THE PAST?

There are important counterarguments to what has been argued in the previous section. They have to be taken seriously.

The most important counterargument certainly is that economics should learn from history. To employ data of the past helps to understand the future. Indeed, it has been argued that this is absolutely essential. Winston Churchill said: *"Those that fail to learn from history, are doomed to repeat it."*

It would be foolish to negate this view. History indeed helps us to better understand future events. This also applies to economics that deals with human behaviour. Intellectuals argue that is therefore possible to predict human behaviour provided the conditions (especially the economic constraints) are reasonably well known. Among others, Machiavelli championed this view: *"Whoever wishes to foresee the future must consult the past; for human events ever resemble those of preceding times. This arises from the fact that they are produced by men who ever have been, and ever shall be, animated by the same passions, and thus they necessarily have the same results."*

Nevertheless, there are problems with this view. Firstly, the knowledge of the past is far from being clear but is rather marred by many uncertainties. Interpretations of the past are much influenced by the zeitgeist, and when the zeitgeist changes, so does the interpretation of the past. This uncertainty also applies to the collection of the data. They capture only selected aspects of the past.

Machine Learning Algorithms dealing with big data increasingly used in economics also depend on past evidence. Such algorithms constitute a significant step ahead. However, they are confronted with the issue that these data reflect conditions of the past. A well-known example relates to medical knowledge based on almost exclusively male subjects. It has been shown that many of these results scarcely apply to women. This problem also applies to many more domains, in which the data are influenced by the conditions obtaining in the past, no longer relevant for the present and the future.

Moreover, the conditions likely to prevail in the future are only vaguely known or not known at all. It is, therefore, open how individuals will act in the future, and how the economy as a whole will evolve. Observations in the field have increasingly been substituted by laboratory experiments. Using this approach, researchers have been able to generate data to test analytical theories. The scientific understanding to what extent the experimental insights can be used for economic policy is seriously missing. These experiments concentrate on one specific intervention in the economy and do not consider the aggregate reaction processes, and the new equilibria emerging. Experimentalists rarely confront the issue of whether the research results scale to larger markets or different settings, in particular to future policy issues. The "scale-up" problem deals with the question of how the insights gained by experiments are useful to understand and predict the new aggregate equilibria produced in different situations, and in particular in the future (see the careful discussion in (Al-Ubaydli et al., 2020; List, 2020). The aggregate

outcome must take into account the reactions to a policy intervention informed by an experimental effect. Such reactions may be due to individuals, other policy makers, enterprises and interest groups. Not only the benefits so far considered but also the costs of a policy intervention have to be taken into account. Such studies require a great amount of additional research so far lacking.

More generally, it is unclear what exactly can be learned from history. We may well learn the wrong things. A more extreme position argues that it is not possible to learn from history at all. As quipped by George Bernard Shaw, “*We learn from history that we learn nothing from history.*” It is in any case difficult to know what can be learned because the world is constantly changing; what was correct in the past may be totally wrong in the future.

Examples of such uncertainty abound. It suffices to look at the outbreak of the Corona Pandemic at the beginning of 2020. Who has foreseen that governments would impose heavy restrictions on the economy provoking downfalls in economic activity of around 10 percent of GNP, and raising unemployment by many million persons (e.g. in the United States around 20 million)? Who has foreseen that some branches of the economy – such as air transport and airports – which before the pandemic were growing quickly, suffered a reduction in activity by 90 percent and more? Who would have predicted that individuals are forbidden to cross national borders, and in some countries, even local borders between provinces and regions? These fundamental changes in conditions (constraints) resulted in a strong change in individual behavior and in the overall economy.

The impact of CoVid-19 is an impressive recent case in which the future is very different from the past. Such development was not foreseen, certainly not by looking at past data. As a result, governments became ill-prepared to meet the new conditions. There are, of course, some scholars and laypersons who predicted similar developments (examples are Smil, 2012; Osterholm & Olshaker, 2017; or Webster, 2018). It is well known that Bill Gates has been warning of a pandemic for years – but without being able to indicate when it would occur.

These insights were certainly not generally acknowledged and, more importantly, did not induce governments to prepare for these catastrophic developments. In particular, such evolutions were rarely, if ever, predicted in academic papers in economics. In many cases, the predictions have been formulated ex-post, i.e. once the results had happened. There are strong incentives to present such ex-post predictions; it provides academic respectability, but more generally seems to be part of human nature.

Provided the world, and in particular, the economy changes only to a limited extent, ideas based on past data may still prove to be useful to deal with in the future. As David Hume states: “*Mankind are so much the same, in all times and places, that history informs us of nothing new or strange ... Its chief use is only to discover the constant and universal principles of human nature.*” However, one may argue that it is better to know nothing about the future than to have a distorted and perhaps even totally wrong picture of the future because they invite mistaken and hugely costly government responses. Barry Eichengreen (2015) provides an instructive account of the uses and misuses of history. The Great Depression of the 1930s and the Great Recession, which began in 2008, occurred against the backdrop of sharp credit booms, dubious banking practices, and a fragile global financial system. When markets drastically dropped in 2008, policymakers used the lessons of the Great Depression trying to avert the worst. While their response prevented a financial collapse and catastrophic depression like that of the 1930s, unemployment in the U.S. and Europe still rose to very high levels, and the economic cost and suffering were intensive.

While there are serious reasons why analyzing the past – reflected in statistical data – might be necessary to predict the future, the existing sole emphasis on direct empirical evidence is highly problematic.

### 3 | WHAT CAN BE DONE?

There are various possibilities to reduce the overemphasis on immediately presenting empirical evidence based on past data.

1. The intense publication pressure imposed on young scholars can be reduced. The absolute requirement to publish in top journals and to instantly present empirical evidence can be discontinued.

Such a publication policy requires a fundamental change in the established norms among professional economists (Merton, 1973; Fuchs et al., 1998; Blaug, 2001; Van Dalen, 2019). To change strongly internalized norms is difficult to achieve. It is particularly unlikely to happen within a discipline that believes to be at the top of the pecking order within the social sciences, and which looks down to political scientists and sociologists using less advanced analytical methods (Colander, 2015; Fourcade et al., 2015).

2. Universities can specialize themselves by supporting innovative ideas, particularly coming from the next generation of academics. This again requires a fundamental change in thinking and policy.

A policy change can enable universities to become prominent: a place where novel ideas are welcome and supported. Existing universities are likely to resist such policy change because they are heavily bureaucratized, and the professors are subject to “intellectual incest” (Colander, 2015). Those university academics who have attained a satisfactory position in the hierarchy within the traditional system, are strongly interested in maintaining this system because it gives them legitimacy and prominence. A real change therefore it is unlikely to happen.

3. The inability of the academic establishment opens a possibility for new institutions to focus on novel ideas, and to attract researchers interested in new and unconventional ideas. The same holds for academic institutions below universities (such as the Fachhochschulen and berufsbildende Schulen in German-speaking countries) that can become known exactly because they deviate from the current economic orthodoxy, and because they are more focused on real issues in the economy and society.
4. Academic journals can drop the need to support analytical ideas by empirical (econometric) evidence. The editors can inform the reviewers that the argument “Where is the empirical evidence?” is not acceptable. Instead, the referees should concentrate on how interesting and valuable an argument is, and how much it contributes to our knowledge about the economy. Referees must seriously deal with the content of a submitted article rather than just criticizing insufficient empirical foundations.
5. Such a change in the refereeing process could provide a chance for scholarly journals to distinguish themselves from those clinging exclusively to the old-established need for empirical evidence. Specialized academic journals can be established with an explicit policy focusing on content, and not focusing on empirical evidence. Such journals may possibly attain a unique and well-regarded position.
6. Publications in scientific outlets abstaining from censoring content, such as SSRN or RePEc, or various economics blogs, can be accepted as valuable contributions to economic science. An additional possibility is to publish future-oriented papers in a book collection. An example is “21<sup>st</sup> Century Economics. Economic ideas You Should Read and Remember” (edited by Frey & Schaltegger, 2019). The authors were free to write what they think to be relevant for economics in the future. What matters in both publication outlets that new insights matter rather than technicalities and orthodox presentation.
7. Academic journals can choose the submitted paper to be published using focal random procedures (Osterloh & Frey, 2020): Papers considered excellent by the editor as well as by the referees should be immediately published as they are. Those papers taken to be of insufficient quality by all are to be immediately rejected. The many papers lying between these extremes can be subjected to a random draw. This procedure provides publication options to scholars wishing to offer a novel conceptual idea. The traditional approach and the random approach can be compared after some years. It may well be that the papers accepted by lot gain as much, or perhaps even more, attention than those selected by referees.

8. Publications in unorthodox journals, even if lowly ranked according to accepted criteria, can be taken more seriously. They can at least be part of the credentials of young scholars engaging in an academic career. It could even be argued that universities should only hire scholars who had published at least two creative papers.

Accordingly, the current tenure and promotion process has to be fundamentally changed. To have published in a top journal must not be taken as evidence that it is a worthwhile contribution. Judging the value of a paper appearing in an academic journal with a high impact factor is an inappropriate argument (see (Osterloh & Frey, 2015). Instead, the members of an evaluation committee should discuss the merits of a paper itself.

9. One way to change the general views and norms in economics consists of emphasizing that the requirement for empirical support is overdone. A more balanced view is required if economics wants to make valuable contributions to society in the future.

While such arguments are necessary, they are most likely not to be sufficient. Competition between various scholarly institutions may become relevant. It may be effective to refer to the danger that presently more lowly ranked academic institutions and research centers may overcome the strong tendency to use “outdated data”. This would allow them to derive insights on how the present, and possibly the future, economy works. The public much demands such insights.

The discussion how to overcome the backward-oriented mode of present economics indicates that there are quite a number of possibilities, but that it is not at all clear whether the overemphasis on empirical evidence can be overcome.

## 4 | CONCLUSIONS

Four major arguments are the pillars of this paper:

1. There is intense pressure to publish in professional journals particularly affecting young scholars wanting to engage in an academic career;
2. Papers submitted to economics journals have to provide empirical (econometric) evidence;
3. The statistical data required to meet this requirement relate to the past, often to many years or decades back. The backward-orientation of economic research results has further enhanced the backward orientation of economic research.
4. As a result, economics is backward oriented as the statistical data necessarily relate to the often far away past, and therefore the (econometric) evidence is of most questionable value. Insights for future economic policy tend to be disregarded.

To overcome this undue concentration on econometric estimates relating to the past, more room should be opened for novel, creative ideas. The present situation is far away from what, for instance, Joseph Schumpeter contributed to economics. He can be called a “Prophet of Innovation” (McCraw, 2007). Case studies and narratives should be more directed to problems and issues of the present and the future. In his article on “Narrative Economics” Shiller (2017, p. 971) speaks of “Narratives as creative ...works”. Akerlof (2020), another Nobel Prize winner, convincingly argues that the strong emphasis on “hard” versus “soft” approaches in economics has high cost, and that grave “Sins of Omission” result. His plea for a larger role of “soft” research is in line with the arguments here advanced. The need to support arguments in a paper by econometric evidence may be considered part of “hard” science that tries to imitate the natural sciences.

A change in the orientation of standard economics is, however, unlikely to happen. This pessimistic conclusion is partly based on the observation that the related fundamental critique of the “Tyranny of the Top Five” (Heckman & Moktan, 2020) so far has had little or no effect on the requirements imposed for an academic career in economics.<sup>4</sup> The more far-reaching change here proposed is even less likely to have an effect.

Economics is a well-established science. Most economic academics have made their careers following the creed of standard economics. They would lose part of their prominence if far-reaching changes were undertaken. Moreover, there is by now a large bureaucracy in universities and elsewhere attuned to, and set to reproduce, this orthodoxy.

A competitive process may induce the general public and economic and social decision-makers to turn to publications more concerned with the future. They may turn to outsiders, be it unorthodox economists, and even more likely cranks, who address current and future issues. They may increasingly disregard the publications in academic economics because they consider them to be outdated. In that case, conventional data-driven economics will survive, but risks to become more and more irrelevant for future economic policy.

## REFERENCES

- Akerlof, G. A. (2020). Sins of omission and the practice of economics. *Journal of economic literature*, 58(2), 405–418. <https://doi.org/10.1257/jel.20191573>
- Akerlof, G. A., & Shiller, R. J. (2010). *Animal spirits: How human psychology drives the economy, and why it matters for global capitalism*. Princeton University Press.
- Akerlof, G. A., & Shiller, R. J. (2015). *Phishing for phools: The economics of manipulation and deception*. Princeton University Press.
- Al-Ubaydli, O., Lee, M. S., List, J. A., Mackevicius, C. L., & Suskind, D. (2020). How can experiments play a greater role in public policy? Twelve proposals from an economic model of scaling. *Behavioural public policy*, 1–48. <https://doi.org/10.1017/bpp.2020.17>
- Blaug, M. (2001). No history of ideas, please, we're economists. *Journal of economic perspectives*, 15(1), 145–164.
- Christensen, G., & Miguel, E. (2018). Transparency, reproducibility, and the credibility of economics research. *Journal of economic literature*, 56(3), 920–980. <https://doi.org/10.1257/jel.20171350>
- Colander, D. (2008). The making of a global European economist. *Kyklos*, 61(2), 215–236.
- Colander, D. (2015). Intellectual incest on the Charles: Why economists are a little bit off. *Eastern economic journal*, 41(2), 155–159. <https://doi.org/10.1057/ej.2014.78>
- Colander, D., & Klamer, A. (1987). The making of an economist. *Journal of economic perspectives*, 1(2), 95–111. <https://doi.org/10.1257/jep.1.2.95>
- Colander, D. C. (2019). *Where economics went wrong: Chicago's abandonment of classical liberalism*. Princeton, Oxford: Princeton University Press.
- Eichengreen, B. (2015). *Hall of mirrors: The great depression, the great recession, and the uses and misuses of history*. Oxford: Oxford University Press.
- Ellison, G. (2002a). Evolving standards for academic publishing: A q - r theory. *Journal of political economy*, 110(5), 994–1034. <https://doi.org/10.1086/341871>
- Ellison, G. (2002b). The slowdown of the economics publishing process. *Journal of political economy*, 110(5), 947–993. <https://doi.org/10.1086/341868>
- Fourcade, M., Ollion, E., & Algan, Y. (2015). The superiority of economists. *The journal of economic perspectives*, 29(1), 89–113. <https://doi.org/10.1257/jep.29.1.89>
- Frey, B. (2003). Publishing as prostitution? – Choosing between One's own ideas and academic success. *Public choice*, 116(1), 205–223. <https://doi.org/10.1023/A:1024208701874>
- Frey, B. S., & Eichenberger, R. (1993). American and European economics and economists. *The journal of economic perspectives*, 7(4), 185–193. <https://doi.org/10.1257/jep.7.4.185>
- Frey, B. S., & Schaltegger, C. A. (2019). *21st century economics: Economic ideas you should read and remember*. Cham: Springer International Publishing.
- Fuchs, V. R., Krueger, A. B., & Poterba, J. M. (1998). Economists' views about parameters, values, and policies: Survey results in labor and public economics. *Journal of economic literature*, 36(3), 1387–1425.

<sup>4</sup>Heckman and Moktan's paper has been circulated for several years before being published in June 2020 in the *Journal of Economic Literature*. The universities thus had access to the information for a considerable time – and could have changed their requirements.



- Heckman, J. J., & Moktan, S. (2020). Publishing and promotion in economics: The tyranny of the top five. *Journal of economic literature*, 58(2), 419–470. <https://doi.org/10.1257/jel.20191574>
- List, J. A. (2020). Non est disputandum de generalizability? a glimpse into the external validity trial. *NBER Working Paper* (w27535).
- Mayer, T. (2004). Dry holes in economic research: Comment. *Kyklos*, 57(4), 621–626.
- McCloskey, D. N. (1985). The loss function has been misled: The rhetoric of significance tests. *The American economic review*, 75(2), 201–205. <https://doi.org/10.2307/1805596>
- McCloskey, D. N., & Ziliak, S. T. (1996). The standard error of regressions. *Journal of economic literature*, 34(1), 97–114.
- McCraw, T. (2007). *Prophet of innovation: Joseph Schumpeter and creative destruction*. Harvard University Press.
- Merton, R. K. (1973). *The sociology of science: Theoretical and empirical investigations*. University of Chicago press.
- Osterholm, M. T., & Olshaker, M. (2017). *Deadliest enemy: Our war against killer germs*. Hachette UK.
- Osterloh, M., & Frey, B. S. (2015). Ranking games. *Evaluation review*, 39(1), 102–129. <https://doi.org/10.1177/0193841x14524957>
- Osterloh, M., & Frey, B. S. (2020). How to avoid borrowed plumes in academia. *Research policy*, 49(1). <https://doi.org/10.1016/j.respol.2019.103831>
- Rowena, K. (2016). Does creativity decline with age? *Scientific American mind*, 27(2), 70. <https://doi.org/10.1038/scientificamericanmind0316-70a>
- Shiller, R. J. (2017). Narrative economics. *American economic review*, 107(4), 967–1004. <https://doi.org/10.1257/aer.107.4.967>
- Simonton, D. K. (2004). *Creativity in science: Chance, logic, genius, and zeitgeist*. Cambridge: Cambridge University Press.
- Smil, V. (2012). *Global catastrophes and trends: The next 50 years*.
- Van Dalen, H. P. (2019). Values of economists matter in the art and science of economics. *Kyklos*, 72(3), 472–499. <https://doi.org/10.1111/kykl.12208>
- Van Dalen, H. P., & Henkens, K. (2012). Intended and unintended consequences of a publish-or-perish culture: A worldwide survey. *Journal of the American Society for Information Science and Technology*, 63(7), 1282–1293. <https://doi.org/10.1002/asi.22636>
- Van Dalen, H. P., & Klamer, A. (2005). Is science a case of wasteful competition? *Kyklos*, 58(3), 395–414. <https://doi.org/10.1111/j.0023-5962.2005.00294.x>
- Webster, R. G. (2018). *Flu hunter: Unlocking the secrets of a virus*. Otago University Press.
- Ziliak, S., & McCloskey, D. N. (2008). *The cult of statistical significance: How the standard error costs us jobs, justice, and lives*. University of Michigan Press.

**How to cite this article:** Frey Bruno S. Backward-Oriented Economics<sup>1</sup>. *Kyklos*. 2021; Volume 74, Issue 2, Pages 187 - 195. <https://doi.org/10.1111/kykl.12263>