



## Retreat from normativism

Marcel Boumans

To cite this article: Marcel Boumans (2021): Retreat from normativism, Journal of Economic Methodology, DOI: [10.1080/1350178X.2020.1868773](https://doi.org/10.1080/1350178X.2020.1868773)

To link to this article: <https://doi.org/10.1080/1350178X.2020.1868773>



© 2021 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group



Published online: 07 Jan 2021.



Submit your article to this journal [↗](#)



Article views: 249




View related articles [↗](#)



View Crossmark data [↗](#)

## Retreat from normativism

Marcel Boumans 

Utrecht University, Utrecht, The Netherlands

### ABSTRACT

In economic methodology, a complete turn to practice is hampered by a broadly shared normative stance towards practice. The root of this normativism is Platonism. Platonism presupposes in essence a well-ordered and harmonious world, which allows for neat categorizations and norms that are implied by them. Science, however, is messy, and any categorization is in the eye of the beholder. This article argues therefore for a more instrumental use of a-posteriori categorizations that are based on empirical studies of the practice in question.

### ARTICLE HISTORY

Received 2 September 2020  
Accepted 25 November 2020

### KEYWORDS

Dirt; essentialism;  
nominalism; normativism;  
Platonism; turn to practice;  
universalism

### JEL

Codes: B40

In the last few decades, one can observe – in general – a move to practice, not only in economic methodology but also in the larger field of philosophy of science. It is, however, still a ‘move’ and not yet the ‘turn’ as was hoped for, e.g. by Hands (2001). There appears namely to be a tension, or perhaps even a gap, between methodology and practice, for which one can aptly apply the well-known expression of Blaug (1980) that economists ‘do not practice what they preach,’ or the diagnosis by Hausman (1992, p. 7) of ‘methodological schizophrenia’, namely that ‘methodological pronouncements and practice regularly contradict one another’. Both are normative statements, judging practice and not methodology, according to which economists are either sinners or schizophrenic. Economic practice seems to fail, not only in the eyes of these philosophers but also more generally in economic methodology. There appear to exist norms for science practice which are not met. But while practice is being questioned, it seems that these norms are taken for granted.

If philosophy of science is indeed the philosophical analysis of the practice of science – which according to me should be the case – it is important to study these norms that impede a complete turn to practice. What are these norms, where do they come from and what legitimates them? In the case of Mark Blaug, it was clear they were the Popperian norms of falsificationism, a philosophical framework that today is only embraced by a few philosophers. Although Hausman (1992, p. 329) admits that ‘there is no well-founded general philosophical system to resolve the methodological difficulties of economics’ and that ‘one must address the problems of economic methodology by studying economics’, he nevertheless assumes that for this study a formal empiricist framework is needed with general definitions of concepts like ‘explanation’, ‘model’ and ‘theory.’ Even if these definitions are based on an empirical study of economics, they are subsequently used to ‘assess’ various practices in economics for their scientific credibility. It seems that there is no escape from normativism in economic methodology, which makes that the turn to practice is not yet completed.

**CONTACT** Marcel Boumans  m.j.boumans@uu.nl  Utrecht University, Kriekenpitplein 21-22, 3584 EC Utrecht, The Netherlands

I believe that economic methodology should leave behind as much as it can this authoritarian normative stance and should embrace a much more positivist attitude. To reach that goal, economic methodology should understand the foundations of this self-proclaimed authority. This paper aims to study them. As a starting point, I will make use Woody's (2014) very suitable reflection on the status of representation and explanation 'after the turn to practice'. Woody claims that the 'turn to practice' is a 'retreat from a certain sort of abstraction'. The 'retreats' she distinguishes are:

- *Conception to representation*: Theories originally treated as abstract conceptual objects characterized predominantly by their logical structure are replaced by theories as artefacts, rendered as particular representations, whether linguistic, diagrammatic or other, and associated with distinct sets of models. [...]
- *A priori to empirical*: Concepts such as evidence and explanation, long considered crucial for articulating the distinctive nature of science, were traditionally cast at the highest level of generality and analysed in terms of syntactic and semantic conditions generated through a-priori analysis. The turn to practice has replaced these analyses with accounts based on the examination of the reasoning invoked by scientists in particular contexts that arise in their ongoing work. Here the methodological shift is at once from a-priori principles whose normative weight is presumed to empirical investigations that must extract, and justify judgements of, the normative, and also from a top-down assumption of generality to a bottom-up construction out of the local and the contextual. In addition, the intention to situate philosophical analysis meaningfully within practice entails being respectful of, and accountable to, the categories of practice operative within the relevant scientific communities.
- *Ideal agent to human practitioner*: Drawing on empirical studies of reasoning, the turn to practice has replaced the ideal agent with something closer to an actual practitioner. [...]
- *Knowing subject to social epistemology*: The inherently social nature of contemporary scientific practice introduces a variety of issues surrounding how knowledge is generated and transmitted, how knowledge is intertwined with issues of authority, expertise, trust and divisions of labour, and how social structure is established and perpetuated to coordinate community deliberation and action (Woody, 2014, pp. 123–4).

This four-dimensional 'turn to practice' faces, according to Woody, distinct and substantial challenges. The first concerns how to avoid getting stuck at a level of particularity that evades any reasonable effort to generalize. 'While we do not want to assume uniformity in science nor essentialize in ways that cannot be justified and are potentially distorting, case studies typically would be of little value if they shed no light on activities and communities beyond those directly considered' (pp. 124–125).

The second challenge is the subject of this paper.

When we build philosophy on strong intuitions and a priori reasoning, in effect, the normativity of the analysis is built in from the get-go. But when we turn to practice and take it seriously, there is a possibility that we will all become social scientists of a more thoroughly descriptive stripe, that is, more akin to certain anthropologists and sociologists, only without the proper training and skill set. What is required, instead, is a framework for analyzing practice that allows us to make assessments regarding the effectiveness or appropriateness of particular practices or actions situated within those practices (Woody, 2014, p. 125).

It is particular the second dimension, a priori to empirical, that I wish to explore in economic methodology in the light of these two challenges: to get from the particular to the general without assuming uniformity and essentializing, and nevertheless to get beyond descriptivism. To me it seems that in current economic methodology, the move in the three other dimensions has become more or less common 'turns', but not the second. With respect to theory, model, evidence, explanation, they are still conceptualized or defined as universal categories, that is to say, 'taken out

of context'. In this contribution, I would like to emphasize the historicity and locality, the particularity, of any standard or norm.

## 1. Retreat from purity

The root of the authoritarian normative stance in philosophy of science is Platonism. Platonism is deeply ingrained in philosophy of science, in particular its tenets of universalism and essentialism. I see this as the main obstacle for a real philosophy of science, that is to say a philosophy of science in practice. Universalism creates a tension between the studies of particular practices, which are local both in space and time, and the search for general claims that do not only yield for these particular practices, but are considered to be essential for research practices. This essentialism implies normativity: the detected essential aspects of a specific kind of scientific research, such as an experiment, are equated with the norms for such research, that is to say how this research should be done.

Universalism presents itself in philosophy of science by insisting that the concepts to be used for the analysis of research practices, such as 'model' and 'explanation,' need to be defined in a clear, concise and unambiguously way. By these definitions, like Plato's Forms, the concepts are transcended from the real world to an abstract world of ideas, where they exist purified from any idiosyncrasy or particularity. This purification, however, is not arrived at by cleaning, these definitions are not arrived at by abstraction in the sense of removal. They are Kantian synthetic a-priori categories.

To enable a complete 'retreat from abstraction' by focussing on how to get from 'a-priori to empirical', I discuss the ideas of several philosophers who have been criticizing Platonism in its various different appearances and have suggested alternatives to universalism in philosophy of science.

The first philosopher of science who took a clear position against the essentialist nature of universalism is Karl Popper. In his article, 'Poverty of Historicism' (1944) and his book *The Open Society and its Enemies* (2002, first published in 1945), he characterized 'methodological essentialism' as the view 'that it is the task of pure knowledge or 'science' to discover and to describe the true nature of things, i.e. their hidden reality or essence' (1944, p. 33). Essences are aspects of the world that do not change in time or in place – they are universal in this respect. According to Popper, essentialism not only believes in the existence of universals, it also stresses their importance. 'Singular objects, it points out, show many accidental features, features which are of no interest to science. ... Science must strip away the accidental and penetrate to the essence of things. But the essence of anything is always something universal' (1944, pp. 94–5).

According to this methodological essentialism, there are three ways of knowing: 'we can know [a phenomenon's] unchanging reality or essence; and that we can know the definition of the essence; and that we can know its name' (2002,, p. 34). Typical essentialist questions are 'what is matter?' or 'what is force?' or 'what is justice?' It is believed, according to Popper, that a penetrating answer to such questions reveals the essential meaning of these terms and thereby the true nature of the essences denoted by them.

In opposition to this essentialism, Popper proposed what he called 'methodological nominalism': 'methodological nominalism aims at describing how a thing behaves in various circumstances, and especially, whether there are any regularities in its behaviour' (2002, p. 34). And therefore, the questions will also be different, they will more of the kind of 'how' questions. As a consequence, Popper saw a different role for language as an 'instrument of scientific description': words are considered as 'subsidiary tools for this task, and not as names of essences' (p. 34). Popper saw the task of science to describe the behaviour of phenomena and suggested that 'this is to be done by freely introducing new terms wherever necessary, and by re-defining old terms wherever convenient, to the utter neglect of their original meaning, words being merely regarded as useful instruments of description' (1944, p. 95).

Imre Lakatos shared many of Popper's ideas, including his anti-Platonism. Platonism is the dominant view in mathematics, and his *Proofs and Refutations* was written to oppose this dominance. The criticism in the book was not directed at Platonism itself but at its consequence, namely formalism, 'the school of mathematical philosophy which tends to identify mathematics with its formal axiomatic abstraction' (Lakatos, 1976, p. 1) and the methodology that belongs to it, the deductivist approach.

The criticism was targeted at the fact that formalism disconnects mathematics from any history. 'None of the "creative" periods and hardly any of the "critical" periods of mathematical theories would be admitted into the formalist heaven, where mathematical theories dwell like the seraphim, purged of all impurities of earthly uncertainty' (Lakatos, 1976, p. 2). The book's aim therefore was to show that 'mathematics does not grow through a monotonous increase of the number of indubitably established theorems but through the incessant improvement of guesses by speculation and criticisms, by the logic of proofs and refutations' (p. 5).

The core of the explored logic of discovery in mathematics is the heuristics of 'concept formation'. Mathematical concepts are not pre-existing Platonist Forms, but are developed in the course of theory formation. It is a continuous process of defining and 'stretching' concepts. 'Concepts and definitions – that is, intended concepts and unintended definitions – can then play funny tricks on each other!' (Lakatos, 1976, pp. 85–86).

In a discussion of Lakatos's book, two years after its publication, David Bloor aptly captures the core of Lakatos's anti-Platonism. According to Bloor (1978, p. 248), Platonists 'usually think that mathematical objects or structures fall into a unique, natural set of kinds or sorts, as if there are specially privileged "real" boundaries which demarcate different kinds of thing'. But Lakatos rejects this.

He treats mathematical 'kinds' as being our creations. We draw the boundary lines. Classification is our achievement and our problem. Nothing is to be gained by seeing different boundary lines as more or less corresponding to the 'real' ones. But this is not all. For Lakatos the world is so densely populated by objects of all shapes and sizes, and there are so many imaginable procedures that can be based on them, that there is an indefinitely large number of different boundaries that we might reasonably draw. (Bloor, 1978, p. 248)

Bloor (1978) not only discussed Lakatos's view but also Mary Douglas's, who had developed a very similar view in her (2002, first published in 1966) book *Purity and Danger*. According to her, purity is the aim of a rigorous ordering in categories. Although these categories are human creations, the ordering is presented as something sacred; aberration from it is dangerous. 'Purity is the enemy of change, of ambiguity and compromise. Most of us indeed would feel safer if our experience could be hard-set and fixed in form' (Douglas, 2002, p. 200). But despite the safety an ordering seems to provide, it does not guarantee that our experience can be fitted into it rigorously. 'The final paradox of the search for purity is that it is an attempt to force experience into logical categories of non-contradiction. But experience is not amenable and those who make the attempt find themselves led into contradiction' (p. 200).

## 2. The scientific workplace is dirty

Platonism and essentialism presuppose in essence a well-ordered and harmonious world, which allows for a neat categorization, and where definitions can indeed have the epistemological role of peepholes to this hidden world. This idea that the scientific endeavour is the discovery of that world is most explicitly challenged by Nancy Cartwright, particularly in her (1999) book, *The Dappled World*.

*The Dappled World* adopts a specific 'scientific attitude' which presupposes that the world around us is a 'messy, mottled world' which cannot be 'covered' by 'one great scientific theory into which all the intelligible phenomena of nature can be fitted, a unique, complete and deductively closed set of precise statements' (p. 6).

This scientific attitude has consequences for the epistemological role of concepts, namely the kind of ‘closure’ they can provide. Closure appears when concepts have the following features: they are unambiguous, that is, ‘there are clear criteria that determine when they obtain and when not’ (p. 7); they are precise, that is they can be given ‘quantitative mathematical representations’ (p. 7); they are non-modal, in the sense that ‘they do not refer to facts that involve possibility, impossibility or necessity, nor to ones that involve causality’ (p. 7); and finally they have exact relations among themselves.

In a messy world closure is of a ‘narrowly restricted kind’ (p. 7). If closure of the concepts of a theory should be supported by the empirical success of that theory, then Cartwright shows that this success is of a ‘severely limited’ scope (p. 9). In this respect, the world is dappled.

Cartwright discusses physics and economics, hence theories about economic or physical phenomena. But her proposed scientific attitude can of course also be applied to the study of science itself. In *The Dappled World*, however, there is no such reflection on philosophy of science. For the application of such an attitude with respect to the study of the scientific world one has to consider the work of Mary Morgan. The messy world explored by Morgan is the world of science; ‘science is messy’ – as she states in the first sentence of her book (2012) *The World in the Model*. To do justice to this messiness of science, Morgan does not aim at a ‘fortified philosophy,’ but instead of a ‘naturalized philosophy of science for economics’: ‘The messy details are important – not just because, as we know, bald narratives lack credibility, but rather because the devil is often in the detail, and thus larger, and important, matters cannot be understood and explained without them’ (p. vx).

Typical for Morgan’s approach is a carefully chosen vocabulary.<sup>1</sup> The study of a certain research practice is highly sensitive to not only the choice of concepts used for that purpose but also to the way these concepts are used. They can restrict or even simplify the analysis too much, particularly when they themselves are restricted by a definition.

The main tool of philosophy is language, but it is a problematic tool. Clarification can come from clear concepts, often meant in the sense of being clearly defined. However, when it comes to this kind of analysis, definitions can limit the study of a practice unnecessarily. One reason is that practitioners often use concepts in a much looser way than philosophers have defined them, and even if they accepted the philosopher’s definitions, these more strictly defined concepts do not easily translate to another practice. This is the reason, I believe, you will not find any commitment to a definition in Morgan’s work, not even for the subject she became most well known for: models. Instead, Morgan discusses the ways in which models function in science and the qualities that make them useful in science. This attitude determines also the kind of questions that are considered to be relevant. In Morgan’s work, the ‘how’-questions are dominant, and the ‘what’-questions non-existent.

Even though concepts should not be closed down by a definition and should instead remain open-ended, the analysis of each practice requires an own working vocabulary to gain the desired insights. In many, if not all, of Morgan’s projects, it involved the development of a specific vocabulary appropriate for that project.

### 3. Conclusion: praise for nominalism

What these philosophers share is nominalism, that is, a non-Platonist view on the epistemological role of concepts as well a specific world view that is connected to it. If the world is well-ordered and harmonious, a closed system – in the sense of Cartwright’s closure – has the ability to tell the truth about that world. But what if the world is not like that, how then to study messiness? Concepts are no windows to the truth anymore, they have a different role. They have the function of helping researchers to tidy up that messiness, to sort that world till the achieved order makes sense to them. It is an instrumentalist attitude to vocabulary, and as a result to theories.

Douglas (2002) developed a relevant account for understanding the process of cleaning, not in the sense of removal (purification), but as ordering. According to Douglas, dirt is essentially disorder:

'There is no such thing as absolute dirt: it exists in the eye of the beholder. [...] Dirt offends against order. Eliminating it is not a negative movement, but a positive effort to organize the environment' (p. 2). In chasing dirt when tidying we are 'positively re-ordering our environment, making it conform to an idea [...] it is a creative moment, an attempt to relate form to function, to make unity of experience' (p. 3). Douglas emphasizes that the identification of dirt should not be considered as a unique, isolated event. 'Where there is dirt there is system. Dirt is the by-product of a systematic ordering and classification of matter, in so far as ordering involves rejecting inappropriate elements' (p. 44). Cleaning is the reaction which condemns any object or idea likely to confuse or contradict cherished classifications, thus 'reducing dissonance' (Douglas, 2002, p. 340). Thus cleaning is part of the epistemological activity of systematization, such as ordering and classification.<sup>2</sup>

The required framework, suggested by Woody, for analysing practice that allows us to make assessments regarding the effectiveness or appropriateness of particular practices, is not a uniform and general applicable framework. For each kind of practice, a dedicated framework needs to be developed with its own vocabulary. Each framework is a different ordering system with its own classifications appropriate to the practice under study. Tidying up in a kitchen is different from tidying up in a bedroom, despite that both are rooms in the same house.

The empirical approach has become more dominant in economic methodology, one sees indeed more studies of actual practices been published, also in this journal. But the attitude with which many of these studies are done is still essentialist: the aim is abstraction, in the sense of purification, to arrive at concepts that define the essences of that practice exactly. Although these concepts are indeed accepted as not a-priori given but abstracted from empirical science studies, they still are seen as universals. Because they are assumed to capture the essences of science, they function as norms for which all practices can only be dirty.

The admittance in economic methodology that dirt is only in the eye of the beholder will enable the removal of the last obstacle – normativism – to complete the turn to practice.

## Notes

1. The following description of Morgan's approach is an excerpt from a broader discussion of her approach one can find in her work and projects with her 'students' (Boumans, 2018).
2. An example of an application of Douglas's account to study practice is the investigation of two practices of data preparation, one in biology and the other in economics, by Boumans and Leonelli (2020).

## Disclosure statement

No potential conflict of interest was reported by the author.

## Notes on contributors

**Marcel Boumans** is Pierson Professor History of Economics at Utrecht University. His main research focus is on understanding empirical research practices in social science from a combined historical and philosophy perspective. He is particularly interested in the practices of measurement and modelling and the role of mathematics in social science. Because models are not complete as sources of knowledge for sciences outside the laboratory, additional expert judgments are needed. This is the topic of his most recent monograph *Science Outside the Laboratory* (OUP, 2015). His current research project 'Vision and Visualisation' focuses on exploring how expert judgments (views) are made and how they could be validated, particularly in those research practices where visualisations are made or used.

## ORCID

Marcel Boumans  <http://orcid.org/0000-0002-8717-8148>

## References

- Blaug, M. (1980). *The methodology of economics, or how economists explain*. Cambridge University Press.
- Bloor, D. (1978). Polyhedra and the abominations of leviticus. *The British Journal for the History of Science*, 11(3), 245–272. <https://doi.org/10.1017/S000708740004379X>
- Boumans, M. (2018). Introduction to the symposium “curiosity, imagination, and surprise”. *Research in the History of Economic Thought and Methodology*, 36B, 3–10. <https://doi.org/10.1108/S0743-41542018000036B001>
- Boumans, M., & Leonelli, S. (2020). From dirty data to tidy facts: Clustering practices in plant phenomics and business cycle analysis. In S. Leonelli & N. Tempini (Eds.), *Data journeys in the sciences* (pp. 79–101). Springer.
- Cartwright, N. (1999). *The dappled world. A study of the boundaries of science*. Cambridge University Press.
- Douglas, M. (2002). *Purity and danger. An analysis of the concept of pollution and taboo*. Routledge.
- Hands, D. W. (2001). *Reflections without rules. Economic methodology and contemporary science theory*. Cambridge University Press.
- Hausman, D. M. (1992). *The inexact and separate science of economics*. Cambridge University Press.
- Lakatos, I. (1976). In J. Worrall, & E. Zahar (Eds.), *Proofs and refutations. The logic of mathematical discovery*. Cambridge University Press.
- Morgan, M. S. (2012). *The world in the model. How economists work and think*. Cambridge University Press.
- Popper, K. (1944). The poverty of historicism, I. *Economica. New Series*, 11(42), 86–103. <https://doi.org/10.2307/2549642>
- Popper, K. (2002). *The open society and its enemies*. Routledge.
- Woody, A. I. (2014). Chemistry’s periodic law. Rethinking representation and explanation after the turn to practice. In L. Soler, S. Zwart, M. Lynch, & V. Israel-Jost (Eds.), *Science after the practice turn in the philosophy, history, and social studies of science* (pp. 123–150). Routledge.