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What makes economics special: orientational paradigms

Paul Hoyningen-Huene[a,b] and Harold Kincaid[c]

[a]Institute of Philosophy, Leibniz Universität Hannover, Hanover, Germany; [b]Department of Economics, Universität Zürich, Zürich, Switzerland; [c]School of Economics, University of Cape Town, Cape Town, South Africa

ABSTRACT
From the mid-1960s until the late 1980s, the well-known general philosophies of science of the time were applied to economics. The result was disappointing: none seemed to fit. This paper argues that this is due to a special feature of economics: it possesses 'orientational paradigms' in high number. Orientational paradigms are similar to Kuhn's paradigms in that they are shared across scientific communities, but dissimilar to Kuhn's paradigms in that they are not generally accepted as valid guidelines for further research. As will be shown by several examples, orientational paradigms provide economics with common points of reference that support its epistemic coherence and make scientific discourse more easily possible across school boundaries. With the help of systematicity theory, a newer general philosophy of science, one can further elucidate the role of orientational paradigms with regard to scientific progress.

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1. Introduction

Especially from the mid-1960s until the late 1980s, both philosophers and economists tried to build bridges between general philosophy of science and economics. Typically, the question was whether the well-known general philosophies of science like those of Popper, Kuhn, or Lakatos captured essential features of economics.[1] However, the results have been disappointing. None of these standard philosophies of science seemed to be appropriate for economics. Important parts of economics do not resemble Popper's falsification pattern in which economic models and theories should be falsifiable and that economists constantly try to falsify them.[2] In a strict Popperian perspective, economics rather looks like a pseudoscience because very often economic models are, due to their abstractness, not at all falsifiable (see, e.g. Sugden, 2000, p. 6). If economics 'advances by expanding its library of models and by improving the mapping between these models and the real world' (Rodrik, 2015, p. 5), then the Popperian ideas cannot apply. 'Mapping' seems a more apt metaphor for how economics works and one that is not reducible to falsificationism.

Thomas Kuhn's famous theory seemed to offer a broader spectrum of scientific behavior, not just constant critical testing, and therefore had a better chance to cover economics.[3] However, it turned out that the identification of the core elements of Kuhn's theory, paradigms with the associated normal science and revolutions, was exceedingly difficult. One reason was that Kuhn's terminology appeared to be too vague to be applied unambiguously to economics. Furthermore, 'orthodoxy' in economics has always faced 'heterodoxy' in a much stronger way than in the natural sciences (but much less than the other social sciences or the humanities), so there is no normal science in Kuhn's sense of a consensus paradigm. With respect to revolutions, economists widely disagreed. Some

CONTACT Paul Hoyningen-Huene hoyningen@ww.uni-hannover.de Institue of Philosophy, Leibniz Universität Hannover, Hanover, Germany; Department of Economics, Universität Zürich, Schönberggasse 1, CH-8001 Zürich, Switzerland

thought that there have never been any revolutions in economics, whereas others denied that any 'revolutions' in economics were of the Kuhnian type. Still others were open to the possibility of the existence of Kuhnian revolutions in economics, but could not agree among each other whether, for instance, the transition from classical to neoclassical economics or the Keynesian revolution was one of them. The central reason for the failure of Kuhn's theory applied to economics is that his theory was designed for the basic disciplines in the natural sciences, and he thought 'it remains an open question what parts of social science have yet acquired such paradigms at all' (Kuhn, 1962/1970, p. 15). For instance, economics is much less isolated from societal influences than the basic natural sciences, especially in their normal science phase, and therefore has a different developmental pattern. Everyday economics is often tied directly to ongoing social issues in a way that fundamental physics is not.

Finally, Lakatos' theory of research programs looked most promising from the set of the different philosophical alternatives, because, among other things, it softened the falsification motive in comparison to Popper.[4] The 'hard core' of a research program in Lakatos' sense is immune against potentially falsifying evidence, reminding economists of practices in economics. However, upon closer inspection, it turned out that the identification of a hard cores in economic research programs was not unequivocally possible, both for large scale programs like neoclassical economics or smaller programs like capital theory. Again, like in Kuhn, some vagueness in the central concepts of Lakatos' theory was seen as one of the most important reasons. Furthermore, it was doubtful whether these research programs were chosen for the reasons that Lakatos adduced, namely, superior empirical strength, especially the constant discovery of novel facts.

Thus, it turned out that economics was not descriptively adequately covered by any of these philosophical positions, and normatively, none of the positions when taken at face value promised to be productive for economics. Of course, these philosophies were developed with the natural sciences in view, especially physics, but even whether they captured essential features of them was controversially debated. However, the broader discussion about their possible fit with economics faded away in the late 1980s, possibly with an implicit consensus that general philosophy of science was not very useful to economics, neither descriptively nor normatively. Economics as a social science is perhaps too different from the natural sciences, especially from physics,[5] despite declarations by some economists to the contrary,[6] such that the (imperfect) general philosophies of natural science could not usefully be applied to economics. Instead, the philosophy of economics turned to more fine-grained questions regarding specific parts of economics.[7] This move reflected the general trend in the philosophy of science that saw discipline-specific issues and studies predominate over general philosophy of science.

However, in this paper, we shall make a new attempt to analyze (positive) economics with categories from general philosophy of science.[8] The aim is to capture and understand the status of economics as a very special discipline. We will try to avoid what we see as the main weakness of the earlier attempts to understand economics in terms of general philosophy of science: that the respective philosophies of science were mainly adapted to natural science and in fact not even to their diversity, but only to physics.[9] We will introduce a new concept into general philosophy of science, 'orientational paradigms' (OPs), that is neutral with respect to different discipline groups; OPs can be found in the natural sciences as well as in the social sciences and the humanities. We will then embed OPs in a truly general philosophy of science, systematicity theory, that covers all scientific disciplines in the wide sense of all research disciplines,[10] thus avoiding the natural science bias. The thesis will be that in economics the number of orientational paradigms is so high, their overlap extensive, and their fluent usage is so commonplace that OPs capture 'the soul of economics.'[11]

We shall proceed as follows. In Section 2, we will introduce the notion of orientational paradigms (OPs). This will be done by developing their similarities with and dissimilarities to Kuhn's paradigms. Roughly speaking, OPs are an attenuation of Kuhn's paradigms. Then we will show by several examples that economics indeed features OPs (Section 3). In Section 4 we will analyze the epistemic role of OPs from the vantage point of a fairly new general philosophy of science, systematicity theory, which supersedes the older theories by Popper, Kuhn, Lakatos, Feyerabend, etc. Systematicity theory claims to be a truly general philosophy of science, covering not only the natural sciences, but also all the other research disciplines, including the social sciences. We will then see how OPs play a more limited role in comparison to Kuhn's paradigms. This will make evident why it is epistemically advantageous for disciplines that lack Kuhn's paradigms to have at least OPs. Economics will be a case in point because it features an unusually high number of OPs.

2. Orientational paradigms

Famously, in 1962 Thomas Kuhn introduced the notion of paradigms in his *Structure of Scientific Revolutions* (Kuhn, 1962/1970). Although there was a somewhat chaotic later development of this concept both in Kuhn and in its reception, there is a reasonably clear characterization of paradigms early in the book. Kuhn takes them 'to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.'[12] Note that this definition allows for paradigms of very different scope, depending on the size of the scientific communities for whom they are exemplary. Note further that the elements that Kuhn later and transitorily subsumed under the title of a disciplinary matrix are implicitly present in the definition, because problem solutions imply the use of theories, ontological commitments, models, and epistemic values. It turns out that such paradigms can only be found in certain phases of the natural sciences. The social sciences, by contrast, remain according to Kuhn in a 'pre-paradigm' state which expresses the fact that they lack these *universally* recognized scientific achievements. The social sciences may feature scientific schools each with a *local* consensus, but they lack an overarching common viewpoint crystallizing in paradigms.[13] This difference is fundamental because the presence or absence of paradigms crucially influences the development of a discipline. An overarching consensus based on paradigms leads to 'normal science' in which scientific progress exists and is easily identifiable. By contrast, scientific communities fractured into different schools cannot agree on what real progress is in their discipline because each school questions the foundations of any other school. By 'school' we mean something like a pre-paradigm or local research tradition. Because our image of science is deeply influenced by the natural sciences and their progress since the seventeenth century, fractured disciplines without tangible progress appear as 'something less than science' (Kuhn, 1962/1970, p. 13).

However, Kuhn had previously noted that economics is somehow different from the other social sciences by displaying somewhat less disagreement: 'It may […] be significant that economists argue less about whether their field is a science than do practitioners of some other fields of social science'.[14] Clearly, Kuhn did not want to imply that economics features paradigms that create consensus in the same way as they do in the natural sciences. Thus, it seems that from the beginning Kuhn saw that economics has, as far as consensus in scientific communities is concerned, somewhat less to offer than the natural sciences but possible somewhat more than the other social sciences. We shall argue in the next section that this special status of economics is due in large part because it has a large variety of OPs. We explain the idea of OPs and contrast them with Kuhn's paradigms. Because OPs are a thinner version of Kuhn's paradigms, we will introduce them by relating them to paradigms in Kuhn's sense, displaying both parallels and contrasts. The reader familiar with expected utility theory is invited to use it as an illustration, and we will explicitly do so in the next section.

1. (like Kuhnian paradigms) OPs are shared across the pertinent scientific communities.

This means that all members of the pertinent community *know* the respective OPs. Some OPs are so wide-ranging that they are known to all members of the whole profession. For example, expected utility theory (EUT) is known to every economist.

2. (unlike Kuhnian paradigms) OPs are common points of orientation rather than fixed points of belief.

Thus, the kind of 'sharing' of OPs is different from the 'sharing' of Kuhn's paradigms. In the respective community, OPs are not *generally* accepted as binding guidelines for further research as Kuhn's paradigms are. Some members may work under their guidelines, whereas others may explicitly distance themselves from them. Therefore, the specific stance of members of the profession towards OPs may define schools or directions within the profession. However, as both factions are able to refer to the OPs, they may facilitate communication across school borders.

3. (unlike Kuhnian paradigms) OPs are not further articulated, developed, and applied to new problems by the *whole* community.

OPs are thus not a source of normal science in Kuhn's sense. However, they have a similar function for the specific schools that accept the OPs as guidelines for research. These parts of the community indeed further articulate, develop, and apply OPs to new cases; they are sources of local progress.

4. (unlike Kuhnian paradigms) OPs are openly and possibly severely criticized by parts of the pertinent scientific community, especially by those who develop alternatives to them.

During normal science, Kuhn's paradigms are not and cannot be the subject of public and publishable criticism in the relevant scientific community. This is what Kuhn once called the 'quasi-dogmatic commitment' of normal science (Kuhn, 1963, p. 349). By contrast, criticizing OPs is a legitimate part of disciplines featuring them, as well as their defense.

5. (like Kuhnian paradigms) The breath of applications of OPs varies.

Some OPs are very general theories, including exemplary applications to concrete problems, that are known to every member of the profession. Such OPs are usually taught already in undergraduate classes in all economics departments across the world. Other OPs are less general, even going down to rather specific models that are only better known in smaller communities of specialists. There, however, they are known to everybody, be it that they are agreed to or criticized.

6. (like Kuhnian paradigms) OPs are historical entities: they come, flourish, and may go.

Typically, OPs develop a remarkable stickiness. They are not necessarily overturned by the development of an alternative theory or model. Because of their orientational function, they don't necessarily lose when a competitor is developed. However, there are cases in which a theory with its application loses its status as an OP and disappears from the scientific scene. The labor theory of value may be a case in point; after its brilliant career in the 18th and nineteenth century, it only survived in the corner of Marxist economics.[15]

We claim that OPs are present in many disciplines making the concept of OP a concept belonging to the *general* philosophy of science. The 'classics' of sociology like Marx, Weber, and Durkheim also play the role of OPs. Also physics features OPs, for instance Bohr's atomic theory that was once a Kuhnian paradigm but lost this status with the advent of modern quantum theory. Nonetheless it still serves multiple purposes. However, all of these OPs may not be developed further by any group of researchers. As a further example, ecology also has multiple OPs – basic models which are used as a reference point in various research areas. A classic example is the Lotka-Volterra equations originally designed to model predator-prey relations (Gotelli, 2001). The idea is that there are two species with their own intrinsic growth rates and carrying capacities that compete with each other. The two Lotka-Volterra equations provide a basic OP for ecology. Everybody knows these equations; some empirical work has resulted from them, and they form a starting point for work in many areas.[16] We claim that OPs like this provide otherwise fractured scientific communities common points of reference which is advantageous for them.[17] We will discuss this claim in more detail in Section 4.

We finish this discussion of OPs with an obvious objection, viz., that the basic concept is too vague and not operationalizable. This, of course, was a major objection to Kuhn's work. While we are for precision when it can be gotten, we note that concepts without necessary and sufficient conditions are common across the sciences and thus it would hardly be surprising if that is so in the history, sociology, and philosophy of science. There is still no explicit definition of 'gene' in molecular biology (Moss, 2003; Waters, 2017); the notion of a scientific law is still contentious. Still, we get on with these concepts because they are useful. One of us has argued for this approach in depth for Kuhn's idea of paradigms (Hoyningen-Huene, 1993, Chapter 4). *We think that OPs should be pursued in the same spirit.*

Economics features OPs, too. In fact, we claim that economics is characterized by such a large number of OPs that its character is importantly shaped by this fact. We turn to this claim in the next section.

3. Orientational paradigms in economics

We are proposing that the supposed special status of economics rests on its having a particularly high number of OPs. This thesis is a specification of the well-known view that economics is theoretically dominated by a variety of mutually partly contradicting models.[18] Our thesis is that many of these models are in fact OPs. We shall illustrate this claim with multiple examples.

A first and fairly fundamental OP in economics comes in the form of production and exchange models, going all the way back to Classical economics. There are multiple, overlapping versions of these OPs. They are invoked across economics to varying extents depending on subdisciplines and historical periods. Thus, they are historical entities. Sometimes they lead to schools or research traditions, but they are far from Kuhnian paradigms that embody methods, theory, epistemic values, etc. in sufficient detail to guide a consensual normal science. Instead, they are models generally known across the discipline at any given point in time after they are introduced, pedagogically essential, and generally starting points for more complex research discussions, and sometime with further assumptions and modeling do turn into concrete research traditions or what can be called local paradigms. They are local because they are not universally applied but they are paradigms in the sense that they lead to more elaborate sets of methods, theory, etc. than do the initial OPs. We illustrate four such variants of the production-exchange OP: the single individual constrained maximization story without prices or markets, that story expanded to two individuals but without production and still without prices or markets (the Edgeworth box paradigm), the consumer demand models with prices, and the general competitive equilibrium model with prices, exchange, multiple individuals, and demand and supply functions.

The constrained optimization story is exemplified in the Robinson Crusoe narrative that goes back to the early classical economists and continues until the present as the basic micro model of individual choice. The Crusoe narrative is an interesting one in the history of economics (see Karagöz, 2014). We cannot go into that history in detail here. However, Marx provides a quick and analytically intuitive analysis of its first stages.

Here is his description in *Capital* Volume 1 (Marx, 1867, p. 56):

> Since Robinson Crusoe's experiences are a favorite theme with political economists, let us take a look at him on his island […] Necessity compels him to apportion his time accurately between different kinds of work. Whether one kind occupies a greater space in his general activity than another, depends on the difficulties, greater or less as the case may be, to be overcome […].

The point Marx is making is twofold. First, individuals making choices given scarce or constrained resources – time and abilities – must make tradeoffs given their preferences and their abilities and do so in the most efficient way. Second, this is a problem we can conceive of in isolation from their relations to others, hence Crusoe on his island alone. Adding another individual to the story calls for more complications, but this constrained preference pursual process is fundamental. Of course, now this story is nearly trivial, though it is still quite possible to find work in the social sciences that ignores it. In economics it is a basic starting point and most economics has to respect it, even though it is far from a Kuhnian paradigm. What are the costs and benefits and what is their tradeoffs is a question that orientates most economic research.

This basic OP then becomes parts of other OPs that are equally common in economics. The obvious next development is to go from an isolated individual to two individuals with different preferences and abilities who trade – so Friday meets Crusoe. This interaction can again be done without markets and prices but instead simply in terms of barter. The simplest version does not even require abilities beyond initial endowments. The Edgeworth box (Figure 1) is the classical example and comes amidst the transition from classical to marginalist theory (Edgeworth, 1881; Pareto et al., 1906/1972). Individuals have preferences described by indifference curves depicting what ratio of consumption of goods are equivalent and an initial endowment. Then the curves of two individuals are plotted with the tangencies representing points where neither can be better off. This then is one of the early formulations of an equilibrium concept. All of this story is now quite well-known, but that is the point: this simple two-person, two-good situation where individuals are getting their best deal as it were is a standard OP in economics. Nobody thinks it tells you in any detail how to do research in complex situations, but it is a model everybody recognizes as a starting point. It is still the starting point in mainstream upper-level microeconomics texts (see, for example, Osborne & Rubinstein, 2020, pp. 137–138).

How this analysis gets realized is various and diverse. Osborne and Rubinstein (2020), Chpt. 8 develop the paradigm initially without prices at all but with individuals, preferences, resources and power. Quite interesting results can be gotten in this way without assuming markets with money prices.

A further OP builds on those just discussed to explicitly bring in market prices. The classic work horse is supply and demand models. Take the consumer demand side. The constrained maximization OP is further developed into what might be called a 'supply and demand (S & D) argument pattern'

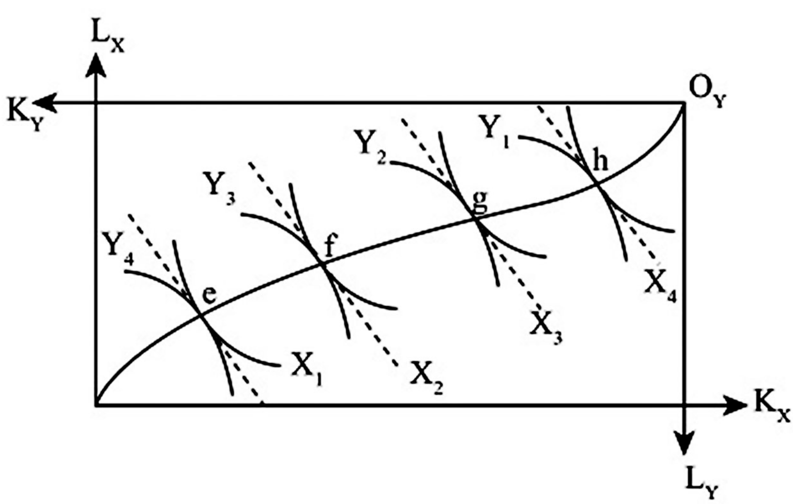

Figure 1. Edgeworth box with indifference curves.

(Kincaid, 1996). The idea of an argument pattern comes in part from Philip Kitcher who develops the idea largely in the context of evolutionary biology (Kitcher, 1981). However, the idea is general and closely approximates the idea of an OP. It involves a basic framework that can be applied across areas but must be filled in or fleshed out according to circumstances. It is not a full-blown theory, but once again a pedagogical and research starting point. It is a strategy a scientific community finds useful to advance its scientific interests.

To explain, the S & D argument pattern requires several things:

1. Identifying the relevant commodity, the market – buyers and sellers, and measure of price (sometimes prices have to be inferred – so-called shadow prices)
2. Explain the market by identifying supply and demand curves and by identifying changes along the curves and shifts in the curves
3. Explain the basic elements determining shifts in supply curves by examining the costs of substitutes and complements, income and so on.

This is a standard OP in economics with a long history that has been developed and expanded in multiple ways (Kincaid, 1996). It is a baseline model that all economists know and is taught from upper-level microeconomics and after. There are many economic situations where it is too simple or not easily supplied, but the S & D OP is a reference point or beginning for many economic discussions.

Yet another step in this sequence of related OPs is general equilibrium theory (GET). The previous OPs abstracted away from complex situations – assuming no production for example. GET is more general, though it obviously involves many abstractions as well. GET runs from very idealized models that show how multiple goods and markets interrelate to complex empirically computable models that are used to advise policy. However, the basic OP is simply that the effects of multiple interacting markets have to be considered. S & D models approach this paradigm when involving the prices of complements and substitutes, though they can make perfect sense as partial equilibrium models. However, GET does this more formally, and is again a baseline model that almost every economist knows. It is certainly not a guiding tool of research everywhere, but the basic OP of asking about general equilibrium effects is a core part of the discipline.[19]

A second obvious OP in economics is utility theory. There is a long and complex history here, and there are certainly many different instantiations. One key step was Samuelson's writings on revealed preference theory Samuelson (1938, 1948). Samuelson in these papers did not work with expected utility but he did show how consumption theory could be represented with a utility function based on observed choices. Another key element in that history starts most explicitly in the 1950s with the spread of Von Neumann and Morgenstern (1944) and Savage (1954/1972) on expected and subjective expected utility theory. Our point is that since the 1950s there has been an OP known to most economists that is generally a fixed point of reference – some version of expected utility theory (EUT) as an account of economic choices. Importantly, EUT is still a core element of neoclassical economics, many economists base their research work on it, and it is still a constitutive element in any economics education. EUT's role as an OP is especially obvious in the work of the pioneers of behavioral economics Kahneman, Tversky, and Thaler. Daniel Kahneman and Amos Tversky's work on cumulative prospect theory (CPT) begins by listing twelve cases of deviations from EUT in its descriptive use, systematically expanding Allais' work of 1953 (Kahneman & Tversky, 1979). They call these deviations 'problems,' indicating by this denomination the reference to EUT, because their observations are not consistent with EUT. They then develop prospect theory as an alternative to EUT, again referring to it by trying to make prospect theory formally as similar as possible to EUT and in fact CPT encompasses EUT in that EUT is an instance of CPT when various parameters are set to zero (Wakker, 2010). Similarly, appeal to EUT occurs in the work of Richard Thaler. For four years, he had a regular feature in the newly founded *Journal of Economic Perspectives*. This feature was entitled 'Anomalies', and it presented 'facts that were inconsistent with the standard way of doing economic theory' (Thaler, 2015,

p. 170). Clearly, EUT as being part of standard economic theory functioned as an OP here, EUT then forms the baseline to which other complications are added:[20] probability weighting in rank dependent utility theory, developed by Quiggin (1982), is even more general and then CPT further adds reference points and loss aversion (Wakker, 2010). Thus, there are a series of related OPs here that no one (well, almost no one) takes to be the right answer, but each is a baseline or framework that organizes discussion and research. A useful extension of this complexity is work arguing that these different OPs are not necessarily in a horse race: they can be enlightening for different subpopulations as tested by mixture models (Harrison & Rutstrom, 2009).

Thus, thinking in terms of OPs here can help clarify the relation between neoclassical expected utility theory and parts of behavioral economics. As noted above and explicitly by behavioral economics practitioners, EUT is a common reference point against which behavioral economics often proceeds. A related question arises: what is the relation between behavioral economics and experimental economics? How can that relation be illuminated from the OP perspective? We discuss these issues next.

What then is the status of behavioral economics from the OP perspective? This is not a question easily answered because in large part 'behavioral economics' is far from a homogeneous development. There are at least two strands in behavioral economics – the 'old' tradition (Sent, 2004) perhaps best represented by Herbert Simon in the 1950s and 1960s and then the current explosion of work that also calls itself behavioral economics as well, represented for example by the work of Kahneman and Tversky, Thaler, Camerer, and many others. The old behavioral economics largely gave up on constrained optimization and to that extent rejected EUT as an OP. Significant parts of the new behavioral economics retain the constrained optimization reference point. However, much that gets called behavioral economics does not retain that OP.

The fact is that much so-called behavioral economics is a diverse set of 'theoretical' approaches. Theoretical is in parentheses because much behavioral economics is relatively atheoretical (see Spiegler, 2019 for a detailed analysis). The typical survey (e.g. Dhami, 2016; Just, 2014) covers topics such as biases and heuristics in reasoning, social preferences, time discounting and prospect theory (though often not noting the two different versions given by its originators, original prospect theory and cumulative prospect theory). The elements of cumulative prospect theory such as loss aversion, reference points, and probability weighting are often pursued as independent topics. Even prospect theory is relatively atheoretical in that there is no account of what determines reference points, and key parameters such as weighting of losses and gains are simply plugged in, even though the assertion that losses are treated differently than gains is supposed to be a major innovation.

Thus, it seems that current behavioral economics is sometimes guided by the EUT reference point, but finding other OPs in behavioral economics is not obvious because of the amalgam of practices involved. Of course, this diagnosis is not extensively supported here, but is hopefully a stimulus for more detailed investigations.

A third illustration of an OP in economics comes from the explosion of experimental work over the last 30 years. In this case the OP we are pointing to begins with lab-based experiments on economic behavior guided by key assumptions from economic theory. Again, this is an OP that does not uniformly dictate research practices, but it is an exemplar or ideal case that those doing experimental economics will recognize and will use as reference point even if they do employ it in its entirety.

Lab-based experimental economics has developed a set of criteria for running a compelling experiment. Three key components are (see Hofmeyr & Kincaid, 2023; Smith, 1976, 1982):

- nonsatiation
- salience
- dominance

Nonsatiation asserts that experimental subjects always prefer more of a reward to less. Salience means that subjects know how the experimental setup will deliver rewards (and sometimes losses) for their choices and that the rewards are sufficient to motivate them to make choices seriously. Not all rewards are salient: a fixed show-up fee is not salient because it is not linked to the choices made in the experiment. Dominance requires that subject payouts be sufficiently high to overcome the cost of paying attention and so to induce subjects to make conscious choices rather than, for example, just choosing randomly.

These criteria describe an OP that is both more specific and explicit than the phrase ‘lab-based’ and allows for applications that are broader than the lab. The key issue is experimental control, but that is a very general notion and the experimental economics OP we have in mind here ties that idea down in ways motivated by economic theory. It is useful to distinguish this experimental economic OP from experiments more in the psychological tradition (see Hofmeyr & Kincaid, 2023). The psychological tradition of lab experiments is almost entirely without incentives to motivate choices (being required to be in an experiment to pass a course is not such an incentive). The classic Kahneman and Tversky experiments had no incentives, for example. Salience and dominance are not generally addressed, though psychologist can worry about such things in some form.

It is important to note that the lab-based OP has further logical extensions. Being lab based is motivated by the idea of having control over key variables. Thus, the lab-based OP has natural extensions outside the lab. Harrison and List (2004) detail these. Among these are, in addition to lab experiments, (1) artefactual field experiments which are lab experiments conducted in the field rather than with college students, (2) framed field experiments which are artefactual field experiments which refer to various variables instantiated in the field, (3) randomized experiments in the field, and natural experiments where randomization of factors occurs without being planned for experimental purposes. The lab-based OP is the fundamental OP and these other experimental practices use it as a reference point even if they involve significant differences or extensions in practice.

So, what then is the relation between experimental economics and behavioral economics from the OP perspective? A significant fraction of behavioral economics is not experimental but observational. In these cases, the experimental economics OP is not in use. Hofmeyr and Kincaid (2019) detail the large literature allegedly supporting CPT that is entirely observational. While CPT is arguably still the most investigated/advocated component of behavioral economics (see the comment in Kpegli, 2023, p. 1 that ‘prospect theory is today the main descriptive model for decision making under risk and uncertainty’), CPT is not unique in this regard. Baker and Nofsinger’s high-level survey of behavioral finance has at most a few pages devoted to experimental evidence out of 700 pages (Baker & Nofsinger, 2010). The classic behavioral economic investigation of taxi driver labor supply (Camerer et al., 1997) is purely observational. We are not complaining about observational evidence but just pointing out that some or much behavioral economics does not have the experimental economics OP.

Furthermore, a significant part of the experimental work that is done in behavioral economics falls into the psychological tradition in that incentives (and thus salience) are lacking. Harrison and Swarthout (forthcoming) survey experiments done on CPT in the last 20 years and find that about one third of them had no salient incentives.[21] Thus, behavioral economics is distinguished from the lab-based experimental economics OP in many ways: much behavioral economics is not supported by experiments and when it is, the experiments are often of a different kind than those advocated by the lab-based economics tradition.

Due to limited space, we will only name and not analyze further OPs in economics.[22] They are all either highly cited or such a common possession of economics that their authors are not even mentioned anymore. These OPs are not universally accepted as correct because they have well-known weaknesses. They vary in scope, which means that they function as OPs either in the whole of economics or in smaller sub-communities (in macroeconomics, for example). Very general OPs in economics come from game theory (see, e.g. Binmore, 2007), which contains the sub-OPs prisoner’s dilemma and Nash equilibrium (Nash, 1950) analysis in general. Somewhat more specialized OPs are Akerlof’s model of asymmetric information (Akerlof, 1970), the Modigliani Miller theorem in corporate finance (Modigliani & Miller, 1958), the rational expectation hypothesis (Muth, 1961), the

CAPM in asset pricing (Sharpe, 1964), the Cobb–Douglas production function (Biddle, 2021), the efficient market hypothesis (Fama, 1970), and basic DSGE models in macroeconomics (Kydland & Prescott, 1982).

Given the existence and importance of OPs in economics, we may ask why it is advantageous for a scientific community to have OPs. This question can be answered when put in the context of a newer general philosophy of science, systematicity theory.

4. OPs and systematicity theory

Why is it advantageous to have OPs? This question concerns very general features of the group of sciences that feature OPs and should therefore be answerable in the context of a general philosophy of science. Unfortunately, the prominent twentieth century general philosophies of science cannot smoothly accommodate OPs, as we could indirectly see in their failure to come to terms with economics. However, in the context of 'systematicity theory,' a new general philosophy of science, the role of OPs can be investigated.[23] Systematicity theory (ST) is a truly *general* philosophy of science in that it covers *all* research fields, not only the natural sciences. It also covers mathematics, the engineering sciences, the social sciences, and the humanities. For simplicity, we shall address all such disciplines as 'sciences.' We can sketch ST here only very briefly.

The main question of ST is 'what is science?', and its general answer is: 'Scientific knowledge differs from other kinds of knowledge, in particular from everyday knowledge, primarily by being more systematic' (Hoyningen-Huene, 2013, p. 14). It is important to see at the outset that ST changes the focus of the question 'what is science?'. Under the influence of Karl Popper, for decades the question 'what is science?' was meant to demarcate (legitimate) science from cognitive endeavors thought to be illegitimate, like pseudoscience or metaphysics. In ST, this demarcation question recedes into the background. Instead, in ST the question is what distinguishes (legitimate) everyday knowledge or professional knowledge from specifically scientific knowledge? The wholesale answer is that scientific knowledge is more systematic.

This very abstract answer must now be developed in two respects. First, the claimed higher systematicity of science manifests itself in nine dimensions (or 'aspects') of science, claiming a higher degree of systematicity in each of these dimensions, e.g. for descriptions, explanations, and the defense of knowledge claims, in comparison to other forms of knowledge. For instance, scientific descriptions are more systematic than everyday descriptions, scientific explanations are more systematic than everyday explanations, and so on. Second, it turns out the meaning of the central term of ST, systematicity, is not constant both across the nine dimensions and across different (sub-)disciplines of science; it is slightly differently concretized in different contexts. So, an increase of the systematicity of descriptions in art history is not the same as in organic chemistry, or an increase of the systematicity of descriptions in botany does not mean the same as a systematicity increase in botany's explanations. The different concretizations of the systematicity concept are connected by a net of family resemblances (Hoyningen-Huene, 2013, pp. 25–30).

ST is primarily meant to be a descriptive theory, and it must therefore be argued empirically, that is by showing that indeed scientific knowledge is more systematic in the nine dimensions than their non-scientific counterparts (see Hoyningen-Huene, 2013, pp. 35–147). However, ST also has normative content (without committing the naturalistic fallacy) because each dimension implicitly carries normative content. For instance, a more systematic defense of knowledge claim is obviously better than a less systematic one. Therefore, a systematicity increase in any of the dimensions of some science improves that science.

The mainly relevant dimensions of systematicity for the appreciation of the role of OPs are 'epistemic connectedness' and 'critical discourse' (Hoyningen-Huene, 2013, pp. 108–124). 'Epistemic connectedness' refers to the fact that different parts of a science cohere in various ways with one another. The claim of systematicity theory is that in science, the degree of epistemic connectedness is higher than in other knowledge generating enterprises, especially in comparison to our common-sense epistemic activities. However, this is also relevant for inter-science comparisons. First, it is obvious that the use of OPs increases the degree of epistemic connectedness of some science. Scientists referring to OPs when explicating alternatives to them or working under their reign by developing new applications of them, generate or utilize epistemic connections among various parts of science. Second, if in a science no such stock of OPs exists that is generally known and used, the degree of epistemic connectedness is clearly lower (unless it had a functional equivalent of OPs, but we do not know of any). Thus, in the light of systematicity theory it is obvious that it is advantageous for a science to have and use a set of OPs.

Another dimension of systematicity theory, 'critical discourse', translates some aspects of epistemic connectedness into the social domain. Science features various social institutions designed to foster critical discourse, more than many other knowledge generating enterprises, especially common sense. Clearly, scientific discourse profits from the possibility that scientific adversaries are able to refer to common ground, namely OPs, even if they assess them differently. They can articulate their differences with reference to the OPs, that is to a sort of coordinate system in which they can locate their opposing positions. Clearly, such points of orientation ease scientific discourse by facilitating mutual understanding and helping to avoid talking past each other. Thus, systematicity theory is capable of explaining why for scientific progress, having OPs is advantageous in comparison to not having them.

5. Conclusion

Economics features OPs, and this paper tried to establish this fact and explain its consequences. OPs build bridges within disciplines whose inner structure is more or less permanently jagged by controversies. These disciplines are in danger of losing their internal coherence, and some social sciences indeed give this impression. Economics is torn by controversies, but OPs mostly in the form of models and theories support epistemic coherence and social cohesion by making critical discourse beyond school borders possible. In the framework of systematicity theory it can be understood how and why OPs contribute to scientific progress in comparison to those disciplines that lack them.

Notes

1. For overviews see, for example, Blaug (1980), Canterbury and Burkhardt (1983), Hausman (1992); and especially (Pheby, 1988/1991) with references to much of the relevant literature. – We treat only Popper, Kuhn, and Lakatos here because they were, in comparison to other general philosophers of science, the most discussed ones in economics.
2. Popper (1934/1959) for the discussion of various aspects of Popper's theory in economics (see Backhouse, 2012, pp. 25–30; Blaug, 1975, pp. 399–402; Hausman, 2018, pp. 13–14; Pheby, 1988/1991, pp. 32–36).
3. Kuhn (1962/1970) for the discussion of various aspects of Kuhn's theory in economics (see Baumberger, 1977; Blaug, 1975, pp. 402–405, 409–412; Bronfenbrenner, 1971; Canterbury & Burkhardt, 1983; Coats, 1969; De Vroey, 1975; Drakopoulos & Karayiannis, 2005; Gordon, 1965; Johnson, 1983; Kunin & Weaver, 1971; Mehta, 1979; Pheby, 1988/1991, pp. 43–53; Stanfield, 1974; Stigler, 1969, pp. 223–225; Ward, 1972). – References to Kuhn after the period in question either used Kuhn's concept of anomalies to criticize the descriptive use of mainstream economics (Thaler, 1987, 2015, especially pp. 169–175, 307), or to articulate fundamental criticism of mainstream economics in Kuhnian terms (see, e.g. Fox, 2014; Walker, 2015; Layborn-Langton & Jacobs, 2018), or when indirectly referring to Kuhn by using the term paradigm in a fairly loose sense (see, e.g. Guala & Mittone, 2010 and Etzioni, 2011).
4. Lakatos (1970) for the discussion of various aspects of Lakatos' theory in economics (see Backhouse, 2012; Blaug, 1975; Drakopoulos & Karayiannis, 2005; Hausman, 2018, pp. 13–14; Latsis, 1976; Pheby, 1988/1991, pp. 61–67; Robbins, 1979; Zouboulakis, 2001).
5. See, e.g. Zouboulakis (2001).
6. See, e.g. Friedman (1953, p. 4).
7. 'Away from Big M'; see, e.g. Truc et al. (2021, p. 68) and Alexandrova et al. (2021).
8. Thus, we will return to the 'big picture', but not in the sense of Alexandrova et al. (2021) regarding an evaluation of the organization of economics.

9. As one of the referees noted, this statement is somewhat too general, because Popper 'attempted to expand and tailor falsificationism to the social sciences.' However, the Popper taken up in economics was mostly the Popper of the *Logic of Scientific Discovery*, in which Popper states that he sees theoretical physics as the most complete realization of empirical science (Popper, 1934/1959, p. 38).
10. Other authors also use 'science' so broadly, for instance (Quine, 1995, p. 49).
11. It may be that this use of OPs is what in large part distinguishes economics from other social sciences like sociology where OPs seem much rarer with little interconnection between them. Demonstrating this is beyond the scope of the paper but a thesis worth pursuing.
12. Kuhn (1962/1970, p. VIII). This very clear, early definition of the concept of paradigm in *Structure* serves our purposes well. The later development of the paradigm concept, both in *Structure* and in the ensuing discussion, is irrelevant in our context. For extensive discussion of the paradigm concept and its development, see, e.g. Hoyningen-Huene (1993), especially pp. 131–162.
13. Kuhn (1962/1970, p. VIII). Compare, e.g. Bruce (2018, p. xvi): 'there is sufficient consensus among ordinary physicists for introductory physics textbooks to state with authority the basic knowledge that is accepted by the trade and must be acquired by entrants. In contrast, introductory social science texts often describe their subjects as a series of competing perspectives.'
14. Kuhn (1962/1970, p. 161) – Although the inner diversity of economics has increased since the early 1960s mainly due to the addition of new subdisciplines, the self-understanding of economics as a science has apparently not changed. However, a serious defense of this statement would need a delicate empirical investigation.
15. Another example was suggested by an audience member (unknown to us) of a talk on OPs: according to his view, real business-cycle (RBC) theory was an OP between 1972 and 2003 but then ceased to be one, because in 2003 the New Keynesian DSGE model emerged.
16. The L-V equations have been taken up in areas outside ecology, suggesting that L-V OP in ecology is just a set of tools. We see the idea, but L-V equations build a model with substantive ecological assumptions. When the equations are transferred elsewhere new relevant substantive assumptions have to be made. Spelling out the role of the math in OPs would be an interesting topic for further research.
17. We note that OPs also fit well with recent work on general philosophy of science, in which it has been suggested that philosophy of economics might profit from a reinvented 'big-picture' strategy. The obsolete form of the big-picture strategy that prevailed up to the 1990s 'ascribed to economics a unified method and evaluated this method against a single criterion of "science"' (Alexandrova et al., 2021).
18. For a recent elaboration of this view, see, e.g. Rodrik (2015). He even claims: 'Models are […] what makes economics a science' (p. 5, also p. 83) – Note that in contemporary economics, as in many other disciplines, the use of the word 'model' is rather loose in the sense that mostly no sharp demarcation to 'theory', 'hypothesis', or 'law' is sought. For instance, the expressions 'expected utility theory', 'expected utility hypothesis', and 'expected utility model' are interchangeably used.
19. For a good walk through from simple models to applied applications, see Cardenete et al. (2017). A still useful reference is Weintraub (1985).
20. This dependence on EUT as fixed point has been widely noted subsequently. For a recent discussion, see Kanazawa (2021).
21. We cannot show that what is true of experimental work on CPT is true of the rest of experimental behavioral economics, but CPT has general been one of the more extensively and rigorous areas of research.
22. We would like to thank Nicolas Wüthrich for suggesting some of these examples of OPs.
23. The main source for systematicity theory is Hoyningen-Huene (2013); for critical discussion, see, e.g. Thalos (2015) and Lohse et al. (2019).

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Notes on contributors

Paul Hoyningen-Huene is professor emeritus of philosophy at Leibniz Universität Hannover and lecturer at the Dept. of Economics at the Universität Zürich. He received a PhD in theoretical physics in 1975 before switching to philosophy of science. His main research interests are the dynamics of scientific theory change, especially in Kuhn and Feyerabend; the nature of science; reduction and emergence; the ethics of science; metaethics; the philosophy of logics, of physics, of biology, of history, of psychology, and of economics; and the philosophy of soccer.

Harold Kincaid is Emeritus Professor of Economics at the University of Cape Town, South Africa. He works in the philosophy of the social and behavioral sciences, the philosophy of science more generally, and over the last decade on experimental economic elicitation of risk, time, and trust attitudes. He has authored or edited 16 books in these areas and many journal articles and book chapters.

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