volume II ♦ issue 2

2011

Logos & Episteme

an international journal
of epistemology

Romanian Academy
Iasi Branch
◆◆◆
"Gheorghe Zane" Institute
for Economic and Social
Research

INSTITUTUL EUROPEAN

Contact address:

Institutul de Cercetări
Economice şi Sociale „Gh.Zane”

Iaşi, str.T.Codrescu, nr.2, cod 700481
Tel/Fax: 004 0332 106507
Email: logosandepisteme@yahoo.com

www.logos-and-episteme.proiectsbc.ro

Logos & Episteme

ISSN-L: 2069-0533
ISSN: 2069-0533

This journal was edited within The Knowledge Based Society Project supported by the Sectoral Operational Programme Human Resources Development (SOP HRD), financed from the European Social Fund and by the Romanian Government under the contract number POSDRU ID 56815

TABLE OF CONTENTS

ARTICLES

EXPLANATION THROUGH SCIENTIFIC MODELS: REFRAMING THE EXPLANATION TOPIC*

Richard DAVID-RUS

ABSTRACT: Once a central topic of philosophy of science, scientific explanation attracted less attention in the last two decades. My aim in this paper is to argue for a new sort of approach towards scientific explanation. In a first step I propose a classification of different approaches through a set of dichotomic characteristics. Taken into account the tendencies in actual philosophy of science I see a local, dynamic and non-theory driven approach as a plausible one. Considering models as bearers of explanations will provide a proper frame for such an approach. In the second part I make some suggestions for a working agenda that will further articulate a sketchy account of explanation through models proposed by Hartmann and Frigg.

KEYWORDS: scientific explanation, scientific models, understanding

I. The explanation topic and its fate

It is hard to overlook the status of the topic of scientific explanation in philosophy of science. The topic was one of the central subjects for few decades during the second half of the last century and it concentrated the efforts of some of the best philosophers. Salmon's book *Four Decades of Scientific Explanation*[1] documents the dense debate from its inception to the end of the fourth decade, i.e., the end of the eighties. Salmon – himself a major contributor to the debate – expresses at the end of the book his view for the future fate of the topic. He advances a complementary view in which the two major approaches that dominated the

* ACKNOWLEDGEMENT: This paper was made within *The Knowledge Based Society Project* supported by the Sectorial Operational Program Human Resources Development (SOP HRD), Financed by the European Social Fund and by the Romanian Government under the contract POSDRU/89/1.5/S/56815.

[1] Wesley Salmon, *Four Decades of Scientific Explanation* (Minneapolis: University of Minnesota Press, 1989).

agenda at that time – the unificationist (as appears in Kitcher's account[2]) and the causalist conceptions of explanation (as expressed mainly in Salmon's work[3]) should complement each other. The pragmatic approach as advanced in van Fraassen's[4] account would play in Salmon's view the decisive role of choosing in a specific context between the two previous complementary conceptions.

A quite immediate question that rises looking retrospectively would ask how much of this view got fulfilled in the meantime? Is it the case that the subsequent approaches focused their main effort in implementing such a kind of strategy? The obvious answer is no.

On the other hand there is a sort of fatigue that gradually made its way in the research attitudes in philosophy of science finding its expression through a sort of rejection of the subject of scientific explanation. Though officially a major topic, it does no longer concentrate the attention of philosophers as in previous decades. Nonetheless at the same time, one can notice a tendency that seeks to resettle the issue in new contexts of inquiry. As the philosophical research reveals new areas of interest in the philosophy of science, the explanation topic gains new dimensions challenging the old requirements and assumptions of the topic.

I'll start my paper by first proposing a sort of ordering of the main approaches on explanation. This way I'll argue for a type of approach that I see to be promising in the context of the recent tendencies in philosophy of science. In a subsequent step I'll argue in favor of adopting a modeling view as a plausible framework to resettle some of the questions related to the explanation topic. I'll continue by presenting briefly some proposals that go some way in the same direction as the one proposed. A specific account by Roman Frigg and Stephan Hartmann will be seen as the most viable frame for the inquiry into explanatory models. I'll suggest some further steps that are to be taken in order to gain a more concrete articulation of this approach.

II. Getting some clues for a plausible approach to explanation

In this section I'm going to suggest four pairs of distinctions that will help me to characterize broadly the existing approaches on scientific explanation and will

[2] As presented for example in Philip Kitcher, "Explanatory Unification," *Philosophy of Science* 48 (1981): 507-531.

[3] As presented in his papers collected in the volume Wesley Salmon, *Causality and Explanation* (New York: Oxford University Press, 1998).

[4] His pragmatic approach is presented in the fifth chapter of his book *The Scientific Image* (New York: Oxford University Press, 1980): 97-157.

permit me further to identify a plausible direction that we could follow in our inquiry. I'm suggesting the following distinctions between: global versus local, dynamic versus static, theory-driven versus non-theory-driven and the approaches that have the tendency to view explanation as an application versus those approaching it as a construction.

The first distinction is that between a local and a global kind of approach. This distinction is based on the way in which two different kinds of considerations (global and local) are to be seen as determinant for the scientific explanation and therefore are to be used in a conception of explanation. A kind of global-type approach will be one in which the global considerations are viewed as central. This does not mean that only explanations that make direct appeal to the most general principles are proper explanations, but that the right criteria that determine an explanation are to be drawn properly from considerations at this level. Correspondingly, the same holds for a local view. Examples of such global approaches are Friedman's[5] or Kitcher's accounts in which the quality of explanatoriness emerges at a global level – those of corpuses of knowledge (as in Kitcher's account) or is given through reduction relations among laws as for Friedman. The accounts of Schurz[6] or Bartelborth[7] are more recent examples of this type of approach. Examples of local types of approaches are such as Salmon's account in which explanation is understood as disclosing the local causal network that brings about the explanandum or the one of van Fraassen in which contextual factors are ultimately determinant for any explanation.

The second distinction is one between static and dynamic approaches. The distinction is primarily between approaches that take into consideration temporal aspects of the scientific structures *versus* those that ignore them. Or in other words, a dynamic perspective would consider the ways elements of scientific knowledge are modified or new elements are constructed in the process of providing an explanation for a phenomenon. The existing accounts are in general of the first sort. Kitcher's account is one that addresses and integrates the temporal aspects of scientific knowledge. His conception considers different corpuses of knowledge from different historical periods as determining the explanations accepted as valid at that time. The account captures the dynamics at the macro level of scientific activity. Nevertheless, this sort of dynamics is only a specific one

[5] Michael Friedman, "Explanation and Scientific Understanding," *Journal of Philosophy* 71 (1974): 5-19.

[6] Gerhard Schurz, "Explanation as Unification," *Synthese* 120 (1999): 95-114.

[7] Thomas Bartelborth, "Explanatory Unification," *Synthese* 130 (2002): 91-107.

and not the relevant one for an adequate local solution on explanation. Kitcher provides us with criteria for comparing and selecting between corpuses of knowledge but he does not provide any clue as to how an explanation pattern is built. The solution would be to provide some insight into how different elements of the patterns evolve, i.e., are chosen, modified or dropped in the course of searching for an explanation of a phenomenon while the macro-constraints at the level of the corpuses would partially influence these processes. In this sense a proper dynamic account would unfold at a local level.

The third sort of distinction between theory-driven and non-theory-driven approaches reiterates the lines of a reaction[8] in today's philosophy of science. In the 'classical' philosophy of science theories played a central role not only as a focus of investigation into their structure but also as making any other topic more or less dependent of such a solution. In the last decades this theory-centered view was shaken due to various investigations into what were considered to be aspects of secondary importance. Such secondary topics were the experimental activity or the modeling one and their related products. Consequently, different topics, among them arguably explanation, gained (more or less) new valences in this new context.

The explanation topic rose at the status of a major subject in the philosophy of science in the heydays of the theory-oriented philosophy of science. Therefore it bears some of the legacy of that context. This could be seen also as one of the reasons why it is rejected in more recent philosophical agendas that assume a radical departure from any "received view"[9] influence. Nevertheless we could read out influences of the theory-centered but also signs from the opposite attitude in today's approaches on explanation.

The last distinction advanced – the one between explanation as an application versus explanation as a construction – is rather a regulative one, that should direct our search under a certain perspective. The distinction points to the relation between the events (or facts) to be explained and the units used to carry out the explanation in the way it assumes in the background the view of the

[8] The reaction of the recent modelists, and I borrow here the expression "modelists" from Carlos U. Moulines, who used it in his book *La Philosophie des Sciences. L'Invention d'une Discipline* (Paris: Edition rue d'Ulm, 2006) to name the approaches in philosophy of sciences that developed model-centered conceptions of scientific knowledge

[9] I use here Suppe's expression from Frederick Suppe, "The Search for Philosophical Understanding of Scientific Theories," in *The Structure of Scientific Theories,* ed. Frederick Suppe (Urbana: University of Illinois Press, 1977), 3-232, to refer mainly to the logical empiricist conception of science.

explanandum entities that are to be plugged into formal schemas or patterns given by the theoretical constructs. The last view might be described using Cartwright words as "the vending machine" view. Her critique[10] points to the fact that theories are conceived as one will "feed them an input in certain prescribed forms for the desired output" and after a while "it drops out the sought-for representation […] fully formed."[11] On the other hand, by using the notion of 'construction' I want to emphasize the different scientific activities involved in the process of explanation. Explanation as 'construction' takes seriously the idea that the representation of phenomena must be constructed and it is through such a process that we may get an explanation.

Now having laid out a sort of a grid on which we might compare different approaches I think that one sort of approach that we could consider not only as pertinent but also as promising for a future working agenda is a local, dynamic, non-theory driven kind and which will avoid conceiving explanation as an application. My choice is backed up by the conviction that such an approach will provide us with new valuable insights into how science works and will bring us closer to a more adequate consideration of scientific practice. To be more concrete I see some major benefits resulting from such an approach and that would justify the above choice. First of all, I think that such an approach would bring us closer to scientific practice through the fact that it emphasizes the local character and the dynamic aspects of explanation. Secondly, placing the issue in a dynamic frame makes this way room for the possibility of taking into consideration seriously the process of explanation (or conceiving explanation as a process rather than as a product). Besides this, from a larger perspective, it opens the possibility to embed scientific explanation into a more general frame, that of scientific inquiry. This move should enhance our insight into scientific practice and help us bridge the gap between the philosophical reconstruction and its object, i.e. scientific knowledge, gap that made older accounts appear artificially and alienated from real science. We'll avoid this way the philosopher's temptation to capture the 'grand' sense of scientific explanation that was questioned by some critiques of the philosophical accounts of explanation and understanding.[12]

---

[10] She directs her critique against the semantic conception of scientific theories.

[11] Nancy Cartwright, "Models and the Limits of Theory: Quantum Hamiltonians and the BCS Model of Superconductivity," in *Models as Mediators. Perspectives on Natural and Social Science,* ed. Mary Morgan et al. (Cambridge: Cambridge University Press, 1999), 244.

[12] Such a critique was voiced for example by J.D. Trout in "Scientific Explanation and the Sense of Understanding," *Philosophy of Science* 69 (2002): 212-233, against the philosophical approaches on scientific understanding as Salmon's or Kitcher's ones.

III. Models would fit the bill

In order to implement such a local, dynamic and non-theory-driven approach my suggestion is that by considering models as the bearers of explanations we would situate us in the right direction. Models could provide the adequate frame for a local approach on explanation, since they are usually built on local considerations. Such an approach will not be a theory-driven one if we consider the recent positions in the philosophy of science seeking to restore the importance and autonomy of models. Such authors as Nancy Cartwright, Margaret Morrison or Mary Morgan[13] have shown that an adequate view on models and modeling activity could not be gained through a theory-centered type of conception.

Models can also in a good sense be viewed in a dynamic perspective and enhance this way a dynamic approach on explanation. For a general perspective, a dynamic approach seems to be proper for constructs that were 'traditionally' claimed to do heuristic work.[14] As Weinert argued in his paper on *Models Theories and Constraints*,[15] models in comparison to theories exhibit fewer constraints. Therefore from a practical view a dynamical perspective on models should be more manageable than one on theories.

Last but not less important is the fact that a modeling approach could avoid the view on explanation as an application. Explanation as a construction process could be reflected either through the process of model generation or the one of model manipulation. The modeling view provides us therefore a concrete frame in which we could investigate the process of building an explanation.

Besides the above characterization, we might also bring some general additional clues to bear on the above choice. One such reason comes from the fact that the debate on explanation proved that pragmatics has to play an important role. Bas van Fraassen is the author that articulated in the eighties a pure pragmatic account on explanation. Nevertheless his position was criticized as being too unconstrained.[16] On one side, models as scientific units in comparison to other sorts of constructs incorporate a very important pragmatic component. It is

---

[13] Some important papers are gathered in the volume *Models as Mediators. Perspectives on Natural and Social Science,* ed. Mary S. Morgan and Margaret Morrison (Cambridge: Cambridge University Press, 1999).

[14] In the "received view" models were mainly seen as heuristic means, dispensable after the formulation of the laws in the new domain.

[15] Friedel Weinert, "Theories, Models and Constraints," *Studies in History and Philosophy of Science* 30 (1999): 303-333.

[16] Philip Kitcher and Wesley Salmon, "van Fraassen on Explanation," *Journal of Philosophy* 84 (1987): 315-330.

this characteristic that should direct our attention to models as explanatory providers. On the other side, they would provide the necessary constraints for a pragmatic approach on explanation, constraints that could to be found by investigating their structure, building and functioning.

Another quite general reason for looking after explanatory virtues of models comes from the recent developments in the modelist camp. One of the central ideas of the recent modelist approach was to emphasize the mediator role that models have.[17] Models are scientific constructs that mediate between theories, and phenomena. This fact qualifies them even better for explanatory jobs since explanation involves precisely this type of effort, namely, of seeking to get the theoretical constructs to bear upon phenomena.

***

Now taking a look into the debate on explanation we could notice that neither of the major accounts made reference to models as serious candidates for the role of explanation bearers. In *"*Aspects of Scientific Explanation"[18] Hempel dedicates an entire section to the discussion of explanation through models. He arrives at the conclusion that models cannot offer genuine explanations due to their intrinsic limitations as scientific units. His position actually unfolds in the frame of "received view" towards models, view that conceives models as being scientific constructs of secondary importance for scientific knowledge. One might find it curious that the inhibition towards considering models explanatory outlived the dismissal of Hempel's account during the debate on scientific explanation.

Though we would not find philosophical accounts that explicitly tried to address explanation through models, there are authors that called for such an enterprise or touched on the issue while pursuing different philosophical aims. Such is Cartwright's thesis in her 1983 book *How the Laws of Physics Lie* in which she states her simulacrum account on explanation rather as a manifesto that remained much unsubstantiated. For Cartwright "to explain a phenomenon is to find a model that fits into the basic framework of the theory and that thus allows us to derive analogues for the messy and complicated phenomenological laws

[17] This idea is mainly articulated in Margaret Morrison and Mary S. Morgan, "Models as Mediating Instruments," in *Models as Mediators*, 10-37.

[18] Carl Hempel, "Aspects of Scientific Explanation,*"* in his *Aspects of Scientific Explanation and other Essays in the Philosophy of Science* (New York: Free Press, 1965), 331-496.

which are true of it."[19] Besides this programmatic claims, her position is rather offensive and critical towards the 'classical' view on laws and Hempel's conception of explanation: "The simulacrum account is not a formal account. It says that we lay out a model, and within the model we 'derive' various laws, which match more or less well with bits of phenomenological behaviour. But even inside the model, derivation is not what the D-N account would have it be."[20] Her position seemed to be isolated in the philosophical landscape of that time. Only in the '90s we find few more accounts taking into consideration explanation through models. Such is Hughes'[21] account that addresses a particular kind of explanation, the one through theoretical models. His proposal is part of a collective effort of more philosophers especially in the second part of the '90s (such as N. Cartwright, M. Morrison, Mary Morgan) that aim to restore the importance of the modeling topic in philosophy of science. Their view backed a rather more pragmatic and pluralistic approach on models. In Morrison pragmatic view models are autonomous agents in the production and manipulation of scientific knowledge. Though her investigations reveal new aspects of the modeling activity it doesn't say much about explanation through models. Under the general claim that "it is models rather than abstract theory that represent and explain the behavior of physical systems"[22] she adds that "a model explains the behavior of the system because it contextualizes the laws in a concrete *way*."[23] But this "concrete way" cannot be made more explicit. This could be seen as a limitation of her pure pragmatic and functionalist approach.[24] In fact we could say more generally that the main consequence of the work of the modelistic camp in the '90s was to reveal the richness of the issues related to the modeling activity and to stimulate the research on the topic.

[19] Nancy Cartwright, *How the Laws of Physics Lie* (Oxford: Oxford University Press, 1983), 161.

[20] Cartwright, *How the Laws*, 161.

[21] R.I.G. Hughes, "Theoretical Explanation," *Midwest Studies in Philosophy* 18, 1 (1993): 132-153.

[22] Morrison, "Models as Autonomous Agents," 53.

[23] Morrison, "Models as Autonomous Agents," 64.

[24] I've discussed this in more detail in my doctoral thesis *Explanation and Understanding through Scientific Models. Perspectives for a New Approach to Scientific Explanation* (PhD diss., University of Munich, 2009).

IV. A plausible general frame for approaching explanation through scientific models

I'll further present a sketchy account on explanation through models advanced by Roman Frigg and Stephan Hartmann (the LOOP account as they call it). Though it was never published but only presented at a conference[25] it lays out in my opinion a promising direction to be followed. After presenting it I'll point to some further necessary steps that I see as immediate entries in a working agenda seeking to implement this approach.

Frigg and Hartmann's account unfolds under a representational approach on models – approach that sees models as primarily being representations – but according to the authors it does not require an explicit account of representation. They claim to follow the previous suggestions of Rom Harré[26] and Nancy Cartwright (already discussed). For Harré and Cartwright explanation provides us a picture of the facts and it is given through representation in models. So in their account the representational function of models is central. Nevertheless, they don't require a worked out theory of representation in order to articulate their account. Only a general picture is assumed through which model represents a target system or some part of it.

In their account the explanandum is a feature or propriety of the target system or an event or phenomenon within this system. They exclude other types of explanadum from their account. The explanans on the other side is the model itself. The problem is then: how does a model M explains an occurrence O exhibited by the target system T that is represented by M? Their account specifies four steps that make out the process of an explanation. The first two steps are called identification steps. In the first one we identify the occurrence in the target (OIT as they call it), i.e., the behavior of interest in the target system that has to be explained. Using their example, Boltzmann ideal gas model in which the gas is represented through an ensemble of a huge number of hard balls moving in a confined space under Newtonian classical laws, the OIT is the expansion of the gas in the entire volume of the container when a separating wall is removed. In the second identifying step, the occurrence in the model (OIM) is identified, i.e., the element in the model that corresponds to the occurrence in the target that we wish to explain. In our example it corresponds to the spreading of the balls in the entire volume.

---

[25] At the conference *Philosophical Perspectives on Scientific Understanding,* Amsterdam August 25-27, 2005.

[26] Rom Harré, *An Introduction to the Logic of the Sciences* (London: Macmillan, 1983).

The next two steps are called the explanatory steps. In the first one, called explanation_1 we have to reproduce the OIM in the model, meaning that the OIM has to follow from the basic assumptions of the model. 'Follow' is not made more explicit in any way but is not reducible to deduction as in Hempel's model. In the mentioned example of the ideal gas one has to show that the approach to equilibrium follows from the assumptions about the balls (the fact that are hard, that they collide elastically, etc). The fourth and last step, called explanation_2, involves the translation of knowledge obtained in the model (and about the model) to the target system. In our example, we know that the balls bounce around such that they reach the equilibrium distribution (what Boltzmann proved) and that the balls are idealizations of a certain kind of the molecules. This way what holds true in the model approximately carries over to the real system.

Having laid out the above sketchy account before proceeding further I want to emphasize two important points. The first one is the fact that the above approach does not have to be taken as a general schema for all sorts of scientific explanations. I do back up the conviction that is shared by many philosophers today that the variety of scientific explanations cannot be captured by a single general schema. The LOOP approach addresses a specific type of explanation – the one through scientific models – and it is possible that even this scope has to be better qualified. The second point I want to make is that the LOOP schema is an empty, quite unsubstantiated schema that has to be filled out. The authors recognize this and see the needed content to be delivered by the different types of representations corresponding to different explanatory strategies. I will address this issue in subsequent paragraphs. Nevertheless both above observations emphasize the need of articulating a solution in the sense of a "local philosophy of science," as Nick Huggett[27] calls them. Huggett characterizes such a "local philosophy" as a position in which "philosophical problems are to be found and treated using the resources of more-or-less delineable scientific programs" and "not by trying to make science fit some prior vision."[28]

A last additional observation is related to the precaution the authors take regarding the issue of explanation and truth. I completely agree with their position that an explanation is an explanation due to its 'inner constitution' and not for how good, bad or fruitful it is. We have to bite the bullet if we are going to consider explanation through scientific models.

---

[27] In Nick Huggett, "Local Philosophies of Science," *Philosophy of Science* 67 (2000): 128-137.
[28] Huggett, "Local Philosophies of Science," 128-29.

Now, I think that there are some urgent tasks to be addressed in order to be able to begin filling out the schema. This shows also the directions that I think we have to move. The first one is related to the authors' specification about the different types of representations. I think this is a particular way of approaching the issue of contextualizing the schema and it is too connected to the problem of scientific representation. We make this way the solution dependent on a solution to another philosophical problem that proved to be recalcitrant. Instead of looking too far we should take some baby steps and engage in exploratory investigations. In this sense we might first look at different types of models and the way they are used in providing explanations. A causal mechanism, for example, in molecular biology, will constitute a different sort of explanation than a theoretical model exhibited through a differential equation. So, the further investigation should take into account specific fields and subfields of science in order to fill out the schema.

An immediate task that we have to undertake if we are to search for filling the schema is to get more clarification on the four steps invoked by the schema. The least problematic seems to be the third step, i.e. the explanation_1 or explanation in the model. All the other steps raise issues that had to be addressed in the first move. As in the previous paragraph, I think that a proper clarification can be done only in the limits of a specific scientific context, comprising a specific type of explanation in a particular scientific field.

The two identification steps are not unrelated and the 'correspondence' between the two occurrences, i.e., OIT and OIM, has to be clarified in the larger frame of the 'correspondence' or the relation between models and the target system. We have to take into account the fact that the two identification steps engage different sorts of means: the OIT involves rather an experimental setting (as in the ideal gas example) meanwhile OIM uses conceptual means. Nevertheless the account could cover (though this is not intended by the authors) also cases in which we model the target system through another material system – as would be the case of a scale model.

The last step appears as the most unusual one. Nevertheless I see it as a nice feature of the schema due to the fact that it did not end the explanatory act in its final product but opens it to the potential implications it might have. It facilitates this way the embedding of explanatory inquiry into the more comprising frame of scientific inquiry. The step is defined by the transfer of knowledge obtained in the model to the target system. This action should relate us to another important issue, which I see as necessary to be addressed. It is the issue of understanding.

In the literature of scientific explanation there are positions that ignore or even reject as inappropriate any approach on understanding (as Hempel's one); but

there are also approaches that seek to account for understanding. Friedman manifestly requires that an explanation account should provide also an insight on how understanding is gained and how it relates to explanation. For Friedman scientific understanding is a 'global affair' given by the global aspects of the explanation, i.e., "the relation of the phenomenon in question to the total set of the accepted phenomena."[29] Following Friedman's ideas, scientific understanding is construed through the connection to the unification of a body of knowledge in the unificationist accounts of Kitcher, Schurz or Barthelbort. These positions assume the global character of understanding and are also the most articulated accounts on understanding.

In order to claim understanding from models we'll have to look at the local level and the specific characteristics that a local sort of understanding might have. Salmon makes room for such a sort of understanding (besides the global one) – understanding of causal mechanisms – in the frame of its causal account of explanation.[30] In the case of models we should expect a variety of types of understanding corresponding to the different types of representations they exhibit.

There are fundamental differences from the sort of global theoretical understanding that was usually considered. We should rather focus on understanding in practice (as Morrison also briefly suggested). In this frame a quite important difference is the one between the understanding obtained in the model and the one claimed over the target system. Hi analyzed this distinction for theoretical models from condensed physics[31] but we might consider it for other types of models. The last step of LOOP-schema extends the understanding gained in the model to the system. This sort of understanding has to be qualified by the specific epistemic engagements that the modeling action involves. The understanding that builds up at this moment could be claimed as the one delivered through models. One might claim also the existence of the other moments of understanding that are generated in the other steps of the schema. In this case we need to know how they related to each other and what is the impact on the final understanding.

So, overall, we might say that there is some urgent work to do in order to make the LOOP-schema a workable solution. The main message is that the

---

[29] Friedman, "Explanation and Scientific Understanding," 18.

[30] Salmon, *Causality and* Explanation, 79-92.

[31] In his paper "The Nature of Model-Based Understanding in Condensed Matter Physics," *Mind and Society: A Journal of Cognitive Studies in Economics and Social Sciences* 3 (2002): 81-91.

schema has to be implemented on specific contexts and it might provide a fruitful general frame for investigating the explanatory virtues of models of specific kinds.

Conclusions

In order to conclude we might say that the actual landscape of philosophy of science and the recent results from the last two decades require the reconsideration of the classical topic of scientific explanation. Instead of dismissing it as irrelevant one should take seriously the challenges of resettling it in a new frame of inquiry. I've tried to suggest such a frame and to argue for its pertinence. My argumentation draws on the recent advancements in the philosophy of science on the subject of scientific models that would provide this general frame. It reveals also the fact that we might encounter a plethora of new sorts of problems that we have to address. Nevertheless as it stands now a new and rich field of research opens for the interested philosopher of science.

INFINITISM AND PRACTICAL CONDITIONS ON JUSTIFICATION

Jeremy FANTL

ABSTRACT: This paper brings together two recent developments in the theory of epistemic justification: practical conditions on justification, and infinitism (the view that justification is a matter of having an infinite series of non-repeating reasons). Pragmatic principles can be used to argue that, if we're looking for an 'objective' theory of the structure of justification – a theory that applies to all subjects independently of their practical context – infinitism stands the only chance at being the correct theory.

KEYWORDS: infinitism, pragmatic encroachment, reasons, justification

According to epistemic infinitism, as I will construe the view, in order for your reasons to justify some proposition for you, your reasons must constitute an infinite set. In this paper I argue that recent work on so-called 'pragmatic encroachment' in epistemology provides the infinitist with a novel way to argue for her view. To set the stage for this argument I'll first say a bit about what it means for a set of reasons to 'justify' a proposition.

I. Three Seemingly Incompatible Principles

I soon will have difficulty breathing. How do I know? I am allergic to peanuts and I just ate a peanut butter sandwich: I still have the taste in my mouth. Allergies to peanuts – mine in particular – result in difficulty breathing shortly after the ingestion of products containing peanuts; and, of course, peanut butter – an essential component of peanut butter sandwiches – contains peanuts.

This is an incomplete list of the reasons I have bearing on the proposition that I will soon have difficulty breathing. Not mentioned in the list are various reasons having to do with my memories of consuming the sandwich and my own peanut allergy and reasons having to do with induction and the stability of laws of nature. If we were so inclined we could distinguish between those reasons that are 'background assumptions' and those reasons that are more operant in the formation of my belief. Such niceties aside, it is justified for me that I soon will have difficulty breathing. I mean the 'justified' here in an 'outright' sense; it's not just that there is some evidence that I soon will have difficulty breathing. There's an attitude I should have toward that proposition – the attitude of outright belief.

What justifies that proposition for me are the reasons I have that bear on the proposition that I soon will have difficulty breathing.

The set of reasons I have bearing on that proposition is not the only set that could justify that proposition for me. Here's another: a very strong man is approaching me. He just told me that he is going to strangle me and he is an honest man. He has a motive to strangle me: I took the parking space he'd been waiting to pull into. Strangulation, when done properly, cuts off a person's air supply, and the man approaching me is a man who can strangle properly. These are not reasons I have, but if I did have those reasons, rather than the set of reasons I do have, it would similarly be justified for me that I soon will have difficulty breathing.

Finally, there are sets of reasons such that, even if I had the reasons in those sets, it would not be justified for me that I soon will have difficulty breathing. For example, that I'm not in the pool – I'm 10 feet away – and that I am an excellent swimmer: if the reasons in this set exhaust the reasons I have bearing on the proposition that I soon will have difficulty breathing, then it is not justified for me that I soon will have difficulty breathing. Of course, this small set of reasons could be part of a larger set of reasons bearing on *p* that would confer justification for me if I had the reasons in the larger set (add, for example, a reason to the effect that I am unsupported above the pool and have a cramp in my leg). But if the smaller 'close to the pool' set exhausts the reasons I have bearing on the proposition that I soon will have difficulty breathing, it is not justified for me that I soon will have difficulty breathing.

The type of theory that tells us how propositions come to be justified for you by sets of reasons is normally called a theory of the structure of justification. The above observations suggest that such theories have at least two tasks. First, the *structure*-task: tell us what distinguishes the 'peanut allergy' and 'angry strangler' structures from the 'close to the pool' structure, and what distinguishes the 'close to the pool' structure from the larger structures of which the 'close to the pool' structures are a part. More generally, distinguish between those sets – or 'structures' – of reasons bearing on *p* that are potentially justification-conferring structures and those that aren't.

Second, the *relation*-task: tell us what relation is such that, when you stand in that relation to a potentially justification-conferring structure, the structure is yours. The 'angry strangler' structure is not my structure because I am not related to that structure in the right way. For one thing, my epistemic position with respect to many of the reasons in the structure is too weak: I may even know that many of the reasons are false. This is not the case with respect to the reasons in

the 'peanut allergy' structure. With respect to those reasons, my epistemic position is very strong. We can suppose, even, that I know all of them to be true.

Some ways for a theory to fulfill the relation-task render the structure-task otiose. Suppose a theory says that a structure is yours just in case you have further good reasons for all the reasons in that structure and that the reasons in the structure are situated, for you, in a larger structure of reasons that, all-together, makes it the case that *p* is justified for you. If that is the way the theory fulfills the relation-task, then it is all too easy to fulfill the structure-task: all structures are potentially justifying structures. Even the 'close to the pool' structure, on this way of fulfilling the relation-task, justifies the proposition that I soon will have difficulty breathing. Any theory, then, can render the structure task otiose by loading the differences between justification-conferring and non-justification-conferring structures into the fulfillment of the relation-task.

That blurs the two different ways that a structure of reasons bearing on *p* can leave *p* unjustified for you – the way in which the 'angry strangler' structure does and the way in which the 'close to the pool' structure does. What we want out of a theory of the structure of justification is a specification of potentially justifying *complete* structures and a specification of the *intrinsic* relations you might bear to those structures such that those structures confer justification for you, where a relation is *intrinsic* just in case your standing in that relation to the structure isn't even partially constituted by your standing in that relation to reasons bearing on *p* outside the structure.

When you stand in the right intrinsic relations to the reasons in a structure, say that you 'have' those reasons and that the structure is 'yours.' When a structure of reasons bearing on *p* is a potentially justification-conferring complete structure – when, were the structure yours, p would be justified for you – say that the structure 'justifies' *p* and that *p* is 'justified by' that structure. On these senses of 'justifies' and 'yours,' theories of the structure of justification should all respect the following Justifying Structure Principle: for all structures of reasons (R), bearing on some proposition (*p*), and subjects (you),

> (Justifying Structure Principle) If R justifies *p* and R is your structure, then *p* is justified for you.

A reason *bearing on p* is a proposition such that your strength of epistemic position with respect to that proposition is relevant to your strength of epistemic position with respect to *p*. Some reasons bear on *p* only when situated in a structure of other reasons, and perhaps against background assumptions. That you just ate a peanut butter sandwich is not normally a reason bearing on the

proposition that you soon will have difficulty breathing, but it can be when situated in a structure that includes the proposition that you are allergic to peanuts.[1]

What is it for *R*, rather than some other structure, R', to be your structure of reasons? Perhaps for *R* to be your structure is simply for you to have all the reasons in R. This would allow R to be your structure while you have defeaters for R and for *p*. Suppose R consists of the propositions that you just ate a peanut butter sandwich and that you are allergic to peanuts. Let *p* be the proposition that you soon will have difficulty breathing and suppose that *p* is justified by R. Both "you just ate a peanut butter sandwich" and "you are allergic to peanuts" are reasons you have bearing on *p*. If for R to be your structure is simply for you to have all the reasons in R, then, by the Justifying Structure Principle, it is justified for you that you soon will have difficulty breathing. But suppose "the peanut butter in the sandwich you just ate is 'allergy-safe'" is a reason you have bearing on *p*. Then it is not justified for you that you soon will have difficulty breathing, which would make the Justifying Structure Principle's consequent false. Therefore, for the Justifying Structure Principle to be true, that R is your structure must entail that you not only have all the reasons in R, but that the only reasons you have bearing on *p* are reasons in R. That the peanut butter in the sandwich you just ate is allergy-safe is a reason you have bearing on *p*. Because that proposition is a reason you have bearing on *p*, R isn't your structure, even though you have all the reasons in R.[2]

But when exactly *is* R your structure? Different kinds of theories might seek different kinds of answers to this question. *Objective* theories might seek for context-neutral answers: what epistemic relation is such that, necessarily, when a subject bears that relation to a justification-conferring structure, *p* is justified for that subject? *Subjective* theories might seek for context-dependent answers: what epistemic relation is such that, when you, in your specific practical context, bear that relation to a justification-conferring structure, *p* is justified for you?

---

[1] This may require some background assumptions, like "peanut allergies tend to result in difficulty breathing soon after the consumption of peanut-products." Alternatively, the proposition that you are allergic to peanuts should itself count as a background assumption. Either way, your structure of reasons includes both the proposition that you are allergic to peanuts and that peanut allergies tend to result in difficulty breathing soon after the consumption of peanut-products.

[2] In order for R to be your structure, need it only be the case that you have each reason in R? Or must you have the conjunction of those reasons as well? There are lottery considerations, here, though not decisive ones. Nothing of substance hinges on it, and I leave the matter open.

Objective theories have this advantage: they apply to all subjects equally, regardless of practical context – independent of needs, goals, purposes, stakes, desires, hopes, and fears. There are some for whom this latter feature will seem like a disadvantage. Good epistemological theories, they will say, *do* take into account the particular practical contexts in which subjects find themselves. But they will say this, presumably, because they think are no intrinsic epistemic relations and complete structures of reasons such that, necessarily, if you bear those relations to those structures, *p* is justified for you, no matter what your practical context is. We can think this, though, while agreeing that it would be a good thing if some objective theory were true and also agreeing that it would be valuable to figure out what the true objective theory is, if there is a true objective theory. And, of course, it's difficult to know whether some objective theory is true unless one first investigates what such a theory would say.

It's clear that many of the traditional theories of the structure of justification do strive to be objective. This may be partly because it has been thought that if two subjects in different contexts bear the same epistemic relation to the same structure, *p* will end up justified for both, or for neither. On that assumption, there is no fundamental difference between subjective and objective theories. But even if that assumption is false, it is still worthwhile to search for objective theories.

On objective theories, the 'having' relation is purely epistemic. Whether *p* is a reason you have, according to any objective theory, is determined solely by what intrinsic epistemic relations you stand in to *p*: two subjects who stand in the same intrinsic epistemic relations to *p* either both have it or neither do. The 'your' relation is also purely epistemic. Because whether a structure of reasons is yours depends only on your intrinsic epistemic relations to reasons bearing on *p*, two subjects who stand in the same intrinsic epistemic relations to all those reasons will share the same structure of reasons bearing on *p*. More formally, objective theories of the structure of justification are committed to the following Objectivist Presumption: for all subjects (you) and structures of reasons (R), there is an intrinsic epistemic relation (E) such that

(Objectivist Presumption) If you stand in E to R, then R is your structure.

Seemingly in tension with this Objectivist Presumption is recent work alleging a tight connection between knowledge and action. On these views, whether a subject knows or is justified is importantly related to what the subject cares about or is rational in doing or what sound practical reasoning is available to the subject or what practical reasons the subject has or what counterpossibilities

are salient to the subject.[3] To use (misleadingly) Jon Kvanvig's evocative phrase, there is 'pragmatic encroachment' in epistemology.

The reason for the 'misleadingly' parenthetical is that to affirm pragmatic encroachment, in the strict sense, is not just to affirm that there are conceptual connections between knowledge (or justification) and the practical. It's to affirm that knowledge (or justification) that *p* can come and go with changes only in your practical context – even if your epistemic position with respect to *p* remains constant. Some who have argued for conceptual connections between knowledge and the practical – e.g. Unger and Hyman – have not gone on to draw the more radical conclusions. Nonetheless, I lump their views in with the more recent 'pragmatic encroachers,' like Hawthorne, Stanley, and Fantl and McGrath, as far as the label goes. I do not include this stronger view as a condition on what I am labeling 'pragmatic encroachment' here. All I assume is the more modest claim – that, for example, what you know has consequences for what you should do.

Even among those who advocate such claims, there are differences in the details. For example, there are debates about whether knowing that *p* is necessary or sufficient for being rational to act on *p*.[4] Because I am interested in what lessons

---

[3] Advocates include (among others) Peter Unger, "Two Types of Skepticism," *Philosophical Studies* 25 (1974): 77-96; John Hyman, "How Knowledge Works," *The Philosophical Quarterly* 49, 197 (1999): 433-51; Jeremy Fantl, Matthew McGrath, "Evidence, Pragmatics, and Justification," *The Philosophical Review* 111, 1 (2002): 67-94, and Jeremy Fantl, Matthew McGrath, *Knowledge in an Uncertain World* (Oxford: Oxford University Press, 2009); John Hawthorne, *Knowledge and Lotteries* (Oxford: Oxford University Press, 2004); Jason Stanley, *Knowledge and Practical Interests* (Oxford: Oxford University Press, 2005); and John Hawthorne and Jason Stanley, "Knowledge and Action," *Journal of Philosophy* 105, 10 (2008): 571-90.

[4] Hawthorne recommends a sufficiency condition for knowledge: "one ought only to use that which one knows as a premise in one's deliberations." (Hawthorne, *Knowledge and Lotteries*, 30) Stanley has a similar principle (which in his "Replies to Gilbert Harman, Ram Neta, and Stephen Schiffer," *Philosophy and Phenomenological Research* 75, 1 (2007): 201, he labels "KAP"): "one should act only on what one knows." (Stanley, *Knowledge and Practical Interests*, 9) Fantl and McGrath endorse necessary conditions on knowledge and the justification required for knowledge: "S knows that *p* only if S is rational to act as if *p*" (Jeremy Fantl, Matthew McGrath, "On Pragmatic Encroachment in Epistemology," *Philosophy and Phenomenological Research* 75, 3 (2007): 559) and "S is justified in believing that *p* only if S is rational to act as if *p*." (Fantl, McGrath, "Evidence, Pragmatics," 78) And, in a change of heart, Stanley ("Replies," 202) says, "I now think it was a mistake to emphasize KAP to the exclusion of other possible connections between knowledge and action," endorsing instead a "considerably less contentious principle that knowing that *p* is *sufficient* for acting on one's belief that *p*" (or, in other words, a "far less controversial claim that if one knows that *p*, it is

the core of the literature can teach us about the structure of justification, I will discuss mainly what seems to be the growing consensus among those who advocate knowledge-action connections – that, at the very least, the following Practical Condition on Knowledge is true: for all propositions (*p*) and subjects (you),

> (Practical Condition on Knowledge) you know that *p* only if you are rational to act on *p*.[5]

I recognize that the growing consensus among pragmatic encroachers is far from being the consensus among others, though much of the hostility to principles like the Practical Condition on Knowledge is directed either at the other direction of those principles or to the more radical contention that knowledge can come and go with changes merely in your practical situation. It is an interesting matter what the consequences of the more modest pragmatic principles might be, and that is what I investigate here.

What is it about knowledge that guarantees, as the Practical Condition on Knowledge states, that if you know that *p*, you are rational to act on *p*? That you are rational to act on *p* does not follow from your believing that *p*. For you might believe *p* even if believing *p* is irrational for you, which would make your acting on *p* in some important way irrational. Nor is whether you are rational to act on *p* dependent on whether *p* is true. *p* might be false, though it is irrational for you to believe that *p* is false. *p* might be false though justified for you. In that case, you would be rational to act on *p*, though *p* is false. Finally, whether you are in a Gettier-type situation is irrelevant, in the sense intended, to whether you are rational to act on *p*: when you are in a Gettier-type situation, it is justified for you that the Gettier-type situation does not obtain. Whatever it is about knowledge that guarantees that you are rational to act on *p* when you know that *p*, it cannot be that you believe that *p*, that *p* is true, or that you are not in a Gettier-type

---

rationally permissible to act on one's belief that *p*.") Hawthorne joins Stanley in arguing for both directions in their "Knowledge and Action." And Hyman endorses both directions in his "How Knowledge Works": "the fact that *p* cannot be *A*'s reason for doing something if *A* does not know that *p*" (448) and "if *A* knows that *p* then the fact that *p* can be *A*'s reason for doing something." (450)

[5] The expression "act on *p*" is ambiguous between "use *p* as a reason for acting in accordance with *p*" and "act in accordance with *p*." I favor the latter.

situation. It must be that *p* is justified for you.[6] Therefore, if the Practical Condition on Knowledge is true, so also, plausibly, is a companion principle about justification:

> (Practical Condition on Justification) *p* is justified for you only if you are rational to act on *p*.

Here, then, are three principles that seem to be in tension:

> (Justifying Structure Principle) If R justifies *p* and R is your structure, then *p* is justified for you.

> (Objectivist Presumption) If you stand in E to R, then R is your structure.

> (Practical Condition on Justification) *p* is justified for you only if you are rational to act on *p*.

Here is an argument that these three principles are mutually inconsistent: first, because E is an intrinsic epistemic relation between you and R, then whether you stand in E to R can't vary with variations in non-epistemic features of your situation. Therefore, on the Objectivist Presumption, if you stand in E to R, then whether R is your structure can't vary merely with variations in non-epistemic features of your situation. Neither can whether R justifies p vary with variations in non-epistemic features of your situation, because whether R justifies p – whether R is a potentially justification-conferring structure – is an objective relation between R and p. Therefore, by the Justifying Structure Principle, if R justifies p and R is your structure, whether p is justified for you can't vary with variations merely in non-epistemic features of your situation.

However, whether you are rational to act on p can vary (or so it seems) merely with variations in non-epistemic features of your situation. So, by the Practical Condition on Justification, even if p is justified for you, whether p is justified for you can vary merely with variations in non-epistemic features of your situation. Contradiction: even if R justifies p and R is your structure, whether you are rational to act on p both can and cannot vary merely with variations in non-epistemic features of your situation.

Is there any way for all three principles to be true? I'll argue in this paper that there is, but only if epistemic infinitism is true. The three principles therefore

---

[6] For more fully developed arguments see Fantl and McGrath, *Knowledge in an Uncertain World*, 96-111.

provide heretofore unnoticed resources for epistemologists looking to argue in favor of epistemic infinitism. A full development of that sort of argument would require sufficient positive support for each of the three principles. There is not space in this paper to both do that and show that the three principles together entail infinitism. The work mounting defenses of each of the principles themselves must wait until another time.

Therefore, the modest goal of this paper is simply to show that if those three principles are true, then infinitism is true: no objective theory of the structure of justification that allows finite structures to justify can be true. Such theories include all objective versions of foundationalism and some objective versions of coherentism. If the Objectivist Presumption is true, there is no finite structure of reasons bearing on any proposition such that, necessarily, the proposition is justified for you if that structure of reasons is yours. To use the shorthand: no proposition is justified by any finite structure of reasons.[7]

II. The Argument from the Practical Condition on Justification

If the Practical Condition on Justification is true, then for all finite structures of reasons (R), all propositions (*p*), and all intrinsic epistemic relations (E), you might stand in E to R while *p* fails to be justified for you. Here's why:

Suppose the Practical Condition on Justification. And suppose, for reductio, that there is a proposition, *p*, that is justified by some *non-maximal* structure of reasons R, where R is non-maximal just in case there is a larger structure of reasons, R+, of which R is a part, such that *p* is more justified for any subject whose structure is R+ than for any subject whose structure is R. By the Objectivist Presumption, there is an intrinsic epistemic relation, E, such that if you stand in E to R, then R is your structure. Suppose you stand in E to R. R, then, is your structure. Because R is non-maximal, though, the stakes might be high enough that you are not rational to act on *p*.[8] By the Practical Condition on Justification, *p* is not justified for you, even though R is your structure of reasons. By the Justifying Structure Principle, therefore, *p* is not justified by R. This contradicts

[7] The influential defender of infinitism is Peter Klein. See, for example, his "Human Knowledge and the Infinite Regress of Reasons," in *Epistemology*, ed. James E. Tomberlin, *Philosophical Perspectives* (Cambridge: Blackwell, 1999), 297-326. The most extended single-text defense of infinitism is Scott F. Aikin, *Epistemology and the Regress Problem* (New York: Routledge, 2011).

[8] The argument for this premise is below.

the assumption for reductio. Therefore, there is no proposition that is justified by any non-maximal structure of reasons.

This does not get to the conclusion that no propositions are justified by any finite structure of reasons. It allows that propositions can be justified by finite but maximal structures of reasons – finite structures of reasons whose degree of justification conferred could not be increased with further reasons. Were your structure of reasons for a proposition finite but maximal, it is less plausible that a change in stakes could affect whether you are rational to act on the proposition. The degree to which the proposition is justified would be maximal and, it seems, could survive any raising of the stakes.

Unfortunately, all finite structures of reasons are non-maximal. There is no finite structure of reasons bearing on any proposition such that the degree to which the proposition is justified could not be increased with further reasons. This even goes for necessarily true propositions and seemingly directly evident propositions like "I have a headache." There are worries about fallibility even regarding propositions as compelling as these, and if you could answer objections to error theories in math and logic, or demonstrate that this is one of those cases in which introspection has not led you astray, the degree to which those propositions are justified would increase. You might be able to know such propositions without those further reasons. It's just to say that it's possible to increase the degree to which even those propositions you are most sure of are justified, by adding further reasons to a finite structure; there are no maximal finite structures of reasons. If there are no maximal finite structures, then the above conclusion – that no proposition is justified by any non-maximal structure of reasons – entails the stronger conclusion – that no proposition is justified by any finite structure of reasons.

If it is always possible for further reasons to increase the degree to which a proposition is justified, then it is likewise possible for your pragmatic situation to be such that, unless the degree to which the proposition is justified has increased, you ought not act on it. There will, for example, always be a hypothetical gamble which, if offered, you would not be rational to take, even if it now is rational to act on the proposition, having not been offered the gamble. As Hawthorne says, "I wouldn't even bet on the law of noncontradiction at any odds, and I think myself rational on that score."[9] By the Practical Condition on Justification, then, even the propositions you are most sure of can become unjustified for you. And, given the

[9] Hawthorne, *Knowledge and Lotteries*, 29.

Justifying Structure Principle, it follows that not even the propositions you are most sure of are justified by any finite structure of reasons.

Is it really true that, given the right gambles, you might fail to be rational to act on even the propositions you are most sure of – e.g. that you have a headache? The argument relies on two premises:

> 1. the degree to which the true proposition that you have a headache is justified is not maximal; it can be increased with further reasons.
>
> 2. if the degree to which the true proposition that you have a headache is justified can be increased with further reasons, then there are states of affairs in which you are not rational to act on the true proposition that you have a headache.[10]

If both of these premises are true, then there are states of affairs in which you are not rational to act on the true proposition that you have a headache. By the Practical Condition on Justification, in those states of affairs, it is not justified for you that you have a headache. If the argument is sound, then because the true proposition that you have a headache is as justified for you as propositions get, the lessons are general: if there are states of affairs in which you are not rational to act on the true proposition that you have a headache, then for any proposition, there are states of affairs in which the proposition is true and you are not rational to act on that proposition.

Are the premises true? The first is a consequence of a companion principle to some widely respected defeater conditions. The defeater conditions I have in mind suggest that if it is justified for you that some proposition is unjustified for you, then that proposition is unjustified for you. As stated, this is too strong to be uncontroversial. However, a weaker condition in the neighborhood seems safer. Undercutting defeaters, even if not always sufficient to destroy justification, are at least relevant to the epistemic status of the propositions they undercut: they are relevant to the degree to which the propositions they undercut are justified for you. In short, if, *ceteris paribus*, it becomes more justified for you that p is unjustified for you, then it becomes less justified for you that p (though the degree to which p is justified for you need not fall below the threshold for knowledge or outright justification).

Likewise, we should think there are analogous support conditions. An unnecessarily strong condition might say that if it is justified for you that some

[10] The argument, below, for this premise discharges the promised duty incurred in footnote 8.

proposition is justified for you, then that proposition is justified for you. But if the milder defeat condition above is acceptable, a milder support condition should be as well: *ceteris paribus*, if it becomes more justified for you that *p* is justified for you, then the degree to which *p* is justified for you increases as well.[11]

While it is implausible that you could acquire additional direct, justification-increasing support for the true proposition that you have a headache, it is not implausible that it can become more justified for you that "you have a headache" is justified for you. For, that proposition is about, among other things, the theory of justification and what is required for a proposition to be justified for you. You are not in nearly as strong a position regarding such issues – not in nearly a strong enough position that the justification of propositions in such domains can't be increased with further reasons. Therefore, it can become more justified for you that it is justified for you that you have a headache. Given the milder support condition, it can therefore become more justified for you that you have a headache. If such is possible with propositions about headaches, then such is possible with any proposition.

One worry about this argument is that it might only become more justified for you that you have a headache if you already have doubts about the reliability of introspection. Philosophers might have doubts about introspective beliefs and if it becomes justified for these 'epistemic sophisticates' (to put it kindly) that introspection is reliable in some particular case, then that might shore up for them the proposition that they have a headache. If the doubts had never arisen – a situation they arrived at only after some thought – the original proposition could have remained perfectly well justified – as justified as, or perhaps moreso than, it is once those worries are justifiedly removed. That it can become more justified for you that you have a headache does not entail that it can become more justified for anyone. For many, the degree to which it was justified that they had a headache was never reduced to a degree that allows for improvement.

At issue is the layperson who has never considered doubts which would, if considered, undermine propositions currently justified for him. Is such a layperson epistemically better off than one who has considered those doubts? Meno thought so, telling Socrates,

[11] This mild support condition is put loosely and includes a reference to justification simpliciter. A more official statement of the principle might be this: *ceteris paribus*, for all degrees of justification, d, if it becomes more justified for you that *p* is justified for you at least to degree d, then the degree to which *p* is justified for you increases as well.

> you seem to me both in your appearance and in every other way, to be like the broad torpedo fish, for it too makes anyone who comes close and touches it feel numb, and you now seem to have had that kind of effect on me, for both my mind and my tongue are numb, and I have no answer to give you. Yet I have made many speeches about virtue before large audiences on a thousand occasions, very good speeches as I thought, but now I cannot even say what it is. I think you are wise not to sail away from Athens to go and stay elsewhere, for if you were to behave like this as a stranger in another city, you would be driven away for practicing sorcery.[12] (80a-b)

Meno goes wrong for any number of reasons. For he thinks he is in a better position before considering doubts than after, even though his starting position is in fact quite thoroughly unjustified. Socrates is therefore quite right to respond as he does after interrogating the slave boy:

> Even now he does not yet know, but then he thought he knew, and answered confidently as if he did know, and he did not think himself at a loss, but now he does think himself at a loss, and as he does not know, neither does he think he knows. (84a-b)

It surely is better to live in doubt than to have unjustified belief thought justified. But what of the case in which, by raising unanswered doubts, we have eliminated – or, in the case of headaches, very slightly reduced – genuine justification? Socrates does worry about this, saying, "So now I do not know what virtue is; perhaps you knew before you contacted me, but now you are certainly like one who does not know." (80d) Laypeople's beliefs in their own headaches are, while perhaps thoughtless, nonetheless justified. Why is it better to raise doubts about those beliefs? Might they not be in a better epistemic position precisely because they haven't considered doubts or raised objections – what Catherine Elgin[13] calls, "the epistemic efficacy of stupidity"?

We might think that we are still epistemically better off with a reduction of thoughtless justification in the service of greater wisdom. But this is not to the point. What we are to compare is our epistemic state having raised and *answered* doubts with our state before the doubts were raised. It is surely better to have raised those doubts and justifiedly answered them than not to have had them raised at all. One is epistemically better off after having justifiedly responded to

---

[12] See Plato, "Meno," in *Five Dialogues*, trans. G. M. A Grube (Indianapolis: Hackett Publishing Co., 1981), 59-88.

[13] Catherine Elgin, "The Epistemic Efficacy of Stupidity," *Synthese* 74 (1988): 297-311.

those doubts because one is able to answer objections that one could not answer before, even though those objections would not easily have come to mind. Therefore, the epistemic sophisticate who has justifiedly shored up her worries is in a better epistemic position, not only than the epistemic sophisticate who has not shored up her worries, but than the epistemic naïf who, in virtue of never having considered the worries, never lost any justification for the original belief.[14]

The second premise is that if the degree to which a proposition is justified for you can be increased with further reasons, then there are states of affairs in which your structure of reasons is the unimproved, original structure, and in which you are not rational to act on the proposition. The easiest way to increase the degree to which a proposition is justified for you is to get evidence for it. So, the degree to which it is justified for you that the fair six-sided die came up 2 or higher is pretty high. Once you look at the die and see that it came up 4, the degree to which it is justified for you is even higher. If the epistemic status of the proposition can be improved in this way, it is clear that there are gambles you are rational to take after looking at the die that you are not rational to take before looking at the die. After looking at the die, you are rational to bet 1 dollar on the proposition, with a potential payoff of $1.05. Before looking at the die, you are not. Therefore, there are states of affairs – states of affairs before looking at the die and in which you are offered the relevant gamble – in which you are not rational to act on the proposition that the fair six-sided die came up 2 or higher.

Here, the degree to which it is justified for you that the fair six-sided die came up 2 or higher is increased rather directly by new information. But the degree to which *p* is justified for you can also be increased 'indirectly' merely by it becoming more justified for you that *p* is justified for you. For example, you might find out that your recent bout of delirium tremens – the one that caused you to see all those spots – has finally come to an end. This makes it more justified for you – even after looking at the die and seeing 4 spots on the facing surface – that the fair

[14] Importantly, the epistemic sophisticate who has shored up her worries is more thoroughgoingly rational to act on the relevant propositions than the epistemic naïf who has never considered those worries and can't answer them. The epistemic sophisticate has all the justifying factors in her favor that the naïf has – the direct presentation of the headache, for example. In addition, the sophisticate has a justified answer to certain objections – that, e.g., introspection is not reliable in this situation. Therefore, if a gamble is offered on whether, despite the objection, she still has a headache, the sophisticate will be rational to take it. The naïf, who has never considered the objection and lacks a justified response, will not be rational to take it.

six-sided die came up 2 or higher, because it eliminates a defeater: that the 4 spots you think you see are in fact caused by your DT.

When the degree to which a proposition is justified for you is increased in this way, is it as clear that there are gambles you are not rational to take before the improvement that you are rational to take after the improvement? If we take seriously the claim that the degree to which the proposition itself is justified is increased, the answer is yes. Before it becomes more justified for you that the proposition is justified for you, you are not able to answer certain objections, e.g. "Maybe those 4 spots are just the product of your delirium tremens." Therefore, if the stakes got too high, you shouldn't be able just to assume the die did come up 4. The ability to answer objections, which is what an increase in the degree to which it is justified for you that a proposition is justified for you always allows you to do (provided that the increase is the result of further reasons), also makes more hypothetical gambles rational for you to take.

Therefore, if the degree to which *p* is justified for you can be increased either directly or indirectly by supplementing the reasons in R with further reasons, there are states of affairs in which R is your structure and in which you are not rational to act on *p*. As we have seen, when *p* is supported by only a finite R, the degree to which *p* is justified for you can always be increased with the addition of further reasons. Therefore, by the Practical Condition on Justification, for all propositions (*p*) and finite structures of reasons (R), there is a subject whose structure of reasons is R but for whom *p* is not justified. By the Justifying Structure Principle, *p* is not justified by R. But R is just an arbitrary finite structure of reasons bearing on the arbitrary *p*. So, no proposition is justified by any finite structure of reasons. That's just to say that there is no finite structure of reasons bearing on *p* and no intrinsic epistemic relation such that if you bear that relation to that structure, *p* is justified for you.

This does not mean *p* can't be justified for you if your structure of reasons bearing on *p* is only finite. What it means is that, though *p* might be justified for you whose structure of reasons bearing on *p* is finite, no theory can correctly say, of any particular finite structure of reasons bearing on *p*, that there is an intrinsic epistemic relation such that *p* is justified for anyone who bears that relation to that structure.

Let me be clear about one further point: the conclusion is not just that no finite structure is 'conclusive' or that you are fallible about any proposition for which you have only a finite structure of reasons. The conclusion requires also that no proposition be justified by any inconclusive structure. Some fallibilists might think otherwise. But if we there is pragmatic encroachment on justification,

then no inconclusive structure – so, no finite structure – is sufficient for justification.

We could give up the Objectivist Presumption, of course. But to abandon the Objectivist Presumption just is to abandon the search for an objective theory of the structure of justification. That's because such theories are precisely those that tell us whether *p* is justified for you simply based on your intrinsic epistemic relations to one of the allowed structures of reasons bearing on *p*. One may not care much about finding objective theories of the structure of justification. But there is no denying that many extant theories of the structure of justification are attempts to be exactly that. And 'giving up' on the Objectivist Presumption wouldn't change the fact – if it is one – that there is one theory – infinitism – that is consistent with the Objectivist Presumption, the Justifying Structure Principle, and the Practical Condition on Justification. It is to this argument I turn in the next and final section.

III. The Case for Infinitism

Objective theories of the structure of justification can tell us either what is necessary for justification or what is sufficient for justification. For the former, a theory must tell us what your structure of reasons, R, needs to be like for *p* to be justified for you by R. For the latter, a theory must tell us what R needs to be like so that *p* is guaranteed to be justified for you by R. Ideally, a theory of the structure of justification would tell us both. If only one kind of theory stands a chance at telling us both, that is a powerful reason to favor that kind of theory.

Foundationalism has traditionally been thought to (at least attempt to) tell us both. According to foundationalism, it is required for *p* to be justified for you by R that R contain an immediately justified reason and sufficiently strong connections among the reasons and between the reasons and *p*. The concept of an immediately justified reason is notoriously unclear, but I'll characterize immediately justified reasons as reasons that can be justified for subjects even if those subjects don't have further reasons for them. If one is an externalist, one may be happy allowing many kinds of reasons to be immediately justified in this sense. If one is a traditional foundationalist, one may want to limit such reasons to Chisholmian directly evident propositions.[15] What is important for our purposes is that, on this construal of immediately justified reasons, foundationalism may very

[15] As Roderick Chisholm says, in defining the directly evident, "what justifies me in counting it as evident that *a* is *F* is simply the fact that *a* is *F*." (Roderick Chisholm, *Theory of Knowledge*, vol. 2 (Englewood Cliffs: Prentice-Hall, Inc., 1966), 28.)

well be right about what is required for *p* to be justified for you by R: perhaps *p* is justified for you by R only if R contains reasons that can be justified without further reasons and if there are the right sorts of connections among the reasons in R and between R and *p*.

It is in saying that satisfying this requirement is also sufficient for *p* to be justified for you by R that objective versions of foundationalism go wrong. Objective versions of foundationalism say that if R contains an immediately justified reason and the right sorts of connections, *p* is justified by R. Therefore, it allows that *p* can be justified by R even if R is finite. As we have seen, no proposition is justified by any finite structure of reasons. Though immediately justified propositions can be justified for you even if you lack further reasons, it will sometimes be the case, when the stakes are high enough, that you need further reasons bearing on *p* – reasons that answer important objections – if you are to be rational to act on *p*. By the Practical Condition on Justification, your structure of reasons may be one of the foundationalism-licensed structures, yet *p* fail to be justified for you. By the Justifying Structure Principle, *p* is not justified by that structure of reasons. Therefore, the foundationalist sufficiency condition is false.

If *p* cannot be justified by finite structures, then the natural move is to an infinitist theory of the structure of justification – a view that makes justification a matter of having an infinite series of non-repeating reasons. A rough sketch of such a view might say that, for *p* to be justified by R, R must be infinite. A parallel sufficiency condition might say that, when R is infinite, *p* is justified by R (provided R satisfies some other conditions[16]). But this is too quick. Even if *p* is justified in this sense by an infinite R, the degree to which *p* is justified might still be increased with further reasons. An infinite structure of reasons need not include every reason. If there is some objection that goes unanswered by the infinite R, then even if you (miraculously) have R, the stakes can be raised so that the objection becomes pressing: you will no longer be rational to act on *p*, and so *p* will no longer be justified for you, despite your having the infinite R. By the Justifying Structure Principle, then, *p* is not justified by the infinite R (though *p* could be justified for you were you to have R and your stakes to remain low).

[16] Of course, not just any infinite structure will do – just as, for foundationalists, not just any finite structure will do. For the infinitist, only an infinite structure of good reasons (for Klein, objectively and subjectively available reasons) bearing the right relations to each other suffices for justification.

What we need is something stronger – a structure of reasons bearing on *p* such that, when that structure is yours, the degree to which *p* is justified for you cannot be increased with further reasons. As an initial attempt, say that *p* is justified by R when R is constituted by an infinite series of reasons in support of *p* and an infinite series of reasons in answer to all possible challenges to all inferences and reasons in R.[17] I have no decisive argument that, were your structure of reasons to be a structure of this sort, the degree to which *p* is justified for you could not be increased with further reasons. But it is hard to imagine what sort of reason could be added to such a structure that would increase the degree to which *p* is justified for you. (It might be suggested that you could add a meta-reason to the effect that you have such a structure. But, plausibly, this reason would already be included in the structure you have because, plausibly, such a reason would figure in the answer to certain kinds of challenges.)

What, then of objective versions of coherentism? According to coherentism, it is required for *p* to be justified by R that R be sufficiently coherent and that *p* contribute to that coherence. This necessity condition, like foundationalism's, might well be right: perhaps there has to be a certain coherence within R and perhaps *p* has to contribute to that coherence for *p* to be justified by R.[18] The sufficiency condition is more complicated. According to coherentism, when R satisfies these requirements – when R is sufficiently coherent and *p* contributes to that coherence – *p* is justified by R. The fate of this proposal depends on whether the coherentism in question admits of an infinitist construal.

Laurence BonJour's coherentism, for example, explicitly requires "that the regress of justification does not go on forever, which would involve an infinite number of distinct beliefs, but rather circles back upon itself, thus forming a closed system."[19] BonJour might be saying that, if you have an infinite number of distinct beliefs, *p* cannot be justified for you. Therefore, if you, with a finite, coherent set of beliefs, managed to become a god, with an infinite, coherent set of

---

[17] For more details, see Jeremy Fantl "Modest Infinitism," *Canadian Journal of Philosophy* 33, 4 (2003): 557-58).

[18] Notice that the infinitist's sufficiency condition does not conflict with either the foundationalist's or the coherentist's necessity conditions. The infinitist can accommodate the requirements in explicating the notions of *a reason* and *support*. See, for example, Fantl, "Modest Infinitism," 554-55, and Klein's discussion of the various ways for the infinitist to account for 'objective availability.' (Klein, "Human Knowledge," 299.)

[19] Laurence BonJour, "The Coherence Theory of Empirical Knowledge," in *Empirical Knowledge: Readings in Contemporary Epistemology*, ed. Paul Moser (Totowa: Rowman and Littlefield, 1986).

beliefs, you would lose all justification. This is not plausible. Alternatively, BonJour might be saying that we can't require, for justification, an infinite number of distinct beliefs; you can be justified with a finite number of distinct beliefs. This is so. It's also not a sufficiency condition. It is one thing for there to be a coherent finite structure such that there is a subject whose structure it is and who is justified. It is another for there to be a coherent finite structure such that all subjects whose structure it is are justified. To get a sufficiency condition, we need to say that the smallest system sufficient for justification is finite. This sufficiency condition is false. There is no finite system that is sufficient for justification; *p* is justified by no finite R.

Keith Lehrer's coherentism has no such explicit anti-infinitist stipulation. It requires merely that a subject can adequately respond to all objections:

> S is personally justified in accepting that *p* at t if and only if everything that is an objection to *p* for S on the basis of the acceptance system of S at t is answered or neutralized on the basis of the acceptance system of S at t.[20]

There is nothing antithetical to infinitism here; it's pretty close to the infinitist view recommended above. If we think that the only way that every objection to *p* can be answered using only reasons in R is if R contains an infinite series of reasons in support of *p* and an infinite series of reasons in answer to all possible challenges to all inferences and reasons in R, then Lehrer's (modified) view entails the infinitist view above.

We can ask of any objective version of coherentism whether it allows the structure it says is sufficient for justification to be infinite. If it does, then, suitably modified, it can give an adequate sufficiency condition for justification. If it doesn't, then it can't give an adequate sufficiency condition for justification. Therefore, an infinitist coherentism will be a decent objective theory of the structure of justification, while a non-infinitist coherentism won't be. The issue is whether we need an infinitist element in any objective theory of the structure of justification. I have argued that we do.

[20] Keith Lehrer, *Theory of Knowledge* 2nd ed. (Boulder: Westview Press, 2000), 137.

LIMITING SKEPTICISM

Vincent F. HENDRICKS, John SYMONS

ABSTRACT: Skeptics argue that the acquisition of knowledge is impossible given the standing possibility of error. We present the limiting convergence strategy for responding to skepticism and discuss the relationship between conceivable error and an agent's knowledge in the limit. We argue that the skeptic must demonstrate that agents are operating with a bad method or are in an epistemically cursed world. Such demonstration involves a significant step beyond conceivability and commits the skeptic to potentially convergent inquiry.

KEYWORDS: skepticism, convergence, Peirce, KK

For an agent to have knowledge of some proposition he or she must be able to eliminate all relevant possibilities of error. Furthermore, since Plato it has been assumed that knowledge is robust insofar as it does not vanish in the light of new evidence or information. So, if one ascribes knowledge to an agent, one is proposing, as Jaakko Hintikka puts it,

> … to disregard the possibility that further information would lead him to deny that *p* although he could perhaps imagine (logically possible) experiences which could do just that.[1]

Notice that in the ascription of knowledge to an agent one is still recognizing that the agent could imagine the possibility of being wrong. Error is always conceivable, but conceivable error is not always relevant to knowledge. Knowing *p* involves the right to disregard irrelevant possible worlds in which it is not the case that *p.* Are the seeds of skepticism smuggled in via the putative knower's need to overlook allegedly irrelevant possibilities? As David Lewis notes:

> If you claim that *S* knows that *P*, and yet you grant that *S* cannot eliminate a certain possibility in which not-*P*, it certainly seems as if you have granted that *S*

[1] Jaakko Hintikka, *Knowledge and Belief: An Introduction to the Logic of the Two Notions* (London: King's College Publications, 2005), 18.

> does not after all know that *P*. To speak of fallible knowledge, of knowledge despite uneliminated possibilities of error, just *sounds* contradictory.[2]

Skeptics argue that the acquisition of knowledge is impossible given the standing possibility of error. Who would want to set such strict standards for knowledge acquisition? According to Lewis, either the skeptic or the epistemologist. In ordinary life, by contrast, Lewis claims that we know many things with Moorean certainty. The fact that we know a lot, he writes, "is one of those things that we know better than we know the premises of any philosophical argument to the contrary."[3] However, the Moorean strategy is not, nor was it intended to be, a direct response to the skeptical challenge. Directly confronting the problem would involve meeting the standards that the skeptic sets. Specifically the requirements of infallibility and certainty. Whereas Lewis challenged the reasonableness of these standards, and was content with emphasizing the "fact that we know a lot."[4] This investigation of the skeptical challenge begins by granting the skeptic the benefit of the doubt.

I. The Benefit of Doubt

Perhaps nature is secretive, refusing to reveal itself to our senses or our scientific scrutiny. Even if nature does reveal itself, perhaps we are unable to grasp the meaning of the message. If the truth of an agent's knowledge claim depends on the 'underlying reality' or some other aspect of nature that transcends immediate experience then the truth of his or her knowledge claim is always *per definitionem* going to outstrip her power to certify the truth of her claim to know. A familiar cast of characters and scenarios relies on some version of this problem: Descartes' *malign genie*, Hume's hidden springs of nature, the Duhem-Quine thesis, Kuhn's incommensurability, Putnam's brains-in-vats, and Rorty's advocacy of edification over inquiry. Such skeptical scenarios depend on the assumption that any proposition is systematically underdetermined by any evidence. This is what is known as *global underdetermination*; two worlds ascribe contrary truth values to proposition *p* such that no evidence will favor the choice of one world over the other. This would leave the decision as to which world is actual

[2] David Lewis, "Elusive Knowledge," in his *Papers in Metaphysics and Epistemology* (New York: Cambridge University Press, 1999), 419.
[3] Lewis, "Elusive Knowledge," 418.
[4] Lewis, "Elusive Knowledge," 418.

underdetermined. If this is the case then global underdetermination would render rational inquiry futile.

Responding to the threat of global underdetermination, epistemologists have noted that agents will almost inevitably engage in some partitioning of worlds such that only relevant possibilities of error are dealt with in the process of knowledge acquisition. This nearly ubiquitous epistemic practice is known as *forcing*.[5]

Skepticism plays on more than one string. In addition to global underdetermination the skeptic may also point to local underdetermination. Ever since the Pyrrhonian skepticism of Sextus Empiricus, the problem of induction and its various derivatives have presented a series of challenges to knowledge acquisition. Skepticism about induction is the result of the possibility of *local underdetermination* obtaining between evidence and the proposition. Kevin Kelly defines local underdetermination in the following way:

> A hypothesis is locally underdetermined by the evidence in a possible world if there is an infinite sequence of evidence possible for all the agent knows, such that each initial segment of this evidence sequence could arise independently of whether the hypothesis is true or false.[6]

The definition implies the lack of a determinate point in time after which the agent can reach a decision concerning the truth or falsity of the proposition in question. Skeptical arguments are designed to show that inquiry is in vain either from the outset – as in the case of global underdetermination or that a counterexample is to be found with certainty at some later stage, rendering further inquiry unnecessary. Global underdetermination suggests dropping inquiry apriori, while inductive skeptical worries based on local underdetermination purport to show with certainty that a counterexample will eventually appear.

II. Convergence

Knowledge, as characterized by infallibility and robustness exhibits convergence. The idea that scientific knowledge is convergent may be found in the works of American pragmatists like Charles Sanders Peirce and William James. Peirce held that scientific inquiry asymptocially converges to truth in the limit, and whatever

---

[5] Vincent F. Hendricks, *Mainstream and Formal Epistemology* (New York: Cambridge University Press, 2006).

[6] Kevin T. Kelly, *The Logic of Reliable Inquiry* (New York: Oxford University Press, 1996), 24.

the theories of science say in the limit exhausts truth. For the present purposes, it suffices to say, that convergence simply means that there is a time such that for each later time, the agent is not going to change his mind pertaining to the truth value of the proposition under consideration. Depending on what time that is, different notions of convergence will arise.

First, convergence with certainty means that there is a finite time after which the agent is not going to change his mind about the truth value of the proposition and that he clearly signals his success by going into his designated state of *halting*:

> *Agent S converges to proposition p with certainty if there is a time n such that*
>
> 1) *S signals at n that he is ready to conjecture*,
> 2) *S conjectures p at n* + 1,
> 3) *S does not signal earlier than n that it is ready to conjecture*.[7]

Convergence with certainty is generally viewed as the hallmark of convergence in epistemology. For instance, in response to Hume's problem of induction, hypothetico-deductivism is committed to formulating universal propositions and waiting around for incoming evidence to refute them. When a counterexample is encountered the proposition in question could not possibly be true – output 0, and halt! An existential proposition has a similar property but instead of being refutable it is verifiable with certainty – conjecture the existential hypothesis and wait for the first corroborating instance in the observed evidence. Eureka! The hypothesis is verified with certainty, so stop inquiry and output the truth.

As attractive as certainty convergence may be it is not always possible to obtain this kind of security. Real epistemological problems are not always amenable to convergence with certainty. In these cases one may choose to drop the halting condition but not the requirement of convergence. Limiting convergence allows the agent to oscillate pertaining to his conjecture some finite number of times. This number need not be specifiable in advance. At some point

[7] Note that immediately prior to the certainty conjecture the agent is required to produce a *signal* (say, Eureka!) of certainty. This is due to the fact that the agent (or the method he applies) may produce the sign of certainty more than once. Therefore, the certainty conjecture is taken to be the one following immediately after the first occurrence of Eureka! Subsequent signals of certainty will be "ignored, as though the method has finished its job and is merely producing irrelevant noise thereafter." (Kelly, *The Logic*, 48.)

nevertheless the agent must reach a convergence modulus and stabilize his conjecture even if he does not know when stabilization has occurred. Thus, limiting convergence does not require the agent to report convergence. Peirce considered a similar idea insofar as he took it to be impossible to say anything about the direction of science in the short run while arguing that science may all the same asymptotically approach the truth in the long run. Similarly for James who recognized that knowledge of universal laws may become impossible to acquire if one is obliged to say when science has gotten it right.[8] Limiting convergence may be defined in the following way:

> Agent S *converges to* proposition p *in the limit if there is a time* n *such that for each later time* n': S *conjectures* p *at* n'.

Now, why entertain a notion of convergence but not of certainty of when convergence has occurred? As Phillip Kitcher asks:

> To be sure, there are [Bayesian] convergence theorems about the long run – but as writers from Keynes on have pointedly remarked, we want to achieve correct beliefs in the span of human lifetimes.[9]

Kitcher's objection misses the mark. One can allow that we humans may not achieve many true beliefs in the 'span of human lifetimes' without thereby falling prey to skepticism. If, for instance our species goes extinct next Wednesday without achieving many true beliefs, the skeptic cannot claim victory. Skepticism, after all, is a judgment concerning the possibility of knowledge. Its success as an epistemological thesis should be independent of the date and time of our demise. The contingent fact of when our species ends its run has no bearing on the claim that knowledge will always be undermined by the possibility of error.

By contrast, treating inquiry as a matter of convergence in the limit is fully consonant with the mission of epistemology and science. For if we have to go to the limit to get the truth, then why not wait around for it, even if only in our philosophical imaginations? Therefore, *pace* Kitcher, reasoning about epistemic conditions in the limit is not vitiated by the possibility that our planet could be struck by an asteroid next Wednesday.

---

[8] William James, "The Will to Believe," in his *Essays in Pragmatism* (New York: Hafner Publishing Company, 1960).

[9] Philip Kitcher, *The Advancement of Science* (New York: Oxford University Press, 1993), 293.

Consider the birds in the trees. Note that "There exists a black raven" is verifiable with certainty while "All ravens are black" is refutable with certainty. "All ravens are black" is also verifiable in limit, because if the agent has not encountered the crucial example of a non-black raven leading him to change his mind, and he is not going to change his mind anymore, then the entire problem of whether all ravens are black becomes a trivial decision problem in the limit. Reasoning about the condition of knowers in the limit might sound like a cheap shortcut to the solution of epistemological problems. However, limiting convergence is a characteristic of any scientific practice in which our claims are subject to revision. As computational epistemologists like Martin and Osherson explain:

> The general point is that Ψ is not required to recognize or signal in any way that its conjectures have begun to converge. In this respect our paradigm is faithful to the situation of real scientists, whose theories remain open to revision by new, unexpected data. It is, of course, possible to define paradigms that require scientists to signal convergence. The prospects for success, however, are then diminished.[10]

For instance computational epistemologist Oliver Schulte proves how the identification of conservation principles for particle reactions is a limiting tractable problem and not one tractable with certainty.[11]

Based on certainty and limiting convergence one may formulate the following two notions of convergent knowledge:

Agent S may know proposition p with certainty iff
(a) *p is true*
(b) *S converges to p with certainty*
(c) *in all possible worlds in accordance with one's choice of forcing clause.*

Agent S may know proposition p in the limit iff
(a) *p is true*
(b) *S converges to p in the limit*
(c) *in all possible worlds in accordance with one's choice of forcing clause.*

[10] Eric Martin, Daniel N. Osherson, *Elements of Scientific Inquiry* (Cambridge: MIT Press, 1998), 12.

[11] Oliver Schulte, "Inferring Conservation Principles in Particle Physics: A Case Study in the Problem of Induction," *British Journal for the Philosophy of Science* 51 (2000): 771-806.

Nozick's celebrated definition of counterfactual knowledge is a case in point for certainty convergence. The inherent decision procedure given by avoiding error and gaining truth together with the counterfactual semantics require the agent to converge in all possible worlds sufficiently close to the actual world in order to acquire knowledge of the proposition of interest. Thus, in terms of forcing, a subjunctive conditional is true just in case the consequent is forced among the closest worlds to the actual world in which the antecedent holds. The idea of introducing the proximity relation is that when the agent's local epistemic circumstances suffice for the truth of the consequent, inquiry may as well just halt. Hence, Nozick's proposal uses decision with certainty as the convergence criterion. Consider now the following subjunctive:

> *(1) If the proposition "All ravens are black" were false, agent S would not believe "All ravens are black" 'now'.*

It seems that (1) would not be true unless

> *(2) If the proposition "All ravens are black" were false, S would have observed something different than he has up until 'now'.*

The problem of induction teaches that up until now, the evidence may be all the same, hence no answer with certainty seems to be forthcoming pertaining to this epistemic problem on the counterfactual account. Though Nozick has a forcing strategy to dismiss brains-in-vats and Cartesian demons as genuine possibilities of error given the proximity relation he has no immediate strategy for the problem of induction if the answer is to be had with certainty settled by local circumstances.

If an epistemic problem is solvable with certainty it is also solvable in the limit, but if a problem is solvable in the limit it is not necessarily also solvable with certainty. Consider two methods Q and R. Let Q be Popperian in nature in the sense that if the first observed raven is black, Q will conjecture that all ravens are black and will continue to project that all ravens are black unless a non-black raven is encountered. Method R by contrast is infallible in nature insofar as it does not conjecture anything which is not entailed by the evidence. Thus if a non-black raven is encountered R concludes that not all ravens are black but refuses to produce a conjecture otherwise.

Suppose the actual world is such that not all ravens are black. Then both methods will refute, with certainty, the proposition that all ravens are black. Suppose, on the other hand, that all ravens in the actual world are black, then Q will conjecture that all ravens are black after the first raven has been observed and

will never alter its conjecture. *Q* will converge to the truth of the proposition in the limit. *R*, by contrast, will fail to generate the conjecture that all ravens are black, due to the requirement of infallibility. *R* will therefore fail to converge to the truth. The Popperian method has the virtue of converging to the truth in the limit, no matter what the truth might be, whereas the infallible method does not reliably converge to the truth in all cases.

Local underdetermination crippled Nozick's proposal because a particularly demanding criterion of success for inductive inferences was imposed, namely *decision with certainty*. Thus weakening the convergence criterion to a limiting one allows for more problems to come within the scope of rational inquiry.

III. Certain doubts

Just as epistemologists have favored certainty convergence, so have skeptics. Both global and local underdetermination bring inquiry to a halt; a decisive possibility of error is either in place apriori or is forthcoming soon enough. Gettier-cases also terminate inquiry with certainty: If Jones does not own a Ford car but Brown all the same is in Barcelona then this suffices for getting it right wrongly, and knowledge as true justified belief is undercut with the same kind of certainty.[12] In sum, demonstrating doubt has generally been a short-run strategy to terminate with certainty:

> *Agent S may be in doubt concerning proposition p with certainty if*
> *S produces a counterexample to p.*

This short-run strategy is insufficient to settle the case in favor of the skeptic since the knower may claim knowledge in the limit. Can the skeptic follow him there? How does the challenge of skepticism fare in the limit? First, consider what skepticism would look like in the limit:

> *Agent S may be in doubt concerning proposition p in the limit if*
> *S produces a counterexample to p.*

This limiting version of skepticism seems to miss the point of the challenge. It says that there is a time such that for each later time, *S* produces a

[12] Running through the remaining Gettier-derived counterexamples – including Russell's anticipation thereof with the stopped clock and Balfour being the prime minister when in fact it was Campbell Bannerman [Bertrand Russell, *Logic and Knowledge: Essays 1901-1950*, ed. R. C. March (London: Allen and Unwin, 1956)] – will reveal more doubt with certainty.

counterexample to *p* although *S* may not know when it is safe to produce the counterexample. In fact, it will never be safe, because if knowledge is defined as limiting convergence, such a time will never arise. If the knowing agent has knowledge of *p* in the limit, then *p* is true and nothing will ever again provoke him to change his mind, even though he may not know when the modulus of convergence has arisen. Once the agent has limiting knowledge and is thereby locked on to the truth forever after in all relevant worlds it seems that the skeptic is left with nowhere to go but to succumb to knowledge! Skepticism is then defeated by the very nature of limiting convergent knowledge.

Again, this easy victory for the knower does not pay proper credit to the skeptical challenge. If knowers move to the limit and the skeptic cannot provide a counterexample, what the skeptic is in need of is not a strategy for showing that the knowers are wrong if they are right *pace* limiting convergent knowledge, but rather an assurance that he, the skeptic himself, can limiting converge to doubt. In this case, the putative knower would be no better off than the skeptic in the limit, and the skeptical challenge stands. The question then is whether the skeptic can converge to a proposition witnessing the impossibility of knowledge in the limit.

This proposition is the Socratic dictum of epistemic modesty embraced by Academic skeptics like Carneades and Archilaus:

> *All I know is that I don't know.*

Another skeptic, Sextus Empiricus, took the dictum of epistemic modesty to mean a universal generalization:

> The adherents of the New Academy, although they affirm that all things are non-apprehensible, yet differ from the Skeptics even, as seems probable, in respect of this very statement that all things are non-apprehensible (for they affirm this positively whereas the Skeptic regards it as possible that some things are apprehended).[13]

In response to the adherents of the New Academy, Sextus Empiricus launched the classical pre-Cantorian diagonal argument against inductive inference to disprove the coherence of the Academic position. By this argument he attacked the Academic skeptics by concluding that their position was just as dogmatic as Sextus' reading of Plato's conception of the actuality of knowledge:

---

[13] Sextus Empiricus, *Outlines of Pyrrhonism, Vol. 1*, trans. R. G. Bury (Cambridge: Harvard University Press, 1933), 139.

> For, when (the dogmatists) they propose to establish the universal from the particulars by means of induction, they will effect this by a review either of all or of some of the particular instances. But if they review some, the induction will be insecure, since some of the particulars omitted in the induction may contravene the universal; while if they are to review all, they will be toiling at the impossible, since the particulars are infinite and indefinite. Thus, on both grounds, as I think, the consequence is that induction is invalidated.[14]

Sextus' argument is based on the assumption that the Academics are supposed to converge to their doubt with certainty. In other words doubt in inquiry is equivalent to stabilizing to the correct answer – no knowledge possible, halt! Sextus takes his argument to undermine this possibility because of local underdetermination.

IV. Long-run doubt

In order to directly confront the skeptical challenge the skeptic must be permitted everything he needs. Hence, let the skeptic

- entertain a limiting convergence criterion

since in the short run he can do no better of proving himself right and the knowers wrong, and

- have use of the infallible method

which Sextus endorses, namely the method that never makes mistakes and only conjectures what is entailed by the evidence. In the limit, armed with the infallible method, the skeptic must prove his case by converging to doubt.

However, the skeptic may resist this way of articulating the conceptual situation. He may, for instance, suggest the possibility that inquiry may simply fail to converge. Failure to converge will take one of the following three forms: Quietism, oscillation or randomness. In queitism, the agent or method simply does not produce an output. No inquiry takes place. One example of the queitist strategy, as discussed above, is the method R. Academic skeptics advocated a queitist approach to inquiry. The relationship to inquiry here is problematic, for the familiar reason that, as soon as the skeptic asserts anything, he is abandoning his queitism and engaging in inquiry.

[14] Sextus, *Outlines*, 283.

In the case of a method or agent which does not converge due to the production of an output that osscillates infinitely often between truth and falsity, the skeptic will be faced with accepting a scenario in which knowledge is not always subject to error, but which regularly produces true outputs.

The third, non-converging scenario is one in which the method or agent produces a random output. In this case, by definition, the method will produce true outputs. Therefore, the skeptic will not be able to appeal to the possibility of error at all points in the process of inquiry.

Given these three alternatives, the skeptic must accept that a non-converging line of inquiry does not license doubt. Doubt in the limit is not failure to converge. Therefore, the skeptic must accept the convergence condition for inquiry. At this stage, the skeptic is committed to the Socratic dictum of epistemic modesty. This is standardly rephrased as axiom 5 of epistemic logic:

$$\neg K_S p \rightarrow K_S \neg K_S p$$

This axiom says that if an agent does not know *p* then he knows that he does not know *p.* Since being in doubt about *p* implies not knowing *p*, by transitivity, being in doubt about *p* implies knowing that he does not know *p* for any arbitrary proposition *p*.

Here is the situation: The skeptic is entertaining the weakest convergence criterion together with the strongest method of infallibility, and the following theorem sets in:

V. Theorem

> If knowledge is defined as limiting convergence, and S is infallible, then $\neg K_S p \rightarrow K_S \neg K_S p$ is impossible to validate.[15]

The theorem demonstrates that if knowledge is defined as limiting convergence, then it contrapositively follows that if agent *S* has not converged, *S* accordingly does not now even in the limit, and the use of the infallible method (or any other method for that matter) makes no difference to this result. So the skeptic cannot, in the limit, converge to doubt, or more precisely cannot converge to axiom 5 which would witness the impossibility of knowledge.[16]

---

[15] Vincent F. Hendricks, *The Convergence of Scientific Knowledge: A View From the Limit* (Dordrecht: Springer, 2001), 212.

[16] For an elaboration of the technical details, refer to Hendricks, *The Convergence*, 212-215.

An immediate objection might be that the skeptic is required here to know that he does not know. Ignorance per se is not sufficient to make the case for skepticism, since in the limit mere doubt without knowledge of one's ignorance will not defeat knowledge. This is because in the limit the knower can do better than the skeptic, since the knower can come to *know that he knows* in the limit.

VI. Theorem

> If knowledge is defined as limiting convergence, then $Kp \rightarrow K_S K_S p$ is possible to validate.[17]

In the limit the knower can know the he knows – so the *KK*-thesis is valid. This may seem quite surprising. William James, for instance, dismisses the *KK*-thesis in the limit, claiming that one may not infallibly know when one has converged to the fact that one has converged to the correct answer. Contemporary computational epistemologists are of the same opinion:

> This does not entail that Ψ knows he knows the answer, since (as observed above) Ψ may lack any reason to believe that his hypotheses have begun to converge.[18]

So far, knowledge has been treated in light of the idea of limiting convergence, and yet limiting convergence is often cited as one of the primary reasons for not validating the *KK*-thesis. How is it possible to have the cake and eat it too?

First distinguish between two interpretations of the implicational epistemic axioms:

> **Synchronic Interpretation**: *An epistemic axiom is* **synchronic** *if the consequent obtains at the very same time the antecedent obtains.*

> **Diachronic Interpretation:** *An epistemic axiom is* **diachronic** *if the consequent either obtains later or would have obtained later than the antecedent.*

Most discussions of the *KK*-thesis (for and against) assume a synchronic interpretation. To date, there has not been a defense of the diachronic interpretation of the *KK*-thesis. However, as will be shown, a diachronic

[17] Hendricks, *The Convergence*, 205.
[18] Martin, Osherson, *Elements*, 13.

interpretation of *KK* is precisely what is needed in order to validate *KK* in the limit.

To have knowledge of a proposition is to have reached a modulus of convergence after which the method continues to project the conjecture over all later times and relevant possible worlds. For the purposes of this argument let a proposition p be the set of worlds in which p is true. The set of worlds making knowing-p true is a subset of the set of worlds that make p true. To have knowledge of knowledge of a proposition p is to reach a modulus of convergence *after* convergence to knowledge of p. This is because the set of worlds making knowledge of knowledge of p true is a subset of the set of worlds making knowledge of p true. Therefore knowledge of knowledge can only happen once knowledge of the proposition has obtained. Hence the inclusion order

$$[K_S K_S p] \subseteq [K_S p] \subseteq [p].$$

One has to opt for a diachronic interpretation of the *KK* thesis in order to validate it. This falls out naturally given a method that respects the inclusion order defined above. First the method converges to knowledge of p. Then, the method must determine whether there are worlds in which it is true that $K_S K_S p$ which are not included in the set of worlds associated with $K_S p$. In short, this dependence simultaneously ensures that the necessary ordering is not violated and motivates acceptance of the diachronic interpretation of *KK*.[19]

In the long run, skeptics cannot know of their doubt, but knowers can diachronically come to know of their knowledge, so in the limit knowers are much better off than skeptics, or rather, ignorance is always only a short-term assurance, if any assurance at all.

VII. In the end

Any objection that the skeptic might consider launching against the limiting convergence strategy will prove unsuccessful by virtue of the character of assertion and inquiry described above. Take for instance the claim that since there exists an epistemically cursed world, a possible world in which agents are always wrong and that therefore our beliefs are always subject to the possibility of error. The possible existence of such a state of affairs may be undeniable. The problem for the skeptic involves defending the claim that the actual world *is* the world of his conception. If he is to take the additional step of asserting that this identity

[19] For an elaboration of the technical details, refer to Hendricks, *The Convergence*, 253-260.

holds, then he needs to play the game of inquiry and knowledge seeking. Once he enters into the epistemological project, he is subject to the kind of limiting convergence arguments presented above.

How might knowledge in the limit be challenged by the conceivability of possible worlds in which agents are always wrong? Consider an agent whose knowledge has converged in the limit not knowing that the point at which he will no longer change his mind has already passed. In fact one could also imagine him conceiving the possibility that he is wrong or that he is an inhabitant of the epistemically cursed world. The agent has every right to ignore the skeptical possibility entirely, in spite of not knowing his entitlement. This agent is (by stipulation) locked on to an unwavering path. His philosophical intuitions might lead him to conceive that this is not the case, but his intuitions are irrelevant, they have no bearing on the fact that his beliefs have all the properties demanded of knowledge.

What then of the skeptic who simply denies that we have knowledge? Assertions of this kind about the current state of our knowledge are similarly irrelevant. In addition to the trivially self-defeating aspect of such assertions, it has been shown by the foregoing argument that this self-ascription simply cannot be correct even given the benefit of the doubt in the most generous of ways. The skeptic will not converge towards knowledge of the futility of inquiry, not even in the limit.

The issue for the skeptic involves demonstrating that knowers are operating with a bad method or that we are in an epistemically cursed world. Such demonstration involves a significant step beyond conceivability or intuition and immediately draws the skeptic into the kind of convergence situations described above.

Having knowledge then, is an objective property of agents that have converged in the limit. One could imagine an agent that has already converged doubting himself, or imagining ways that he could be wrong. That's just fine. His job at that point would be to continue inquiring by whatever reliable means are available to him. Of course, from a third-person perspective, he will simply continue down the same path that he was on before and will continue having the robust features of a knower, in spite of any skeptical worries that might afflict him.

LOTTERIES, KNOWLEDGE, AND PRACTICAL REASONING

Rhys MCKINNON

ABSTRACT: This paper addresses an argument offered by John Hawthorne against the propriety of an agent's using propositions she does not know as premises in practical reasoning. I will argue that there are a number of potential structural confounds in Hawthorne's use of his main example, a case of practical reasoning about a lottery. By drawing these confounds out more explicitly, we can get a better sense of how to make appropriate use of such examples in theorizing about norms, knowledge, and practical reasoning. I will conclude by suggesting a prescription for properly using lottery propositions to do the sort of work that Hawthorne wants from them.

KEYWORDS: lottery paradox, knowledge norm, practical reasoning

I. The Argument[1]

Suppose that Jane holds a ticket in a fair lottery with 10,000 tickets. She knows that the probability of her winning is a paltry 0.01%. She is offered a penny in exchange for her ticket, and reasons as follows:

> The ticket is a loser.
> So if I keep the ticket I will get nothing.
> But if I sell the ticket I will get a penny.
> So I'd better sell the ticket.

Hawthorne makes at least three comments about such reasoning.

> "It seems clear enough that such reasoning is unacceptable."[2]

> "It is clear that if one asks ordinary folk why such reasoning is unacceptable, they will respond by pointing out that the first premise was not known to be true."[3]

---

[1] John Hawthorne, *Knowledge and Lotteries* (Oxford: Oxford University Press, 2004), 29-30.
[2] Hawthorne, *Knowledge and Lotteries*, 29.

> "When we claim that no one can know that he or she will lose the lottery, part of what is going on is that we realize that no one is in a position, in advance of the lottery draw, to acceptably sell a lottery ticket for minimal return."[4]

In other words, Hawthorne argues not only that such reasoning is clearly unacceptable, but moreover that the source of the pathology is that the first premise is not known. He concludes that one ought not use premises in practical reasoning that one does not know.

Two observations about the interpretation and analysis of Hawthorne's argument seem particularly salient. First, we must be careful not to construct the example so that the *reasoning* is found defective on the basis of an attendant *decision's* being defective. Second, we must be careful to construct the example so that the pathology of the reasoning is best explained by whether the agent knows the premise(s) and not merely by appealing to decision-theoretic considerations.

II. Analysis

We must distinguish between Jane's conclusion to sell the ticket for a penny and her reasons for doing so. This is because there are potential confounds in the sources of the intuitions to which Hawthorne appeals. Part of one's intuition that Jane reasons irrationally might consist in one's regarding her *decision* to sell the ticket for a penny as irrational; in rejecting her conclusion, one may look for a fault in her reasoning. But this is not how Hawthorne wants the example to function. He does not want his argument to depend on the rejection of her reasoning based on whether her conclusion is sub-optimal. His position is that, irrespective of whether it's rational for Jane to sell her ticket for a penny, her *reasons* for doing so had better not be that the ticket is a loser (since she can't know this).

This is important because Jane's decision is *not* clearly unacceptable. One possible confound is that the propriety of her decision is partially a function of the expected value (EV) of her ticket.[5] The standard decision-theoretic norm for reasoning in lotteries and wagers begins with specifying the EV for one's ticket:

---

[3] Hawthorne, *Knowledge and Lotteries*, 29-30.

[4] Hawthorne, *Knowledge and Lotteries*, 178.

[5] Christopher Hill and Joshua Schechter give a thorough and detailed analysis of Hawthorne's argument but miss this other serious error. See their "Hawthorne's Lottery Puzzle and the Nature of Belief," *Philosophical Issues*, 17 (2007): 102-22.

the product of the probability of winning and the prize to be won.[6] If the EV in this case is below what Jane is offered for her ticket, then she should sell the ticket.[7] If the (solitary) first prize for her lottery were less than $100, then the EV of her ticket would be less than a penny. In such a case she should sell her ticket for a penny, if possible. So, contra Hawthorne's (3), Jane would be rationally justified in selling her ticket for 'minimal gain' in advance of information on the result of the lottery draw. It follows that Hawthorne's example is not such a clear case of unacceptable reasoning; it depends on details that are not specified in his treatment. In many circumstances, such a decision would be rationally appropriate.[8]

Furthermore, the potential pathology of Jane's reasoning is not clearly to be identified in whether she *knows* the first premise. If the prize structure for the lottery were specified, we would be in a better position to analyze the propriety of Jane's reasoning. For example, if the (solitary) first prize were greater than $100, then Jane would be engaging in poor reasoning in coming to sell her ticket for a penny. Elsewhere Hawthorne suggests that the lottery has the following structure: 10,000 tickets and a (solitary) first price of $5,000.[9] Here Jane's ticket has an EV of $0.50. If Jane were to sell her ticket for $0.01 she would be making a mistake since this is below the EV of her ticket. This raises the prospect that the intuitive problem with her selling the ticket is entirely a matter of her ticket's EV, given what she is offered in the sale. That is, what explains the pathology of her reasoning will sometimes be that she is selling her ticket for less than it is worth. No mention of whether she *knows* the premises is required for this analysis. Hawthorne holds that in this case there is a difference between being offered the chance to sell the ticket for a dime rather than a cent.[10] But since both are well

---

[6] Strictly speaking, for a single prize wager, EV = (Probability of Winning * Prize to be Won) – Cost of Wager. Since Jane already owns the ticket, we ignore the cost of the ticket in calculating the EV.

[7] And assuming that one's utility function is sufficiently linear and that one is risk neutral. Being risk neutral means that an agent equally prefers a *wager* with expected value w to the expected value of the wager (without having to take the risk of losing). These are not particularly good assumptions given research into how people *actually* behave; but, at least for the purposes of decision theory, these are standard assumptions for ideally rational agents. Furthermore, these assumptions are in play for the norms of decision theory, even though they may fail to describe actual agents.

[8] Most lotteries have a negative expected value: that's how the corporations running lotteries make money.

[9] Hawthorne, *Knowledge and Lotteries*, 85.

[10] Hawthorne, *Knowledge and Lotteries*, 85 n91.

below the EV of the ticket, both decisions are irrational. By Hawthorne's argument, if the reasoning in both cases depends on the premise that one's ticket will lose, then both are irrational decisions (on decision-theoretic grounds) and both use bad reasoning (because the premise is not known). The key point is that the case should be constructed so as to avoid confounds based on intuitions surrounding the *decision* to sell a lottery ticket prior to information on the draw. The focus must be on an agent's reasons.

One might think that Hawthorne (with Jason Stanley) has responded to this sort of objection.[11] Hawthorne and Stanley argue that decision theory doesn't properly account for our folk uses of 'know' in appraisals of practical reasoning. They argue for the *Action-Knowledge Principle* (AKP) and *Reason-Knowledge Principle* (RKP).

> AKP: Treat the proposition that *p* as a reason for acting only if you know that *p*.[12]

> RKP: Where one's choice is *p*-dependent, it is appropriate to treat the proposition that *p* as a reason for acting iff you know that *p*.[13]

Hawthorne and Stanley argue that AKP "straightforwardly accounts for the use of epistemic terms in appraisals of practical reasoning" and that the most natural explanation is by appealing to AKP as the norm of practical reasoning rather than the expected utility norms of decision theory.[14]

Of course, Hawthorne and Stanley's position is meant to be consistent with the norms of decision theory, but they take their account to be more complete. They argue that a complete theory of practical reasoning must integrate an EV-based account with a reasons-based account, in order to accommodate cases in which people perform decision-theoretically correct actions for the wrong reasons. For example, suppose that in deciding whether to hire one of two candidates, a hiring committee chooses the stronger candidate – but only because her name starts with a B rather than a C. The right decision was made (it

---

[11] John Hawthorne and Jason Stanley, "Knowledge and Action," *Journal of Philosophy* 105, 10 (2008), 571-590.

[12] Hawthorne and Stanley, "Knowledge and Action," 577.

[13] Hawthorne and Stanley, "Knowledge and Action," 578. An option is *p*-dependent iff the most preferable option on supposition of *p* is not the same as the most preferable option on the supposition of ¬*p*.

[14] Hawthorne and Stanley, "Knowledge and Action," 578. I prefer using EV to *expected utility* or EU; but these terms are interchangeable for current purposes.

maximizes the firm's EV) but the reason (other things being equal) was terrible. Hawthorne and Stanley argue that decision theory can only evaluate a decision based on EV considerations and is not able to account for our intuitions about making the right decision for bad reasons. So they argue that in the lottery case Jane may be coming to the right decision (to sell her ticket) if the EV of her ticket is less than what she is offered in the sale, but she's doing it for poor reasons (because she does not know that her ticket *will* lose).[15]

Yet even if we grant Hawthorne and Stanley's criticism of decision theory as being inadequate for a complete theory of practical reasoning, this does not count against my argument that the analysis of the lottery case (at least as Hawthorne represents it) is confounded by decision-theoretic considerations. That Jane's decision is a poor one is adequately explained by the observation that she ought not to sell the ticket *because* she's selling it for less than it's worth. If Jane's ticket had an EV of $0.001, however, she would be making the right decision to sell her ticket. In neither case would we have to appeal to whether she knew the propositions that she uses in her practical reasoning.

Conclusion

I have argued that Hawthorne's example of pathological practical reasoning is defective in at least two respects. First, there are possible confounds producing the intuition that such reasoning is defective, besides the intended interpretation that the problem consists in reliance on a premise that is not known. One of these confounds is that the perception that the *decision* is irrational may determine the rejection of the reasoning on which it was based. However, it is not clear that the conclusion of the argument is irrational. In some circumstances it may count as good reasoning. And second, when relevant details are added to make the example pathological, the problem with the reasoning is not necessarily that the agent lacks knowledge of a premise. Hawthorne's purposes require a lottery case in which the *best* explanation is that the agent does not know the premise that the ticket will lose. That is, the case should not admit of a better explanation by merely appealing to decision-theoretic considerations. The problem is that Hawthorne (and Stanley) continues to feature the original problematic version of the lottery case as a

[15] On such an account the decision to sell a ticket would be correct if the EV of the ticket is less than what is being offered in a sale *and* the agent's reasons for selling the ticket are that the EV is less than what is being offered in the sale. The reasons could not include propositions which the agent does not know (such as that the ticket is a loser).

paradigm example of pathological practical reasoning (involving an agent using premises that are not known).

Does this mean that Hawthorne's argument is scuttled? That conclusion would be too strong. In fact, Hawthorne and Stanley mention a much better case but don't feature it as their paradigm case.[16] Suppose that Jane holds a ticket in a lottery with 10,000 tickets. The solitary first prize is $20,000. She knows that the EV of her ticket is $2. She is offered the opportunity to sell her ticket for $5. On decision-theoretic grounds she should sell her ticket.[17] However, prior to information on the draw she had better not reason as follows:

> The ticket is a loser.
> So if I keep the ticket I will get nothing.
> But if I sell the ticket I will get $5.
> So I'd better sell the ticket.

Although she comes to the correct decision-theoretic conclusion, she does so for bad reasons. Why are her reasons bad? Hawthorne (and Stanley) argue that she does not know the first premise. This is a much better example of using the selling of a lottery ticket before information of the draw as a case of pathological practical reasoning than Hawthorne's original case. The latter case avoids the confounds present in the former.

Ideally, the scope of these remarks will extend beyond simply analyzing a single case of reasoning about a lottery ticket. More generally, I hope to motivate the idea that we must make explicit what aspect of practical reasoning is to be the object of evaluation when we argue from cases: the decision, or the reasons for making a decision. If only the decision is the proper object of evaluation, then it is plausible that only decision-theoretic considerations are required for the analysis, and no appeal to whether premises are known is necessary. But if an agent's reasoning in practical decision making is a proper object of our interests, then illustrative cases ought to be constructed so as to indicate that decision-theoretic considerations alone are insufficient to capture our intuitions. Abiding by this principle in the case at hand – by constructing it so that the proposed sale would be for *more* than the ticket's EV – would remove the confound that poor reasoning in lottery cases is better explained by decision-theoretic considerations alone. Furthermore, the case should be constructed so as to not be confounded by

[16] Hawthorne and Stanley, "Knowledge and Action," 575.

[17] We'd specify that we are not concerned with endowment effects, nonlinear utility functions, loss aversion, transaction costs, and so on.

intuitions surrounding rejecting an agent's reasons based on the impropriety of the attendant decision. The original lottery case suffers this confound but the latter case does not. Consequently, Hawthorne (and Stanley) should cease featuring the original case as the central example of poor practical reasoning.[18]

[18] I would like to express my thanks to Tim Kenyon, John Turri, Dave DeVidi, and Mathieu Doucet for their help on various versions of this paper.

WHAT EINSTEIN WANTED

Nicholas RESCHER

ABSTRACT: Einstein envisioned a clear difference between a bottom-up physics that moves from observations to the conjecture of explanatory generalizations, and a top-down physics that deploys intuitively natural principles (especially of economy and elegance) to explain the observations. Einstein's doubts regarding standard quantum mechanics thus did not simply lie in this theory's use of probabilities. Rather, what he objected to was their status as merely phenomenological quantities configured to accommodate observation, and thereby lacking any basis of derivation from considerations of general principle.

KEYWORDS: quantum theory, probability, phenomena, principles, rationalization.

I. Einstein's Discontent

Albert Einstein always thought that the quantum theory in its then-standard formulation offered no more than a phenomenological observation-descriptive account of reality, bereft of any grounding rationale on the basis of fundamental principles.[1] And Einstein disdained as scientifically insufficient and inadequate any theory which (as one recent expositor puts it) "owes its original to [mere] 'facts of experience'… [since] however compelling these may be, physicists then still did not have a 'general theoretic basis' capable of providing a logical foundation for the phenomenology at issue."[2] Einstein was intent upon explanatory understanding and therefore steadfastly rejected any observationality

[1] See Jeroen van Dongen, *Einstein's Unification* (Cambridge: Cambridge University Press, 2010), 177-78. It may be of incidental interest that the writer can himself claim a somewhat curious family connection with Einstein. For there once lived in Adingen, in the Neckar valley in the Swabian region of Germany, one Salomon Pappenheimer (1794-ca. 1870) – a merchant and the richest man in town. He married three times. His first wife died in childbirth. His second wife was Sarah Rescher (1805-1834) of my family, who died enroute to a visit in North America when her ship foundered in a storm. He thereupon married his third and last wife. She was Margot Einstein (1806-1868) of the family of the great Albert.

[2] Van Dongen, *Einstein's Unification*, 125.

bottom-up, merely phenomenological empiricism. As that just-cited expositor puts it:

> He remained convinced that his program – his top-down approach, based on maxims of simplicity and naturalness … was a promising alternative that in the end would carry the day.[3]

On this basis Einstein was deeply discontent with quantum theory in its Bohr/Copenhagen version by having what has been characterized as "a methodological discomfort with the nature of its recourse to probabilities."[4] He resisted the introduction of underived probabilities into quantum physics because – as he himself put it – "I still believe in the possibility of a model of reality – that is to say, of a theory which represents things themselves and not merely the probability of their occurrence."[5] He thus viewed the probabilistic description of quantum phenomena as not so much incorrect as incomplete because in admitting probabilities as basic given facts in physics, quantum theory failed to do justice to reality's descriptive definiteness of condition.

As Einstein affirmed in a 1929 address:

> I admire in the highest degree the achievement of the younger generation of physicists which goes by the name of quantum mechanics, and I believe in the deep level of truth of that theory, but I believe that its restriction to statistical laws will be a passing one.[6]

Maintaining that "God does not play dice with his universe," Einstein insisted that probabilities ought not to be introduced into physical theory as underived givens – or, perhaps better, *takens* – but should be accounted for in nonprobabilistic terms. Accordingly, Einstein told Peter Bergmann in 1949 that "I am convinced that the probability concept must not be introduced into the description of physical reality as primary [i.e. without derivation from plausible nonprobabilistic conditions]."[7] As he saw it, probabilities as such are never basic, and his battle-cry was: *ad*

---

[3] Van Dongen, *Einstein's Unification*, 174.

[4] Van Dongen, *Einstein's Unification*, 177-78.

[5] Cited in Van Dongen, *Einstein's Unification*, 177.

[6] Alice Calaprice (ed.), *The Expanded Quotable Einstein* (Princeton: Princeton University Press, 2000), 246. The original edition (drop "Expanded") appeared in 1996 and a later revision (change "Expanded" to "New") in 2005.

[7] See van Dongen, *Einstein's Unification*, 154-55.

probabilitatem esse deducendam: probability always is – or should be – something derivative. But how derivative, and deducible from what?

To answer this question it is – strange to say! – expedient and instructive to go back to the very origin of modern physics in the 17th century, albeit not to Newton but to Leibniz.

II. The Leibnizian Project

Leibniz regarded physics as an applied mathematics – or perhaps better, an enriched mathematics – one that is enlivened by its enmeshment with matters of existence in the real world. He writes: "There is nothing which is not subordinate to number; Number is thus like a metaphysical figure (*numerus quasi figura metaphysica est*) and arithmetic is a kind of statics of the universe by which the powers of things are discovered."[8] And as Leibniz saw it, the mathematicizing of nature is subject to certain basic principles. Nature has a vast host of problems to solve in the determination of her *modus operandi*. And this determination will have to align with an array of fundamental parameters of rational merit as encapsulated in certain basic principles of rational systematization.[9] Like Einstein long after him, Leibniz envisioned a rational universe.

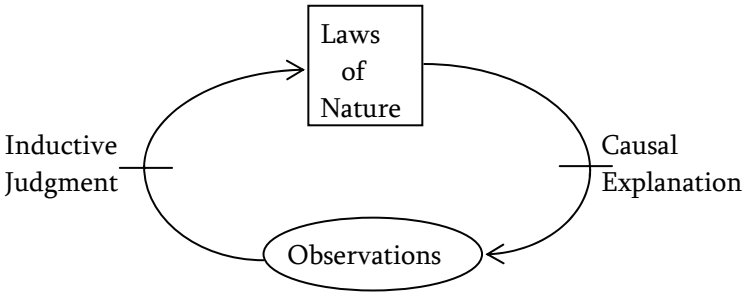
The Leibnizian program in physics accordingly sought to dig through to a stratum deeper than that of the Newtonian synthesis. For Newton's own program in physics was essentially that of the ancient Greek mechanicians and astronomers. With Archimedes and Ptolemy, it asks "What laws of nature can we stipulate to 'save the phenomena' by providing an adequate accounting for why

[8] GP VII 184. Citations in this style refer to C. I. Gerhardt, ed., *Die philosophischen Schriften von G. W. Leibniz*, 7 vol.'s (Berlin: Wiedmann, 1875-90).

[9] The principal secondary sources bearing upon Leibniz's physics include: Martial Gueroult, *Dynamique et métaphysique leibniziennes* (Paris: Les Belles Lettres, 1934); George Gale "The Physical Theory of Leibniz," *Studia Leibnitiana* 2 (1970): 114-127; Diogenes Allen, "Mechanical Explanations and the Ultimate Origin of the Universe Accordingly to Leibniz," *Studia Leibnitiana*, Sonderheft 11 (Wiesbaden: Franz Steiner, 1983); Hans Poser, "Apriorismus der Prinzipien und Kontingenz der Naturgesetze: Das Leibniz-Paradigma der Naturwissenschaft," in *Leibniz' Dynamica*, ed. Albert Heinekamp (Stuttgart: Franz Steiner, 1984; *Studia Leibnitiana* Sonderheft 13), 164-79; Herbert Breger, "Symmetry in Leibnizian Physics," in *The Leibniz Renaissance* (Firenze: Leo S. Olschki, 1989), 23-42; and Francois Duschesneau, *Leibniz et la méthode de la science* (Paris: Presses Universitaires de France, 1993).

our observations are as they are?" And it addresses this question as per the pattern of Display 1.[10]

Display 1
CLASSICAL SCIENTIFIC SYSTEMATIZATION



There is an elegant equilibrium here. The phenomena instantiate and illustrate the operation of the laws, the laws determine and account for the phenomena. And in this neat arrangement there is both ontological and epistemological closure. However, Leibniz sought to go even further, taking a more ambitious line, one which in effect says: "Fine. Let's give this program our efforts. But let us then suppose we are successful in getting a grasp on nature's laws. Then there still remains the question: "Now viewing these laws themselves as our 'phenomena' how can we best 'save' *them* – how can we account for these laws themselves?"

And so even as standard physics studies nature's phenomena via observation and experimentation to discern the laws governing nature's phenomenal modus operandi, so Leibnizian physics studies nature's laws in thought-experimental deliberation to discern the "archi-tectonic" principles of rational economy and factual efficacy governing nature's lawful modus operandi. As Leibniz himself put it:

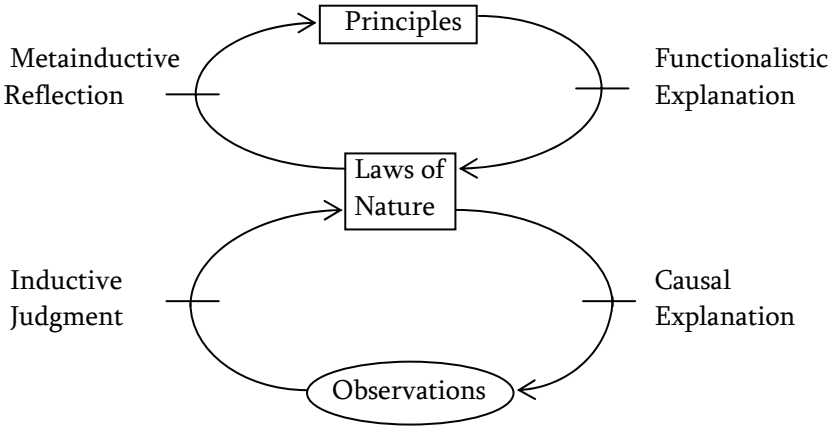
> We can see the wonderful way in which metaphysical laws of cause, power, and action are present throughout all nature and how they pre dominate over the

[10] The given schematic enfolds, sight unseen, the crucial stage of applicative testing of the laws leading either to confirmation or replacement/revision.

> purely geometric laws of matter themselves, as I found to my astonishment (*admiration*) when I was explaining the laws of motion.[11]

And so as Display 2 shows, Leibnizian physics augments classical physics by superimposing upon it an added cycle of systematization consisting in a meta-inductive step to a set of explanatory principles that make it possible to account for the laws of nature. Even as classical physics seeks to 'save the phenomena' by addressing the question of why the observations are as they are, so Leibnizian

Display 2
LEIBNIZIAN SCIENTIFIC SYSTEMATIZATION



physics seeks to provide a scientifically cogent and rationally plausible answer to the question of why the laws of nature are as they are.[12]

---

[11] GP VII 305. Trans. in L. E. Loemker, ed., *Leibniz: Philosophical Papers and Letters*, 2 vol.'s (Chicago: University of Chicago Press, 1956); 2nd edition in one volume (Amsterdam: Reidel, 1970), 488-89. (Henceforth cited as simply Loemker.)

[12] Leibniz himself then took the further step of adding yet another cycle of systematization that proceeds in theological terms to provide a rational explanation of the explanatory principles themselves. As he put it in a 1679 letter to Christian Philip:

> For my part I believe that the laws of mechanics which serve as foundation for the whole system [of physics] depend upon final causes, that is to say, on the will of God determined to do what is most perfect ... (GP IV 281-82 (Loemker 273).)

As Leibniz saw it, the world exists as is because God has chosen to create it that way. And God has so chosen it because that particular world design is optimal. Now here one could, in theory eliminate the middle man and move directly from optimality to existence. In a post-Kantian, not to say post-Nietzschean world, such a sidelining the deity may have a certain

As Leibniz saw it, such principles of rational design as those of continuity, of conservation, and of least effort can both guide our researches into nature's laws and provide a framework for understanding and explaining the results of our investigations: they both serve to explain nature's mode of operation and provides evidential quality-control for our investigative hypotheses. It was on this basis that Leibniz said such things as:

> All natural phenomena could be explained mechanically [i.e., scientifically] if we understood them well enough, but the principles of mechanics themselves cannot be so explained … since they depend on more substantive [i.e. deeper] principles. (*Tentamen anagogicum*, GP VII 271 (Loemker 478).)

Leibnizian physics is thus a two-tier affair. It sees the world's phenomena as explicable by the laws of nature, but has it that these laws themselves are to be explained with reference to fundamental principles of rational coherence. As Leibniz himself put it:

> All the particular phenomena in nature could be explained mechanically if we were capable enough… But I hold, nevertheless, that we must also consider how these mechanical principles and general laws of nature themselves arise from higher principles and cannot be explained by quantitative and geometrical considerations alone.[13]

What Leibniz ardently wanted was a functional account showing the physical laws of nature as we have them to be the optimal means of satisfying basic principle of operational economy – and so for classic physics to be exhibited as the best solution of a problem of rational design.

Considerations of rational intelligibility ('sufficient reason') – broadly understood to encompass such factors of rational economy at large, conservation, and symmetry [e.g., of action and reaction) – provide the driving impetus of Leibnizian physics. And here, as Leibniz saw it, the prime principles are those listed in Display 3. For what Leibniz emphasized in his physics was not just the lawfulness of nature, but the lawfulness of nature's laws – their systemic

---

appeal. But this view of the matter just was not Leibniz's – no matter how insistently Bertrand Russell thought it should have been. To be sure, a purely naturalistic Leibnizianism would of course refrain from taking this further, theological step, but for Leibniz himself it was crucial. In any case, for Einstein's version of exactly this selfsame picture see van Dongen, *Einstein's Unification*, 51-57.

[13] GP IV 391 (Loemker 409).

harmonization within a systemic order as geared to principles of rational intelligibility. The salient and characteristic goal of Leibnizian physics is accordingly oriented to the discovery of deeper physical – or, rather, *metaphysical* – principles for grounding Nature's laws. Its key aim is not just the *discovery* of laws via *phenomena* but preeminently the *explanation* of laws via *principles*. And he set out to deploy such eco nomic and aesthetic principles to account for the explanation of laws. For in Leibnizian physics, the situation is that, first, the laws as best we can discover them be used as a launching-platform for discerning the appropriate principles, and thereupon that these principles can and should be deployed to explain how and why it is that those laws are what they are.[14] To be sure, there is circularity here, but it is supportive and substantive, and not vicious.

Display 3
LEIBNIZIAN PRINCIPLES

- *Fertility* (variety, abundance, diversity, complexity)[15]
- *Economy and Simplicity* (least effort, ease of operation, greatest efficiency, least time, least action)
- *Continuity* (gaplessness, amplitude)
- *Definiteness* (specificity, precision, mini-max determinacy)
- *Uniformity* (regularity)
- *Consonance* (simplicity, uniformity, consistency, regularity)
- *Conservation* (equivalence of action and reaction and generally of the *causa plena* and *effectus integer*)[16]
- *Elegance* (symmetry, harmony, balance)

Accordingly, Leibniz has it that:

> Although all the particular phenomena of nature can be explained mathematically or mechanically… it becomes increasingly apparent that

[14] Although Leibniz holds that it lies in our power to see how the fundamental principles of natural philosophy can, at least in principle, account for the laws of physics, it is beyond our power to see how they account for nature's particular detail. This insight is reserved for God alone. On this issue see Hans Poser, "Apriorismus."

[15] The duly balanced combination of all of these factors is what Leibniz calls *harmony*, which is for him, the hallmark of perfection.

[16] On this principle see especially Leibniz's letter to de l'Hôpital of 15 January 1696 (GM II 308).

> nevertheless the general principles of corporeal nature and of mechanics are themselves of metaphysical rather than merely geometrical form.[17]

The insistence not just on the lawfulness of nature but on the higher-order lawfulness of nature's laws is the hallmark of Leibnizian physics.

Leibniz insisted that the natural world is designed to function efficiently and economically, and for this reason its investigation must proceed on the principle that "the best hypothesis is that which plans the most phenomena in the simplest way."[18] Rational economy lies at the core: even sufficient reason has its economic dimension. (Why have something be so if one can losslessly dispense with it – i.e., if there is no good reason for its being so?) Leibniz thus envisioned such principles as formative constraints on the laws of nature. For they are not merely or only matters of mathematical elegance but manifest the pressure of rational economy on nature's modus operandi. Given this gearing to the modus operandi of intelligence, the metaphysics of optimality and the epistemics of rational intelligibility stand coordinate with one another in Leibniz's thought.[19]

---

[17] "Discourse of Metaphysics," §18; GP IV 444 (Loemker 315).

[18] See the preface to Leibniz's edition of *Nizolius* and compare Massimo Mugnai, *Introduzione alla filosofia di Leibniz* (Torino: G. Einaudi, 2001), esp. 152-63.

[19] Many thoughtful people have over the years taken much the same line. Thus in addressing a university convocation in the late 1800's Joshua L. Chamberlain (Civil War hero, Governor of Maine, and Bowdoin University president) said:

Sooner or later … they [our men of science] will see and confess that these laws along whose line they are following, are not forces, are not principles. They are only methods … Laws cannot rightly be comprehended except in the light of principles … Laws show how only *certain* [limited] ends are to be reached; it is by insight into Principles that we discover the great, the integral ends … Now the *knowledge of these Laws* I would call *Science* but the *apprehension of Principles* I would call *Philosophy*, and our men of science may be quite right in their science and altogether wrong in their philosophy. (Quoted in W. M. Wallace, *Soul of the Lion: A Biography of General Joshua L. Chamberlain* (Edinburgh: Thomas Nelson & Sons, 1960), 232-33.)

The perspective at issue here is in much the same spirit as the more profoundly developed ideas of Leibniz, who did, however, see Principles as still belonging to *natural* philosophy and thus to science itself.

III. Leibniz's Implementation of his Program

The Mechanics of Rebound

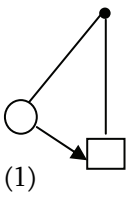
Consider the issue of ball-bouncing in mechanics. And let us start with a billiard-table cushion here. Nature faces the following problem:

> To propel a ball from point X to point Z by bouncing it off the cushion. Which path is Nature to choose? What impact-point Y is to be appropriate here?

The most "convenient" path is of course the shortest – which is also the fastest when the ball moves at a constant velocity. And it is exactly this path – the one which, as it were, maximized the economy of effort that Nature in fact chooses, with its characteristic feature that the angle of incidence equals the angle of rebound.

Again, let it be that a suspended moving elastic object meets a suspended standing one as per the diagram of Display 4. First let it be that the moving object (1) has greater mass then the standing one (2). Then on Cartesian principles (i) they will both move in the direction of the heavier, and (ii) if the moving object has less mass then the standing one, then the later will remain in place while the former bounces back in the direction from which it came. But there are problems here.

Display 4
IMPACT INTERACTION



For so reasons Leibniz, if the difference in masses be only a minuscule amount ($\in$) in object (1)'s favor then the motion of object (1) after impact will be $\rightarrow$, but if object (2) is even minimally the more massive object (1)'s motion after impact will be $\leftarrow$. An infinitesimal difference in input will have a substantial difference in result. This violates Leibniz's principle of continuity thereby also violates simplicity in specifying a significantly different modus operandi in

fundamentally analogous cases. For Leibniz the Principle of Continuity provided for a uniformity of result that insists on the same outcome coming from different directions of approach. Accordingly, this principle was the Archimedean fulcrum that he used to dislodge the principles of Cartesian physics.[20]

The Optics of Reflection and Refraction

As Leibniz saw it, the principle of processual efficiency also governs laws that describe the motion of light. He put the matter as follows in his *Discourse on Metaphysics*:

> Snell, who first discovered the rules of refraction, would have waited a long time before discovering them if he first had to find out how light is formed. But he apparently followed the method which the ancients used for catoptrics, which is in fact that of final causes … For when, in the same media, rays observe the same proportion between sines (which is proportional to the resistances of the media), this happens to be the easiest or, at least, the most determinate way to pass from a given point in a medium to a given point in another. And the demonstration Descartes attempted to give of this same theorem by way of efficient causes is not nearly as good.[21]

And the same efficiency principle of time minimization obtains in refraction when rays of light travel from one medium into another – say from air to water. Here nature's modus operandi obeys 'Snell's Law' which proportions the angles of reflection resistance and refraction to the density of the medium at issue, a relationship that once again maximizes efficacy by minimizing transit time.[22]

Leibniz ardently espoused this extremal, efficiency-oriented perspective, and he reproached Descartes with having used (in accordance with the Cartesian program) a more clumsy mechanical method in the derivation of Snell's law, instead of the more elegant *a priori* principle of least time or distance.[23] As he saw it, those Newtonian process-descriptive phenomenological laws of physics are to be derived from deeper, rationally cogent principles.

But how did this work out? Here some historical background is relevant.

---

[20] See in particular his *Critical Thoughts on the "Principles" of Descartes*, GP IV 354-92, esp. 375 (Loemker 397-98).

[21] "Discourse of Metaphysics," §22, G IV 447-48 (Loemker 317-18).

[22] The law in question was stated by Willebrord Snell in 1621.

[23] "*Tentamen anagogicum*," GP VII 274 (Loemker 478).

IV. The Leibnizian Heritage: Rational Mechanics

In the 1740s P.L.M. de Maupertuis enunciated the principle of least action and used it to ground Fermat's principle and derive Snell's law in optics. His discussion was soon extended and generalized by Leonard Euler, who thereupon represented the principle as fundamental and applicable to all physical systems and not merely to light. In 1751 Maupertuis' claims to priority were challenged by J. S. Koenig who cited a 1707 letter from Leibniz to – describing results tantamount to those in Euler's 1744 paper. This publication created an intense priority dispute. Maupertuis and his supporters demanded that Koenig produce the original of the Leibniz letter, and when Koenig could only produce copies of this and related letters there was a sharp reaction. As president of the Berlin Academy, Euler himself accused Koenig of forgery. And the Academy declared the letter spurious and sustained Maupertuis' claim to priority for the principle of least action. Koenig however, continued to defend Leibniz's claim and various eminent figures – including Voltaire and Frederic II of Prussia – took sides in the quarrel, the former defending Koenig and the latter Maupertuis. The matter stood on an indecisive footing for some 150 years until it was settled by modern Leibniz scholars who discovered contemporary copies of those Leibniz letters cited by Koenig in various archives.[24]

Leibniz's vision of a physics based on principles certainly found traction. The value of the principle of least action lies in its unifying effect; it provides a basis for the axiomatic development of large sections of physical theory. Here Leibniz's insights were extended by Maupertuis, and in Lagrange's *Méchanique analytique* the principle of least action was shown to be a sufficient basis for the deduction of the laws of mechanics, and the work of Hamilton extended this result to optics and dynamics. Some idea of the power of this principle can be gained from the following except from a paper in which Hamilton presented his results on optics to the Royal Irish Academy in 1824:

> Those who have meditated on the beauty and utility, in theoretical mechanics, of the general method of Lagrange, who have felt the power and dignity of that central dynamical theorem which he deduced in the *Méchanique analytique* …, must feel that mathematical optics can only then attain a coordinate rank with mathematical mechanics…, when it shall possess an appropriate method and become the unfolding of a central ide… It appears that if a general method in

[24] On the historical issues see Philip E. B. Jourdain, *The Principle of Least Action* (Chicago: Carus, 1913). See also Carnetius Lanczos, *The Variational Principle of Mechanics* (New York: Dover, 1986).

> deductive optics can be attained at all, it must follow from some law of principle, itself of highest generality, and among the highest results of induction…, (This) must be the principle, or law, called usually the Law of Least Action.[25]

In the hands of the great masters of classical mathematical physics – Euler, Lagrange, Laplace, Gauss, and Hamilton – the Principle of Least Action became the mainstay of rational mechanics. And the work of Gibbs and Mach further amplified its role.[26] But from the very outset, Leibniz had already envisioned its significance and that of the general minimax principle from which it derived. However, as the 19th century moved along, other ideas and other paradigms came into prominence and by its end principles like minimax, economy, simplicity, and least action were not greatly in vogue.

Moreover, a surprising revival has transpired in the later years of the 20th century. Various capable scientists have found their way back into a Leibnizian state of mind. Simplicity, fertility, and lawful order are back in vogue. Einstein wrote that "experience justifies one belief that nature is the realization of the simplest mathematical ideas that are reasonable."[27] The astronomer Mario Livio proposes a "cosmological aesthetic principle" encompassing such functions as simplicity, symmetry, continuity. The physicist Anthony Zee has the universe continuing in creative terms such functions as "unity and diversity, absolute perfection and boisterous dynamism, symmetry and lack of regularity."[28] And the physicist Freeman Dyson maintains that nature's simple laws appear to be designed to "make the universe as interesting as possible."[29] Cosmologists Julian Barbour and Lee Smolin see the universe as exhibiting order amidst "extremal

---

[25] Quoted from the article "Light," *Encyclopedia Britannica*, eleventh edition.

[26] For an overview of this historical development see Ernst Mach, *Die Mechanik in ihrer Entwicklung* (Leipzig: Brockhaus, 1901), and also Jourdain, *The Principle of Least Action*.

[27] Quoted in Mario Livio, *The Accelerating Universe* (New York: John Wilem, 2000), 34. Einstein speculates that considerations of simplicity alone may determine the laws of nature: "What really intrigues me is whether God could have created the world any differently; in other words; whether the demand for logical simplicity leaves any freedom at all." Calaprice, *The Expanded Quotable Einstein*, 221.

[28] Anthony Zee, *Fearful Symmetry* (Princeton: Princeton University Press, 1999), 211.

[29] Quoted in John Horgan, *Rational Mysticism: Spirituality Meets Science in the Search for Enlightenment* (New York: Haughton Mifflin, 2003), 172.

variety."[30] The idea of a physical domain subject to the rational efficacy and economy at work in Leibnizian physics is still alive and stirring.[31]

V. Einstein's Penchant for Principles

But let us now return to Einstein. The Leibnizian distinction between descriptively empirical phenomenological laws and the underlying rational principles that they instantiate and implement actually played a key role in Einstein's thought. In his oft-cited London *Times* note of 28 November 1919 he discussed the epistemology of physical theories and emphasized the distinction between "constructive" or merely empirical theories based only on observation and "principle theories" that have a cogent rationale for being as is. And he insisted that those principle theories "have greater logical perfection and security in their foundations."[32] And Einstein went on to maintain:

> My interest in science was always essentially limited to the study of principles … That I have published so little is due to this very circumstance, as the need to grasp principles has caused me to spend most of my time on fruitless pursuits.[33]

Writing to Paul Ehrenfest in 1925, Einstein described himself as a "principle-fanatic" (*Prinzipienfuchser*").[34] Einstein's principles went beyond anything that constitutes a physical "law" as ordinarily construes (i.e., as a mathematical relationship between physical parameters of some sort, like $F = ma$ or action = reaction). As early as 1919 he wrote:

> Along with this most important class of theories there exists a second, which I will call "principle-theories." These employ the analytic, not the synthetic, method. The elements which form their basis and starting-point are not hypothetically constructed but empirically discovered ones, general characteristics of natural processes, principles that give rise to mathematically formulated criteria which the separate processes or the theoretical representations of them have to satisfy. Thus the science of thermodynamics seeks by analytical means to deduce necessary conditions, which separate events have to satisfy, from the universally experienced fact that perpetual motion is impossible.

---

[30] Julian Barbour and Lee Smolin, "Extremal Variety as the Foundation of a Cosmological Quantum Theory," published on the web at http: arxiv.org/hep-th/9203041.

[31] I owe some of these references to William C. Lane.

[32] Cited in van Dongen, *Einstein's Unification*, 50.

[33] To Maurice Solomon in 1924. See Calaprice, *The Expanded Quotable Einstein*, 245.

[34] See van Dongen, *Einstein's Unification*, 162-63.

Among the fundamental principles of physics at work in Einstein's thought were "simplicity" and the "economy" of process which harked back to Ernest Mach's conception of physics, and beyond him to the tradition of rational mechanics.[35] As he saw it, "nature is the realization of the simplest conceivable mathematical ideas [that serve for an explanation of certain fundamental facts]."[36] And he accordingly affirmed that:

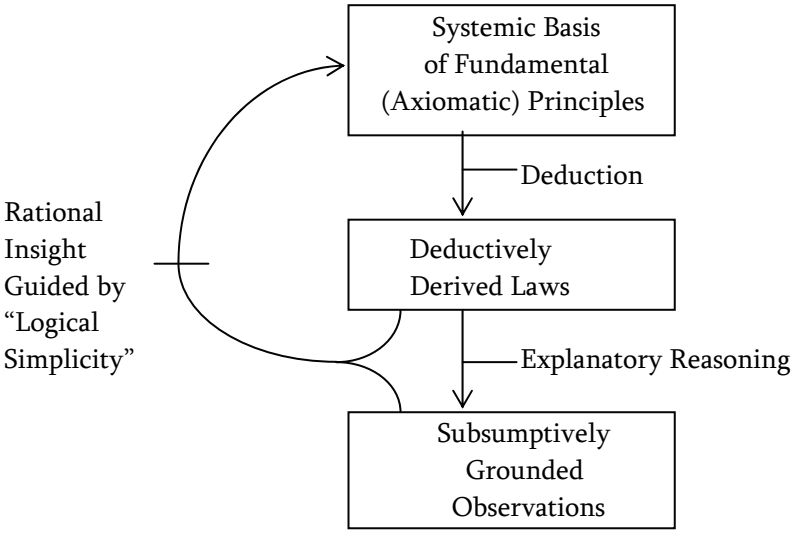
> I believe that [nature's] laws are *logically simple* [his italics] and that trust in this logical simplicity is our best guide, so that it suffices to proceed from only a few empirical data. If nature were not arranged correspondingly to this belief, then we would have no hope at all of achieving any deeper understanding.[37]

In conversations with Valentin Bargmann, Einstein repeatedly insisted that his efforts in unified field theory were attempts to find the simplest theory in a given class.[38] Rational economy via what Einstein himself termed the "logical simplicity" of theories was his guiding star. In his 1933 Herbert Spencer lecture at Oxford, "On the Methods of Theoretical Physics," Einstein declared "Our experience hitherto justified us in believing that nature is the realization of the simplest conceivable mathematical ideas. I am convinced that we can discover by means of purely mathematical construction the concepts and laws connecting them with each other which furnish the key to the understanding of natural phenomenon."[39]

---

[35] See Einstein's "On Generalized Theory of Gravitation," *Scientific American* 182 (1950): 13-17.
[36] Van Dongen, *Einstein's Unification*, 52. Compare the ampler discussion of this passage in John Norton "Nature is the Realization of the Simplest Conceivable Mathematical Ideas: Einstein and the Canon of Mathematical Simplicity," in *Studies in the History and Philosophy of Modern Physics* 31 (2000): 135-70; see esp. 136-37.
[37] Letter to Bohm of 24 November 1954. See Van Dongen, *Einstein's Unification*, 181-82.
[38] See Van Dongen, *Einstein's Unification*, 147.
[39] A. Einstein, *Ideas and Opinions* (New York: Bonanza, 1954), 270-76. On the relevant issues see John D. Norton, "Nature is the Realization of the Simplest Conceivable Mathematical Ideas: Einstein and the Canon of Mathematical Simplicity," *Studies in the History and Philosophy of Modern Physics* 31 (2000): 135-70.

Display 5
EINSTEINEAN SCIENTIFIC SYSTEMATIZATION



NOTE: The diagram is adapted from Einstein's own 1952 depiction as presented and analyzed in Van Dongen, *Einstein's Unification*, 51-55.

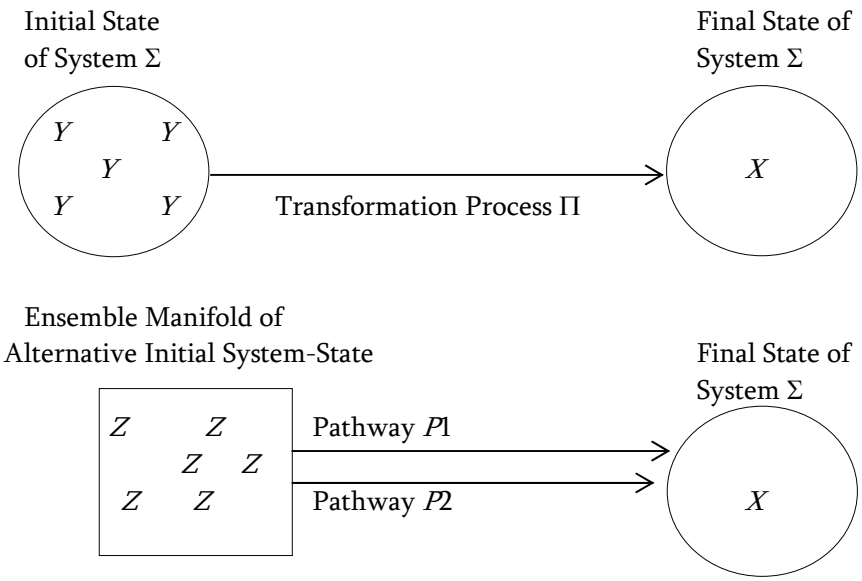
In a 1952 letter to Maurice Solovine, Einstein gave a diagrammatic sketch of the methodology he advocated for the physical sciences, which in its structure is closely analogous to the Leibnizian systematization of Display 2. (See Display 5). The fundamental kinship between Einstein's vision of the methodology of systematization in physical science and that of Leibniz becomes readily apparent when one compares the tripartite structure of Displays 5 and 2. And the kin ship at work is all the more strikingly notable when one acknowledges that consideration of economy and simplicity is in each case the driving force of the process of systematization that is at work.

VI. Einstein's Approach Illustrated

But how is one to obtain probabilities from nonprobabilistic processes via considerations of simplicity and rational economy? The general idea is implicit in

what Einstein wrote to Bohm: "I do not believe in micro- or macro-laws, but only in structure laws that lay claim to a universally binding validity."[40] And the pivotal fact is that when such laws are to serve the interests of simplicity across the entire ensemble of possible state-conditions, a recourse to probabilities can become derivatively necessary. For once one poses a problem to nature – or indeed once nature sets a problem to itself – the economic factors of the effectiveness and efficiency of a given solution come upon the agenda. And just here it becomes possible for probabilities to enter in, seeing that there are some such optimization problems that are best addressed by probabilistic machinery.

Display 6
SYSTEMIC EVOLUTION

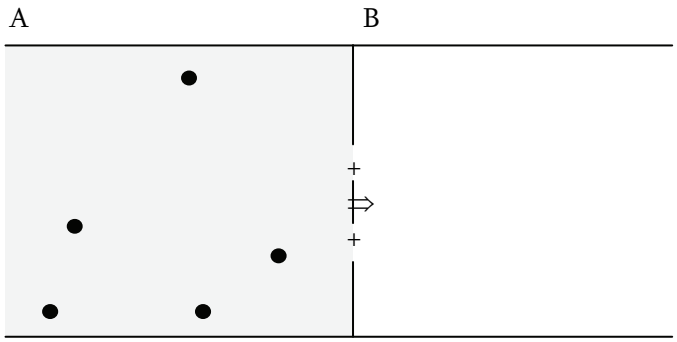


To convey a general idea of such a pathway to probability let us consider a simple illustration of a solution of the type familiar from classical rational mechanics which endeavored to show how various laws of nature are as is because conformity to them provides for maximal efficiency-effectiveness-economy of operation. Consider a process Π that takes all constituents of a physical system Σ

[40] Letter to Bohm of 24 November 1954. See Van Dongen, *Einstein's Unification*, 181.

from an initial state to an end-state as per the top of Display 6. But now let it be that this can be accomplished by one of two pathways P_1, and P_2, where that initial state can be any one of an ensemble of alternatives Z. (See the bottom half of Display 6).

Display 7



Two lawful modes of transition can be contemplated here:

(1) One based on a deterministic law to the effect that every Type 1 constituent of the system effects its state-transit by pathway P_1, and every Type 2 constituent of the system effects its state transit by pathway P_2.

(2) One based on a non-deterministic law that says that any given constituent effects its transit stochastically by P_1 with probability p_1 and by P_2 with probability p_2.

Now such a transition in the condition of the system can be deemed efficient to the extent that it effects the transition at issue more smoothly – more rapidly or economically – when considered on average across the whole spectrum of the initial-state ensembles. And on this basis, there will be some state-transitions that will operate more efficiently by (2)-style randomness than by (1)-style strict lawfulness.

For an example here consider the set-up depicted in Display 7. At issue here is a hypothetical transmission process where there is to be a transfer of the five 'units' distributed on side A of a barrier to side B. There are two revolving-door turnstile considerations between the two sides, each of which can allow the passage of one 'unit' per second. Now consider two possible and plausible lawful rules for unit transit:

I. Effect transit via the nearest passageway.

II. Effect transit via a passageway selected 50:50 at random.

The rule to be adopted is to be general, covering the entire spectrum of alternatives – the 'ensemble' range of alternative possibilities for distributing units or the *A* side of the barrier. It is clear that, in these conditions, rule II would make the transfer of units from such to side *B* no-one efficient (i.e. faster). For if efficiency to be achieved throughout the entire ensemble of possible initial conditions, then the behaviour of individual constituents may well have to be governed by laws geared to probabilities. It would obviously be more efficient and speedy to have those units pick a gate at random to minimize a traffic jam than to follow a uniformly fixed strict rule.

Further, consider also the prospect that those two connective turnstiles rotate at different speeds, say one at twice the rate of the other. Then the optimizing rule for those individual units would not be to effect a transit with one-to-one-randomness as between the two turnstiles but to head for the faster at a two-to-one ratio of probability. For maximum efficiency the operative probability would then have to be adjusted to the mechanical mode of turnstile operation. Probability would thus become derivative from nonprobabilistic features of the *modus operandi* of the physical set-up at issue via considerations of efficiency and economy.

What we have here is the realization of a mode of operation which, while indeed allowing the individual subunits of a system "to throw the dice" as it were in line with probabilistic variation, this so functions as to realize a process which overall achieves its product in a way that is optimally effective, efficient, and economical.

As such illustrational indicate, it can readily prove to the advantage of a system in point of economy, stability, or viability that its components should behave randomly. For rigid regularity involves overload, and randomness helps to keep things on an even keel. Take the analogy of human affairs. Not every passenger should go to the same side of the boat. In evacuating an unevenly occupied building the universalized instruction "Go to the nearest exit" may not be as effective as "Just leave" (by whatever exit you may wish).

The salient point of such a condition of things lies in its showing that if certain definite global conditions are to be realized with maximal efficacy in the comportment of a physical system, then its constituent elements may have to conform to probabilistic laws of behavior. On such an approach, probabilities need not enter by unexplained fiat, but can prove to have an explanatory rationale in

terms of fundamental principles. And it was apparently just this sort of thing that Einstein had in view.

The fact of it is that Einstein had nothing against probabilities as such: it is only *elemental* probabilities that have no rationale in considerations of principle that he finds objectionable. "That there should be statistical laws that require God to throw dice in each individual case, I find highly disagreeable."[41] Accordingly, probabilities should not just spring into being *ex nihilo,* but should emerge as part of a solution to a problem of optimization under plausible constraints.[42] As he saw it, those physical processes – probabilistic and improbabilistic alike – should have a cogent rationale. He did not hesitate to decline that "When I am judging a theory I ask myself whether, if I were God, I would have arranged the world in such a way."[43]

Conclusion

The definitive task of Leibnizian Physics – and of the rational mechanics to which it gave rise – was to show that the laws of nature themselves represent solutions to problems of optimization under constraint mediated by considerations of economy and efficiency: in sum, to equip those laws with a rationally cogent explanation for being as is. As this approach developed from Maupertuis to Hamilton, rational mechanics was a realization and elaboration of the Leibnizian vision of physics with its prospect of grounding the laws of nature in underlying principles of economy and efficiency. And Einstein's position with respect to quantum theory ran along just these lines. For what Einstein wanted was a functional account showing the physical laws of nature as we have them to be the optimal means of satisfying basic principles of rational economy – and so for quantum theory to be exhibited as the best solution of a problem of rational design. In short, Einstein's great pragmatic desideratum in physics was isomorphic with that of Leibniz. Einstein too was committed to the quest for a Leibnizian physics.

The idea of probing behind the laws of nature to consider why they should be what they certainly fascinated Leibniz and impelled his thought in a

---

[41] Calaprice, *The Expanded Quotable Einstein*, 260.

[42] John Norton has reminded me that in other contexts too search problems such as that of the travelling salesman will often be solved most efficiently by probabilistically geared processes.

[43] Calaprice, *The Expanded Quotable Einstein*, 259. Given this perspective on the matter it should be clear that (despite Peter Pesic, *Labyrinth: A Search for the Hidden Meaning of Science* (Cambridge: MIT Press, 2000), 149-50) Einstein did not flatly object to having randomness as such play a role in nature. He objected, rather, to having this transpire without a cogent justificatory rationale.

theological direction. And even Einstein himself was on board here – at least in his more ruminative moments. For he expressed surprise that "despite such harmony of the cosmos as I, with my humble human mind, am able to recognize, there yet are people who say that there is no God."[44] To be sure, Einstein's God was certainly not personal and anthropomorphic but rather something along the lines of a governing force or power endowing the universe with a harmonious rational order – something akin rather to the cosmic *nous* of Plato's *Timaeus* than to the Judeo-Christian God.

But the fact remains that when Einstein made his oft-quoted remark "I believe in Spinoza's God who reveals himself in the orderly harmony of all that exists"[45] he was far from being on target. For his position with regard to the rational methodology of physics was in fact much closer to the optimalism of Leibniz than to the absolute necessitarianism of Spinoza.[46]

The crux of the matter is that Einstein wanted quantum probabilities to be obtained by derivation under the aegis of rationally cogent basic principles. In specific he was seeking for a higher-level perspective of physical principles that would engender the probabilistic detail of quantum theory as the demonstrably adequate resolution of a problem of optimization under constraints – a projection of the classic standpoint of rational mechanics into the latter-day realm of quantum mechanics.[47] And there really seems to be no ultimately compelling reason of fundamental principle why he cannot have his way here.[48]

---

[44] Calaprice, *The Expanded Quotable Einstein*, 214.

[45] Calaprice, *The Expanded Quotable Einstein*, 204. Also quoted in Rebecca Goldstein, *Incompleteness: The Proof and Paradox of Kurt Gödel* (New York: Norton, 2005), 259. Of course, Einstein rejected the idea of a personal God. See Calaprice, *The Expanded Quotable Einstein*, 146-53. Einstein felt a spiritual kinship with Spinoza as a fellow Jew (see Pesic, *Labyrinth*, 144-45). And he lacked Gödel's familiarity with Leibniz's thought.

[46] Spinoza's necessity was absolute and unconditional; Leibniz's necessity was axiological and pivoted on an optimality geared to harmony, economy, and elegance to design. And just here Einstein actually took the Leibnizian route: "What really interests me is whether God could have created the word differently; in other words *whether the demand for logical simplicity* leaves and freedom at all." (Calaprice, *The Expanded Quotable Einstein*, 221: my italics.) Spinoza's necessity is unconstrained; Leibniz's is constrained by conditions of harmony, economy, simplicity, that is, by just those value considerations that Spinoza eschews.

[47] Actually, a way of developing *relativity* theory within the framework of rational mechanics is developed in Arnold Sommerfeld's *Electrodynamics: Lectures in Theoretical Physics*, Vol. III, trans. E. G. Ramberg (New York: Academic Press 1964); German original: *Vorlesungen über theoretische Physik* (Wiesbaden: Klemm Verlag, 1945). I owe this reference to my colleague Kenneth Schaffner.

[48] This chapter was presented as a Luncheon Lecture at the Center for Philosophy of Science at the University of Pittsburgh in September of 2010.

TRUTH AND THE CRITIQUE OF REPRESENTATION*

Gerard Leonid STAN

ABSTRACT: The correspondence theory of truth was regarded for many centuries as the correct position in the problem of truth. The main purpose of this paper is to establish the extent to which antirepresentationalist arguments devised by the pragmatists can destabilise the correspondence theory of truth. Thus, I identified three types of antirepresentationalist arguments: ontological, epistemological and semantic. Then I tried to outline the most significant varieties for each type of argument. Finally, I evaluated these counterarguments from a metaphilosophical perspective. The point I endeavoured to make is that these arguments are decisive neither in supporting the pragmatist theory of truth, nor in proving the failure of the correspondence theory of truth. Actually, we are dealing with two distinct modes of looking at the same problem, two theoretical approaches based on different sets of presuppositions. By examining the presuppositions of the classical theory of truth, the pragmatists engage in a theoretical undertaking with therapeutical qualities: they contributed significantly to the critical evaluation of a series of dogmas. The belief in the power of the human mind to mirror reality exactly as it is was one of these dogmas.

KEYWORDS: antirepresentationalism, pragmatism, corespondence theory of truth, presupposition

I. Introduction

The approach provided by the correspondence theory of truth was regarded for many centuries as the correct and 'obvious' position in the problem of truth. For a long time no thinker doubted the validity of this theory. The postulate "a proposition is true if and only if it corresponds to a fact" seemed to have the indisputability of a divine commandment. Forced by the epistemic consequences of the distinction between things in themselves and phenomena, Kant is the first

* ACKNOWLEDGEMENT: This paper was made within *The Knowledge Based Society Project* supported by the Sectorial Operational Program Human Resources Development (SOP HRD), Financed by the European Social Fund and by the Romanian Government under the contract POSDRU/89/1.5/S/56815.

philosopher to have questioned the validity and justifiability of the correspondence theory of truth. The German philosopher notes that an epistemic subject can compare an empirical judgment with its corresponding thing only insofar as it is able to *know* the said thing. Thus, the empirical judgment (a second-order representation) is brought into relation not with the object as such, but with a sensible reconstruction of the object (a first-order representation). One can go so far as to say that the judgment of a thing is in concordance with a mental fact (a sensible intuition), and not with the exterior thing. But since the epistemic access to the thing in itself is not possible, we can never know whether the phenomenon corresponds to the thing in itself; consequently, we can never know whether the judgment of an epistemic subject, formulated on the grounds of the 'synthesis of phenomenal data' represents the reality accurately or corresponds to it. As Putnam noted,

> you must not think that because there are chairs and horses and sensations in our representations, that there are correspondingly noumenal chairs and noumenal horses and noumenal sensations. There is not even a one-to-one correspondence between things-for-us and things in themselves. Kant not only gives up any notion of similitude between our ideas and the things in themselves; he even gives up any notion of an abstract isomorphism.[1]

Therefore, the judgments of sensible and rational beings cannot *on principle* be exact representations of things. But if the kantian argument is accepted, the very grounding of the correspondence theory of truth is brought into question.[2]

The chief purpose of this research paper is to establish, in principle, the extent to which a series of first-order antirepresentationalist arguments[3] can destabilise the correspondence theory of truth. Thus, I will first identify and expose the main objections formulated by pragmatist philosophers to the power of representation of the mind. At the same time, I will try to find possible

---

[1] Hilary Putnam, *Reason, Truth, and History* (Cambridge, London, New York: Cambridge University Press, 1981), 63-64.

[2] The connection between the epistemology of representation and the correspondence theory of truth is also emphasised by Habermas: "The concept of knowledge as representation is inseparable from the concept of truth-correspondence. When we discard one of them, we cannot retain the other." (Jürgen Habermas, *Etica discursului şi problema adevărului* [*Discourse Ethics and the Problem of Truth*] (Bucureşti: Editura Art, 2008), 60 – my translation.)

[3] Antirepresentationalist, antifoundationalist and fallibilist arguments make up the core of the pragmatist position in relation to classical epistemology.

perspectives from which to counter these objections. Finally, I will critically evaluate these counterarguments from a second-order, metaphilosophical perspective, and I will try to establish the extent to which this type of counterarguments can be regarded as decisive in the rejection of the correspondence theory of truth. Finally, the point I will try to make is that these arguments are decisive neither in supporting the superiority of the pragmatist theory of truth, nor in proving the failure of the correspondence theory of truth. Actually, we are dealing with two distinct modes of looking at the same problem, two theoretical approaches based on different sets of presuppositions. Yet, from this perspective, we can never argue that a set of presuppositions is better than another. They simply exist, are adopted as being natural or obvious and eventually come to organise the solutions to problems in almost necessary formulae.

II. Types of antirepresentationalist arguments

The pragmatist approach to truth was meant to be, since its beginnings, an alternative to the correspondence theory of truth. Pragmatists devised counterarguments to this theory based on the idea of the impossibility of the epistemic subject to build exact representations of the states of the external world. Rejecting the possibility of representing facts through propositions was equivalent to rejecting the correspondence theory of truth. In the pragmatist philosophy, this idea generated three types of counterarguments to the traditional theory of truth: ontological, epistemic and semantic counterarguments.

The *ontological counterarguments* essentially state that the idea of the existence of an external reality, which can be known as it is, is a metaphysical dogma. Terms and sentences cannot be directly connected to facts, and their reference is left uncertain. Reality remains most of the times inscrutable and therefore sentences cannot correspond to facts. In another, kantian inspired, version, external states of being exist only as conceptual or internal representations of the human mind; consequently, there is no correspondence to facts, but a concordance or coherence between products of the human mind. The mind sooner represents its own internal operations rather than external states of being. Thus, the distinction facts-sentences is itself no longer tenable (being declared a metaphysical residue), and the problem of the possibility of representation becomes a pseudo-problem. Such counterarguments were put forth by W.V. Quine, Hilary Putnam and Donald Davidson.

Secondly, the *epistemic counterarguments* to the representationalist thesis state that the human mind, by means of its cognitive structures, cannot accurately mirror or represent the external world; the sensory input does not have an

epistemic nature and thus cannot serve as a basis for knowledge; the products of human cognition – ideas, judgments or other kinds of 'representations' – cannot correspond to facts. Thus, the problem of knowledge should no longer be regarded as a fundamental problem of philosophy. This type of counterarguments can be found mainly in the work of Wilfrid Sellars, W.V. Quine and R. Rorty.

Semantic counterarguments, the third type of pragmatist counterarguments to representationalism, state that there is no vocabulary or set of sentences which would give us the correct representation or description of a state of being. Each vocabulary is merely an instrument which can give us a simple description of a reality; the decision of 'describing' a fact in a specific vocabulary is made exclusively on pragmatic grounds. The physical world does not speak a certain language and cannot help us decide which vocabulary would be more suitable for describing it. Versions of this counterargument are found mainly with William James and Richard Rorty.

The three types of counterarguments are not strictly delimited, as they are in fact instances of the same antirepresentationalist principle in different theoretical domains. We can even regard each type of counterargument as a consequence of the other two. The critique of the power of representation of the mind or of sentences and the critique of external realism are implicitly or explicitly converted by pragmatists into critiques of the idea of correspondence-truth. In the following paragraphs I will try to identify these arguments throughout the pragmatist philosophy and to emphasise the main objections they engendered. At the same time, I will try to understand the presuppositions these arguments are based on. The relevance of these presuppositions will be discussed in the conclusion of this short research paper.

III. Ontological arguments against representationalism

This type of antirepresentationalist argument states that we are connected in knowledge and speech to our minds to such an extent that the contours of external reality appear to us blurry or inscrutable. The fact that the contours of the world become clearer occurs not due to an adequate representation, but to an effort to clarify one's theories and concepts. Clarity belongs to theories and concepts, not to the world as such. The external world seems irretrievably lost and states of being, inscrutable. The tradition of Western philosophy made 'reality' into one of the obsequious names of God, out of a religious need to worship a non-human power. The antirepresentationalist ontological arguments of pragmatists were devised precisely to free the human mind from the toils of Reality, from the

trap of another divinity. I will trace the way Quine and Putnam developed this type of argument.

In *Word and Object*, W.V. Quine argues that the connection between language and the world is ensured by 'occasion' sentences, meaning that they possess stimulus meanings that are the same for all members of a linguistic community. Occasion sentences, for Quine, "are sentences such as 'Gavagai,' 'Red,' 'It hurts,' 'His face is dirty' which command assent or dissent only if queried after an appropriate stimulation."[4] The main characteristic these sentences have is that their truth varies with momentary sensory stimulations. Yet more individuals can experience the same stimulation because for Quine, stimulations are universals. When is a sentence of this kind understood? Quine's answer comes naturally: a sentence can be understood when the fact that makes it true is identified.[5] However, the problem of identifying the said fact is not that simple. On the one hand, it seems possible to identify the fact corresponding to an occasion sentence by means of identical sensory stimulations. On the other hand, the problem of identifying the facts corresponding to standing sentences (sentences that do not change their truth value with different sensory stimulations) is almost insolvable. As we shall see, understanding such an utterance and, ultimately, the fact it represents is impossible without understanding the theoretical framework that makes it possible.

The theoretical consequence of the famous experiment of radical translation in *Word and Object* is the thesis of the inscrutability of reference, fundamental in rejecting representationalism from an ontological perspective. The linguist's translation of the expression 'Gavagai,' uttered by the speaker of a completely unknown language while pointing towards a rabbit, is problematic. 'Gavagai' could mean: 'Rabbit,' "This is a rabbit's foot," but also 'Animal,' 'Rodent' or 'White.' What is the origin of this referential ambiguity? It is the fact that the linguist does not know the 'referential mechanism' or the 'individuation mechanism' (demonstratives, articles, pronouns, the distinction singular-plural etc.) of the language he/she has just got in contact with. The linguist can acquire only a possible stimulus meaning of 'Gavagai' from sensory stimulations. In this case, its translation would have to involve a correlation with non-verbal stimulations (behaviour, context etc.). The stimulus meaning of an utterance or

[4] W.V.O. Quine, *Word and Object* (Cambridge: MIT Press, 1960), 35-36.

[5] W.V.O. Quine, "Mind and Verbal Dispositions," in W.V. Quine, *Quintessence. Basic Readings from the Philosophy of W.V. Quine*, ed. R.F. Gibson, Jr. (Cambridge, London: Harvard University Press, 2004), 317.

the meaning acquired from sensory stimulation sums up an individual's disposition to accept or reject the utterance as a response to the stimulation.[6] 'Gavagai' has a correct empirical meaning for the native, but not for the linguist.[7] In other words, 'Gavagai' is an observation sentence referring to a reality that is well determined for the native; on the contrary, it is not clear for the linguist whether the sentence is a perceptual one, and the reference is left opaque. The simple ostensive experiment, uncorroborated with a mastery of the individuation mechanism of the natives' language ('this one', 'the same as...', 'different from...') does not allow the linguist to identify the reference correctly.[8] In a later work, Quine states that the linguist should base his/her attempt at translation not only on stimulus meaning, but also on empathy with the native's experience.[9]

Without going too deeply into the theoretical nuances of the radical translation experiment, we can perceive the reasons leading to the assertion of the *inscrutability of reference*. Rorty believes that Quine's greatest ontological contribution was the dissolution of the fundamental distinction between language and fact.[10]

If the reference is hard to identify, if language is so opaque that the reference becomes inscrutable, if language and reality are 'entities' that cannot be distinguished in order to understand how they relate, then the possibility of representation seems definitively compromised.

In *Reason, Truth, and History*, Hilary Putnam devised an antirepresentationalist argument based on ontological observations. What he tries to demystify is the world as it was thought by the external realist. For the external

---

[6] Quine, *Word and Object*, 34.

[7] The native knows the correct meaning not in the sense that there is an entity in his mind we could call 'the meaning of Gavagai,' whereas in the mind of the linguist that entity is inexistent. In *Ontological Relativity*, Quine says: "To discover the meanings of native of the native's word we may have to observe his behavior, but still the meanings of the words are supposed to be determinate in the native's mind, his mental museum, even in cases where behavioral criteria are powerless to discover them for us. When on the other hand we recognize with Dewey that 'meaning … is primarily a property of behavior,' we recognize that there are no meanings, nor likenesses nor distinction of meaning, beyond what are implicit in people's dispositions to overt behavior." (W.V.O. Quine, "Ontological Relativity," in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969), 28-29.)

[8] Ilie Pârvu, *Arhitectura existenței* [*The Architecture of Existence*], vol. II (Bucureşti: Editura Paideia, 2001), 144.

[9] W. V. O. Quine, *Pursuit of Truth* (Cambridge, London: Harvard University Press, 1992), 43.

[10] Richard Rorty, *Philosophy and the Mirror of the Nature* (Princeton: Princeton University Press, 1979), 202.

realist, the world is ready-made, made up of self-identifying things. Things and states are what they are even without the taxonomies of natural scientists. The signs in the mind which stand for a thing have a causal relation to that thing. On the other hand, the internal realist is convinced that signs do not correspond intrinsically to objects. For example, in a mind there can be signs, such as an opinion on electrons, which originates in physics textbooks, not in causal relations to electrons. Thus, it would be absurd to regard the connection signs-objects as intrinsical, unconnected with the one who uses them and with the purpose of using those signs.

> But a sign that is actually employed in a particular way by a particular community of users can correspond to particular objects within the conceptual the conceptual scheme of those users. 'Objects' do not exist independently of conceptual schemes. We cut up the world into objects when we introduce one or another scheme of description. Since the object *and* the signs are alike *internal* to the scheme of description, it is possible to say what matches what.[11]

Therefore, according to Putnam, the objects of the world are rather produced than discovered.[12] We cannot speak of knowing the world 'as it is,' but of a perpetual shaping of its states according to the conceptual schemes we use:

> What I am saying, then, is that elements of what we call 'language' or 'mind' penetrate so deeply into what we call «reality» that the very project of representing ourselves as being 'mappers' of something 'language-independent' is fatally compromised from the very start.[13]

In spite of the fact that the independence of facts from mind and language is compromised, there are experiential inputs to knowledge which science uses. If it were not so, natural science would have been a gratuitous exercise of imagination. But *all* of these experiential inputs to knowledge, according to Putnam, are shaped by our concepts.[14] In this context, to speak about the correspondence of judgments to reality is to adopt the perspective of the divine eye, to believe in the fact that

---

[11] Putnam, *Reason, Truth, and History*, 52.

[12] In *The Many Faces of Realism*, Putnam mitigates his verdict and writes that it would be an exaggeration to say that the mind constitutes the world; the correct thing to say would be that the mind and the world constitute together both the mind and the world. Hilary Putnam, *The Many Faces of Realism* (La Salle: Open Court, 1987), 3.

[13] Hilary Putnam, *Realism with a Human Face* (Cambridge, Massachusetts, London: Harvard University Press, 1990), 28.

[14] Putnam, *Reason, Truth, and History*, 54.

the states of the world possess an essence which can be known at first hand, without a conceptual mediation.

Davidson pushes the ontological criticism of representationalism even further by abandoning the scheme-content dualism. This dualism, present with Quine and even Putnam, is essential to the idea of representation and to the correspondence theory of truth. The scheme-content dualism, common to the whole Western metaphysical tradition, is built around the principle that conceptual schemes organise reality or the sensible data. The result would be that the world is a sort of chest of drawers (made up by the category scheme) in which clothes are stored (the sensible objects or data). Maintaining the scheme-content dualism leads to Quine's conceptual and ontological relativism. On the other hand, discarding this dualism would result in the dissolution of both ontological and conceptual relativity. Furthermore, discarding this dualism would render irrelevant the problem of representation and undermine the legitimacy of the correspondence theory of truth. According to Davidson, the truth value of sentences does not depend any longer on reference to facts, but on reference to other sentences, this being the maximal objectivity epistemic communities can reach.[15]

Even if the arguments of Quine and Putnam do not coincide in all details, both tell us the same thing: the world of the classical realist, made up of states of things independent of the human mind does not exist for us, as humans; 'the world as it is' can be an object of faith only in classical metaphysics, a mere dogma. And since it is absurd for humans to accept the existence of a world in itself, likewise absurd must be the pretension to represent 'the world as it is,' as well as the pretension to correspond to 'the world as it is.'

Obviously, in their turn, these ontological antirepresentationalist arguments gave rise to criticism and counterarguments. They were accused of promoting a form of solipsism, of replacing one dogma with another (for instance, it purportedly replaced the dogma of 'external reality' with the dogma of 'internal reality'), of perpetuating an unclear relation between experience and theory, between the empirical and language, between sensory stimulation and social convention,[16] of promoting a scepticism of meaning, of being self-contradictory

[15] Donald Davidson, "On the Very Idea of a Conceptual Scheme," in his *Inquiries into Truth and Interpretation* (Oxford: Oxford University Press, 1984), 198.

[16] Noam Chomsky, "Quine's Empirical Assumptions," in *Words and Objections. Essays on the Work of W.V.O. Quine*, ed. Donald Davidson and Jaakko Hintikka (Dordrecht: Reidel Publishing Company, 1975), 66.

etc.[17] As Habermas suggests, ontological relativity may appear because we relate to the world as to a totality of language-determined facts. His suggestion is that the world should be suggested as a totality of things. Things are always the same, only the vocabularies or descriptions we create are different.[18] An objection to Habermas could be that by this he tries to smuggle back in the perspective of the divine eye.

IV. Epistemic arguments against representationalism

For pragmatists, the human mind is an instrument more of building and sustaining arguments than mirroring states of being. The epistemic antirepresentationalism adopted by pragmatists is based on the fact that humans cannot leave their own finite and perspectival minds when they know. Any item of knowledge benefits from a sensory input which is shaped and processed by the structures, categories or theories inherent to the human mind. From a pragmatist perspective, this sensory input cannot be regarded as a foundation for knowledge because a sentence can be based only on other sentences. An empirical sentence (a sentence on a fact) can be coherent only with other sentences, not with the fact as such; as Wilfrid Sellars argues, judgments, as epistemic entities, cannot be reduced to data on facts given by the senses, to non-epistemic entities. Therefore, we can never tell to what extent a sentence represents or corresponds to a real fact.

In Wilfrid Sellars's version, this argument takes the shape of criticism of the myth of the 'given.' Traditional epistemology – be it Cartesian or logic emipiricist – regarded as uncritical the distinction between what is inferred about a thing and what is given in the direct experience of that thing.[19] The given, in all its various

---

[17] In this respect, it would be useful to remember one of Graham Priest's comments on Quine. Undoubtedly, the most dramatic consequence of Quine's argument is that "the idea that one refers determinately to objects in talking must be given up." Yet asserting this position leads to a contradiction: "Objects in the world transcend anything we can determinately refer to in speaking. Yet, patently, Quine does refer to rabbits, rabbit parts, and other objects in his ruminations on reference. Even a skeptic about sense would be hard-pressed to deny this. Indeed, even to claim that one cannot refer determinately to objects presupposes that we can refer to those objects (and not to undetached object parts) to say what it is that we cannot refer to. Thus we have a contradiction at the limits of expression..." (Graham Priest, *Beyond the Limits of Thought* (Cambridge, New York, Melbourne: Cambridge University Press, 1995), 220-221.)

[18] Habermas, *Etica discursului şi problema adevărului*, 59.

[19] Wilfrid Sellars, *Empiricism and the Philosophy of Mind* (Cambridge, Massachusetts, London: Harvard University Press, 1997), 13.

forms, refers to 'that which is unmediated,' 'that which is present,' the content of sensory data, sentences, relations, particulars, universals or primary principles (regarded as objects of unmediated knowledge).[20] For classical philosophers, *the given* is the way the human mind is able to anchor itself in the real world. For instance, our empirical knowledge is valid only because our sensory data represent the perceived facts with maximal accuracy. In other words, sensory data give us 'the world as it is.' All true empirical sentences could be regarded as reductible to a set of sensory data. Consequently, the true opinions on the external world would be its exact representations, mediated by the unmediated data of the senses.

Yet Sellars argues that there can be no cognitive capture of the sensory data without processing, modifying, altering them; the simple presence of perceptual experience has no epistemic value, it does not enable us to know anything. Any 'given' comes to possess an epistemic value only through the intervention of the human cognitive structures, of the concepts. The perception of a state of things has an epistemic value only when accompanied by a judgment on that state, only when its content is categorised through concepts.

> For we now recognize that instead of coming to have a concept of something because we have noticed that sort of thing, to have the ability to notice a sort of thing is already to have the concept of that sort of thing, end cannot account for it.[21]

The contribution of the sensory input to knowledge is not denied by Sellars,[22] but it is altered from that present in the classical picture. *On the one hand*, the sensory input can no longer be the fundamental basis for inferential or logic-conceptual knowledge. Therefore, knowledge can make no claim any longer to the status of objective, precise representation it had in the classical empiricist picture due to the certainty of the given. *On the other hand*, the sensory input is altered or 'coloured' in any act of knowledge by our conceptual structures, thus making its purity and its nature of 'given' become doubtful. After all, the 'given' is a sort of philosophical 'legend,' a legend that embodies the ambition of traditional

[20] Sellars, *Empiricism and the Philosophy of Mind*, 14.

[21] Sellars, *Empiricism and the Philosophy of Mind*, 87.

[22] In this regard, Sellars is explicit: "If I reject the framework of traditional empiricism, it is not because I want to say that empirical knowledge has no foundation." (Sellars, *Empiricism and the Philosophy of Mind*, 78.)

epistemology to evince ultimate sources and grounds for knowledge.[23] In brief, the epistemic connection to facts is impossible because the sensory data or facts are not epistemic entities and thus can be neither represented, nor taken as grounds for knowledge or truth conditions of sentences.[24]

Quine's theoretical position is close to Sellars'. This position, generated by his holistic theory of meaning, points towards two interrelated problems: *the translation of a theoretical sentence* and the *subdetermination of theories*. In his famous radical translation imaginary experiment, Quine raises the question of translating not only occasion sentences, but also theoretical (standing) sentences. While occasion sentences can be translated on the grounds of connecting stimulus meanings to behaviours, theoretical sentences can be understood only if one understands the background theories guiding the judgments and behaviour of the natives. In other words, these sentences are not directly rendered true by sensory stimulations, but only by connections with other sentences (which are not directly in contact with sensory stimulations). If the linguist interested in speaking that language could learn enough from the vocabulary and grammar of the natives' tongue, but would also understand the set of theories tacitly adopted by the natives, he/she could translate in his/her own language almost every utterance produced by the natives. If one day somebody told him "Come quickly, a demon has got into Oio-Oio," the linguist could translate this utterance by "Oio-Oio has an epileptic seizure. I must try to help him." The translation was not literal, but it was

> paraphrasing the native's utterance about the demonic possession with one's own, about the epileptic seizure; even so, the function of communication of the language was perfectly accomplished, and both actors of the speech act behaved as the others expected.[25]

---

[23] William S. Robinson, "The Legend of the Given," in *Action, Knowledge and Reality. Critical Studies in Honor of Wilfried Sellars*, ed. H.-N. Castaneda (Indianapolis: Bobs-Merrill Company, 1975), 83.

[24] In the matter of the truth problem, Sellars comes close to the solution envisioned by Peirce: the truth is that which is eventually accepted by everyone examining a certain problem which generates doubt. See Wilfrid Sellars, *Science and Metaphysics* (London: Routledge & Kegan Paul, New York: Humanities Press, 1968) 116-150.

[25] Ion C. Popescu, *Corabia lui Tezeu sau empirismul fără dogme* [*The Ship of Theseus or Empiricism Without Dogmas*] (Bucureşti: Paideia, 1997), 41-42.

In other words, the success of translating a theoretical sentence depends on the extent to which the linguist succeeds in understanding the theory of the native which explains the 'fact' to which it refers.

One and the same fact can be explained by many theories. In Quine's terms, theories are *subdetermined* by the fact. The linguist succeeds in translating a theoretical sentence not through a mechanical synonymity, by automatically replacing some words, but by trying to discover which of his/her analytical theories could correspond to the theory behind the utterance of the native. The reasons which prevent the two different theories from generating discrepancies in actions, since the linguist and the natives succeed in understanding one another and in acting convergently, should be sought in the fact that both theories are coherent with the same set of perceptual sentences, with the same fact.

We can notice a Kantian, transcendental logic in Quine's reasoning; according to Graham Bird, in *Word and Object* Quine paints a Kantian picture of the conditions of possibility of experience.[26] While in Kant's logic, the a priori forms of the subject, of the sensibility and of the intellect made experience and hence, knowledge, possible, in the philosophical picture presented by Quine, a priori forms are replaced by the set of theories or analytical hypotheses adopted by somebody at a certain time. Only by correlating the sensory given with certain analytical hypotheses, with certain background theories can a sentence be understood and analysed from the perspective of truth.

The pragmatist thinker who succeeded in pushing the epistemic criticism of representationalism to its last consequences was Richard Rorty. As he himself states, his epistemological position is based to a large extent on the ideas of Sellars and Quine. In *Philosophy and the Mirror of Nature*, Rorty tries to demystify the way modern philosophers, as well as logical empiricists, theorised the 'neutral' frame of any epistemic experience. This frame was ensured (and it still is, for philosophers who haven't acknowledged the consequences of pragmatist criticism) by the mind seen as a 'mirror of nature,' as a medium capable to obtain 'privileged representations' of facts. We will be looking at the critical evaluation Rorty performs on the representationalist epistemologies of Locke and Kant.

Rorty blames Locke for confusing justification (the relation between sentences and judgments) and causality (the relation between facts and the sensory data). John Locke forgets the fact that every item of knowledge is ultimately a justified assertion and that there are extremely few situations where

[26] Graham Bird, "Editorial Review: Kant and Contemporary Epistemology," *Kantian Review* I (1998): 11.

we take the proper functioning of our body to be *justification* or *grounding* enough.[27] In formulating this objection, Rorty relies on T. H. Green's distinction between an 'element of knowledge' and a 'condition of the body' which allows the acquisition of knowledge. The senses and their proper functioning are purely physiological aspects of the inner workings of a body and not elements of knowledge.[28] And the proper functioning of our senses cannot be regarded as a guarantee for knowledge and sensory data are not epistemically relevant entities.

> Granted that we sometimes justify a belief by saying, for example, 'I have good eyes,' why should we think that chronological or compositional 'relations between ideas' conceived of as events in inner space, could tell us about the logical relations between propositions? [29]

Thus, the logical relations between propositions are not dependent on the relations between sensible ideas or data, which derive from certain physiological or psychic characteristics of the knowing subject. Understanding something about the succession or structure of sensory data does not implicitly mean understanding something about logical, grounding or justificatory relations present between judgments. The analysis of epistemic entities should be made by appealing to other epistemic entities and not by invoking non-epistemic entities.

Rorty tries to explain how 17th century empiricists came to make such an error by saying that they simply did not think of knowledge as justified true belief.

> This was because they did not think of knowledge as a relation between a person and a proposition. We find it natural to think 'what S knows' as the collection of propositions completing true statements by S which begin 'I know that...' (...) But Locke did not think of 'knowledge that' as the primary form of knowledge. He thought, as had Aristotle, of 'knowledge of' as prior to 'knowledge that,' and thus of knowledge as a relation between persons and propositions.[30]

Locke's error is believing that propositions can be justified by facts. But facts get to the mind due to the proper functioning of the senses, yet they have no epistemic relevance. On the other hand, propositions can be justified only by logical relations. And if propositions cannot be justified by facts, then they cannot represent facts and the mind cannot function as a mirror of nature.

---

[27] Rorty, *Philosophy and the Mirror of Nature*, 141.
[28] T. H. Green, *Hume and Locke* (New York: Thomas Y. Crowell Company, 1968), 19.
[29] Rorty, *Philosophy and the Mirror of Nature*, 141.
[30] Rorty, *Philosophy and the Mirror of Nature*, 141-142.

Kant seems to take an important step away from 'knowledge of' towards 'knowledge that,' replacing Locke's 'ideas' with 'propositions.' Yet, in Rorty's view, Kant is still under the influence of representationalism, the reason being that, instead of bringing to the forefront of his epistemological analyses the judgment or the proposition as a fundamental entity of knowledge, he becomes interested in the psychological mechanism by means of which the components of the proposition make the proposition possible. In order to explain this mechanism, Kant appealed to the synthesis of non-linguistical entities, to a series of internal representations – intuitions and concepts. In other words, building a proposition (a linguistical action) is based on the psychological mechanism of synthesis. But grounding a proposition on a psychological representation is an epistemological absurdity. According to Rorty, logical empiricists make a similar error.

These antirepresentationalist arguments were challenged in their turn because they alledgedly lead to epistemic relativism and scepticism, denied the contribution of sensory experience to the forging of scientific knowledge, dismissed inductive logic as a part of the scientific logic, denied that observation sentences are connected to facts, or took epistemology on a road leading to a confusion of truth with justification or assertability etc.

V. Semantic arguments against representationalism

This type of argument is based on the idea, defended by Dewey and Wittgenstein, that language is not an image of reality, but a sort of collection of tools with multiple uses. Consequently, by its very nature, language is not meant to describe facts; language overflies reality, passes over facts, pointing to them rather vaguely in a conversational context. Language does not have a vocabulary capable of 'engaging a dialogue' with real states of things; the reason is simple: reality does not 'speak' a language with a vocabulary. In other words, the descriptive power of language is more a myth than a real property. And if facts cannot be captured in sentences, they cannot be represented, either.

An early version of this argument is found with William James. In his sixth and last 1906 conference, held at the Lowell Institute, Boston, William James accepted that truth is a property of ideas in agreement with reality. Up to this point, his view on truth seemed close to that of the correspondence theory of truth. James believes that the differences between his point of view and that of the supporters of the correspondence theory of truth emerge when the meaning of the terms 'agreement' and 'reality' is specified. The supporters of this theory, according to James, do not succeed in being analytical enough in specifying what this 'correspondence' or 'agreement with reality' is. From his point of view,

sentences referring to facts never become veritable copies. James gives an example: let us think of the idea we have about a clock on a wall. This idea is about the clock's dial, not so much about its mechanism:

> But your idea of its 'works' (unless you are a clock-maker) is much less of a copy, yet it passes muster, for it in no way clashes with the reality.[31]

If the idea we have about a clock could copy reality accurately, it should reflect all the parts that make up the clock mechanism, as well as the way they work together. But this does not happen, and our ideas fail sistematically in copying or representing the object to which they refer. Thus, our idea about a fact does not accurately reflect the structure of that fact, it does not correspond to it precisely, but rather functions as a pointer, and most of the times a vague one at that. The idea about a fact points to that fact, acts as a guide towards the fact, but rarely does it say anything specific about the fact. This idea has a pragmatical value, it is a kind of convention accepted in the communicational and actional interactions between the members of a community. The more an idea enables the orientation of the members of a community in their actions, the better it guides towards a fact, the bigger the chances are for it to be designated as true. But merely designating a name as true does not equal its being considered a faithful representation of the fact to which it guides.

On the trail of James, Wittgenstein II and Davidson, Richard Rorty adopts an iconoclastic position in the understanding of language and truth: language does not have a privileged relation with the states of the physical world; moreover, an epistemic subject does not take a decision regarding the truth of a judgment based on the signs of the physical world. Truth is a property of linguistic entities, like sentences. And as sentences are made, created, so are truths. Heedful of Davidson's ideas, Rorty argues that language is neither a medium of representation of the external world, nor a medium of self-expression.[32] The physical world (or our own self) does not possess an essence that would allow its disclosure or representation only with the help of a special vocabulary. Furthermore,

---

[31] William James, "Pragmatism," in *William James. Writings 1902-1910* (New York: Literary Classics of the United States, Inc., 1987), 573.

[32] Richard Rorty, *Contingency, Irony, and Solidarity* (Cambridge, New York, Melbourne: Cambridge University Press, 1989), 11.

> the world does not provide us with any criterion of choice between alternative metaphors, that we can only compare languages or metaphors with one another, not with something beyond language called 'fact'.[33]

The vocabularies of physics or biology, according to Rorty, are not closer to the 'things in themselves,' and even less 'dependent on the mind' than those used by, for instance, contemporary cultural criticism.[34] In fact, language is a collection of vocabularies, none of them having any privileged status. Alternative vocabularies are rather a type of alternative tools than steps or parts of a special, super unified vocabulary, capable of supplying an accurate representation of reality.[35] Each vocabulary can be a good tool for formulating and solving a specific type of problem.

Language does not have a specific function, of describing states of being and it does not have rigid rules, a purpose, or an essence. Davidson's theoretical principle on which Rorty establishes his position is this:

> There is no such thing as language, not if a language is anything like what many philosophers and linguists have supposed. There is therefore no such thing to be learned, mastered, or born with. We must give up the idea of a clearly defined shared structure which language-users acquire and then apply to cases. And we should try again to say how convention in any important sense is involved in language; or, as I think, we should give up the attempt to illuminate how we communicate by appeal to conventions.[36]

If language does not possess a structure allowing it to represent facts, there can be no sentences corresponding to facts. There are true sentences, only that their truth is not established in reference to facts, but in reference to other sentences other people believe. And since truths depend on sentences, since sentences depend on vocabularies and since vocabularies are created by people, then truths are also created by people. It is not the world that decides on the truth of sentences, but the skill of people joining sentences together to build arguments in their support.

This type of argument could be blamed for semantic relativism, for denying the descriptive capacity of the language of natural science or for trying to reduce

---

[33] Rorty, *Contingency, Irony, and Solidarity*, 20.

[34] Rorty, *Contingency, Irony, and Solidarity*, 16-17.

[35] Rorty, *Contingency, Irony, and Solidarity*, 11.

[36] Donald Davidson, "A Nice Derangement of Epitaphs," in *The Philosophy of Language*, ed. A.P. Martinich (New York, Oxford: Oxford University Press, 1996), 475.

something fundamental (the logical operations of reason) to something less fundamental (the linguistic practices of a community – using a vocabulary or a language).[37]

VI. Philosophical presuppositions and the answers to the problem of truth

After surveying and analysing the main types of pragmatist arguments against the representationalist position, it is only natural that we should ask ourselves whether these arguments can decisively undermine the correspondence theory of truth. In fact, this is how one should judge: since the human mind cannot represent accurately states of things, then the possibility that sentences may correspond to facts is compromised. If we accepted this conclusion, we should also accept that the correspondence theory of truth is compromised. Yet a judgment like this would be totally inadequate. The reason does not pertain so much to the solidity of antirepresentationalist arguments, as to the fact that most of these arguments are based on different presuppositions than the ones on which the correspondence theory of truth is based. The presuppositions in themselves are neither true, nor false, but they confer meaning and a certain configuration to a philosophical (or scientific, artistic etc.) position. The tenets of the correspondence theory of truth make sense only if we accept the fundamental presuppositions of this theory, just as the tenets of the pragmatist theory make sense if we accept the fundamental presuppositions of this theory. By laying side by side the presuppositions on which the two approaches to truth are based, the differences will become more clearly apparent. I hope that the differences on the level of the presuppositions will clarify the differences on the theoretical level. Moreover, I will hopefully render transparent the causes which generated criticism on both sides, as well as the reason for which these criticisms are not decisive in abandoning the classical position.

The traditional approach to truth, the correspondence theory of truth, is based on the following presuppositions: (A) Truth has a nature or an essence which a theory of truth should evince and explain; (B) There is an ontological fissure between sentences or utterances, on the one hand, and things, states of being or facts, on the other; (C) States of things are independent of the human

[37] Thus, according to Thomas Nagel, "Looking for the ultimate explanation of logical necessity in the practices, however deeply rooted and automatic, of a linguistic community is an important example of the attempt to explain the more fundamental in terms of the less fundamental." (Thomas Nagel, *The Last Word* (New York, Oxford: Oxford University Press, 1997), 39.)

mind and its inner workings, but it has a structure that can be understood by the human mind; (D) The human mind works as a mirror of reality; the main functtion of language is to represent states of things; (E) Truth is the name of a relation between sentences and states of things; (F) In a way, facts and states of things compel us to consider some sentences true and others false; (G) There is a single true description of a state of things, the one that captures its structure; the human mind can reach a situation which would allow it to make the correct (ultimate, true) description of a state of things; (H) On principle, a correct and complete description of the whole reality is possible.

On the other hand, the presuppositions of the pragmatist theory of truth – most of which were valid in the previously analysed argumentative sequences – are the following: (a) Truth has no essence or specific nature or, at best, they are not problems worthy of attention; (b) The philosophical investigation of truth should not seek to answer the question "What is the nature or the essence of truth?"[38]; (c) The essence is rather a philosophical construct, a product of the thirst for homogeneity of classical metaphysicians; (d) The world exists for individuals only as an epistemic given, as an internal reconstruction, as reality in the mind of the epistemic subject; (e) No clear lines can be drawn between concepts and facts; (f) Language does not have a specific function (of describing reality or self-expression); (g) Processing the sensory data is by default equivalent to *distorting* it through concepts and theories; (h) The world cannot compel us in any way to accept a sentence as true; (i) Truth is a kind of coherence between opinions. Most of these presuppositions were valid in the antirepresentationalist arguments examined in the previous sections. They are not properly justified anywhere in the pursuits of pragmatist philosophers, but they are the sometimes unseen pillars of their theoretical attitude towards the impossibility of representation and overcoming truth as correspondence to facts.

The pragmatist theory of truth is an alternative to the correspondence theory of truth not in the sense that it brings fair counterarguments, but in that it build its position (and, implicitly, its counterarguments to the correspondence theory of truth) based on other presuppositions, other evidences. The merit of those who defend this theory is that perhaps they are less inclined to accept

[38] Rorty's opinion is that "'The nature of truth' is an unprofitable topic, resembling in this respect 'the nature of man' and 'the nature of God,' and differing from 'the nature of the positron,' and 'the nature of oedipal fixation.' But this claim about relative profitability, in turn, is just the recommendation that we in fact say little about these topics, and see how we get on." (Rorty, *Contingency, Irony, and Solidarity*, 8.)

certain presuppositions as dogmas. In criticising the presuppositions or dogmas of the classical theory, they became more aware of the risk of uncritically accepting a theoretical position. Antirepresentationalist arguments had the undeniable merit of making us understand that the idea of a mind which, by its nature, mirrors or represents states of being is not self-evident. Likewise, the idea of founding knowledge on facts, on non-epistemical entities is not self-evident.

After all, pragmatism proposes a shift in the 'philosophical attention' towards another *Gestalt*, towards another theoretical configuration. Realism and pragmatism are different theoretical attitudes configured by the adoption of different presuppositions. The question "Which approach to truth is legitimate and correct?" presupposes a sort of hierarchical monocentrism of philosophical approaches that is not at all legitimate. Theoretical solutions to the problem of truth are alternate, but not in the sense that they are better than others or that all theories are equally good. They are alternate because they are generated by different sets of presuppositions. From this perspective, as they belong to different philosophical traditions,[39] they are incommensurable and reciprocally opaque. If this is indeed the case, then we can neither tell which approach is better, nor say that they are equally good. But, since reason is neither realist, nor pragmatist, arguments and counterarguments can be devised and assessed. Yet we cannot find objective standards which would allow us to decide which configuration of presuppositions will lead to a more workable theory of truth. We cannot build a theoretical position without presuppositions or one which would neutralise the presuppositions of different approaches under discussion. Examining a problem from another perspective than the traditional one – in this case, the idea of representation, fundamental to the correspondence theory of truth, from a pragmatist perspective – is eventually a *therapeutical* undertaking. Finally, such an

[39] I am using the expression 'philosophical tradition' in a sense close to that of 'research tradition', used by Larry Laudan to explain the system of representations more or less tacitly accepted by scientists who have been or are working in the field of the same science: "A research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain." (Larry Laudan, *Progress and its problems. Toward a Theory of Scientific Growth* (Berkeley, Los Angeles, London: University of California Press, 1977), 81.) While research traditions, depending on the theories developed in their midst, can be confirmed or disproved, philosophical traditions can only prove to be more or less fertile. In any case, the emergence of another philosophical tradition is not equatable with the *disproof* of the present philosophical traditions, but a 'shift of vision' towards another significant *Gestalt* of presuppositions.

undertaking guards us from dogmas by showing that a certain perspective in thinking a problem is neither natural, nor self-evident. By looking through the pragmatist lens at the presuppositions of the classical theory of truth, we understand how little obvious its tenets are. And this is precisely the main philosophical benefit: bringing into critical discussion theses that were traditionally accepted without any critical evaluation.

VAGUENESS, IGNORANCE, AND EPISTEMIC POSSIBILITIES

Zoltán VECSEY

ABSTRACT: The paper focuses on a hitherto unexamined version of the third possibility conception of vagueness. It is claimed that statements about borderline cases can be treated by analogy with statements about epistemic possibilities. The proposed account can be readily subsumed under the generic category 'third possibility view' because, in contrast to definitively true and definitively false application cases of vague predicates, statements about borderline cases are interpreted as non-truth-functional.

KEYWORDS: vagueness, ignorance, epistemicism, third possibility views

I. Introduction

In order to explain away the apparently paradoxical features of borderline cases, third possibility theories of vagueness are typically forced to introduce non-classical truth values into their semantics. Theories that make use of the Strong Kleene evaluation scheme or the method of supervaluations have to solve the puzzles of borderline statements by postulating truth value gaps. According to these theories, statements about borderline cases of a vague predicate come out true on some valuations and false on others, and are thus neither definitely true nor definitely false. One reasonable way to recover this semantic deficit is to conceive gappy statements as representing a third truth value, say ½. Borderline statements are then assigned the value ½.

Something similar happens in the case of paraconsistent accounts of vagueness. Theories that are committed to standard systems of paraconsistent logic have to admit truth value gluts in their formal semantic frameworks. Dialetheists, for example, permit borderline statements to be both true and false. This form of semantic anomaly is thought to be effectively resolved by the introduction of a non-classical truth value 2. While classical values 1 and 0 represent definitely true and definitely false statements, respectively, the third value 2 has the function of representing borderline statements that are supposed to fall in the intersection of definitive truth and definitive falsity.

© LOGOS & EPISTEME II, 2 (2011): 273–274

Theories of vagueness based on many valued logic differ from the accounts mentioned above in that they operate with a set of non-classical truth values instead of only one. In many valued settings borderline statements can take any member of the set of real numbers in the closed interval [0, 1] as they truth value. Such numerical values are often equated with degrees of truth. The main idea is that the higher numerical value a borderline statement has, the closer it is to definite truth, and similarly with lower values and definite falsity.

On my view, neither of the presently available third possibility solutions for dealing with the problem of borderline cases is entirely satisfactory. There is a general argument against them that goes something like this.[1] Predicate vagueness seems to indicate the presence at least of three things. First, if *F* is vague, then there must be cases where the application of *F* is definitely true and cases where the application of *F* is definitely false. Second, if a sorites series is created for *F*, then there must be a seamless transition between the cases of definitely true and definitively false applications. Third, there must be a borderline area of cases where the applications of *F* are neither definitely true nor definitively false. The question arising from this quick characterization is the following: Is there a noncontradictory way to find a proper semantic classification for borderline cases between the poles of definite truth and definite falsity? In positing a novel type of truth value – ½, 2, or real numbers in the closed interval [0, 1] –, third possibility theories answer the question in the affirmative. A crucial problem with this kind of answer is, however, that it is not in line with the seamlessness of the transition between contrasting cases of applications. More concretely, if borderline applications were assigned a third type of truth value that is strictly incompatible with definite truth and definite falsity, seamless transitions would have to be regarded as impossible. Instead of seamlessness, one would be confronted with sharp demarcations between true or false applications and applications that are gappy, glutty, or have an intermediate degree of truth. That would imply that we should categorically deny the existence of one of the most basic phenomena of predicate vagueness.

Given the simplicity and persuasiveness of this argument, I am inclined to think that all existing third possibility theories of vagueness should be abandoned.

---

[1] For further details see Crispin Wright, "On Being in a Quandary: Relativism, Vagueness, Logical Revisionism," *Mind* 110 (2001): 45–98, and "The Illusion of Higher-Order Vagueness," in *Cuts and Clouds: Vagueness, Its Truth, Its Nature, and Its Logic*, eds. Richard Dietz and Sebastiano Moruzzi (Oxford: Oxford University Press, 2010), 523-549, Stephen Schiffer, *The Things We Mean* (Oxford: Clarendon Press, 2003), and Matti Eklund, "Vagueness and Second-Level Indeterminacy," in *Cuts and Clouds*, 63-76.

At the same time, I do not think that the argument suffices to show that any such theory is untenable. As it frequently happens in other branches of fundamental linguistic-philosophical research, the theoretical possibilities in this domain of investigation are not yet fully explored. The remainder of this paper will focus on a hitherto unexamined version of the third possibility conception of vagueness. It will be claimed that statements about borderline cases can be treated by analogy with statements about epistemic possibilities. The proposed account can be readily subsumed under the generic category 'third possibility view' because, in contrast to definitively true and definitively false cases of application, statements about borderline cases will be interpreted as non-truth-functional.

II. "It is definitively the case that *Fa*"

Most contributors to the vagueness debate are of the opinion that the definitely operator ought to play a central role in the characterization of borderlineness.[2] It is not too surprising, however, that there is little agreement about how to specify that role in a generally acceptable way. One of the reasons behind the disagreement is that the views about the relationship between definiteness and truth differ significantly as we move between competing sides of the debate. Those who hold that borderline applications of vague predicates must be associated with some kind of non-classical truth value like to try to persuade us that the definitely operator should be interpreted in semantic terms. Consider the example of supervaluationism. Supervaluationists typically hold that the application of *F* to *a* is definitely true only if *Fa* comes out true under all semantically admissible evaluations. On the other hand, they suggest that definiteness and truth come apart when *a* counts as a borderline case of *F* according to some evaluations. In these cases, they contend, definitely *Fa* has to be evaluated as false, while *Fa* has to be assigned the value ½. Note that the the definitely operator displays here a splitting behavior. In clear cases of application it appears to be inextricably linked to the truth of the predicate it modifies. Suppose poor Fred has zero hairs on his head. Then the statement 'Fred is bald' comes out true according to all semantically admissible systems of evaluation, and so is definitely true. Beyond the undisputably clear cases, however, where the application of *Fa* becomes neither true nor false, definitely *Fa* should be regarded as false. Take now the case of borderline bald Felix who has 1025 hairs on his

[2] One notable exception is Rosanna Keefe, who argues that the (technical) question of definiteness does not belong to the central part of the theory of vagueness. See her *Theories of Vagueness* (Cambridge: Cambridge University Press, 2000).

head. According to supervaluationists, the statement "Felix is bald" is gappy, but the statement "It is definitively the case that Felix is bald" is false. This is obviously implausible. The definitively operator is similar in one sense to the generalized quantifiers 'some' and 'all.' These are technical terms that have well-understood counterparts in most natural languages. And if the semantics of these counterparts dictates uniform behavior in relevantly similar contexts, then it is also reasonable to require that the technical terms should behave uniformly in relevantly similar contexts. The contexts of our natural language statements about Fred and Felix are similar in the sense that they presuppose a relatively strong correlation between the properties of being definitely bald and being bald.[3] If being definitely bald as applied to Fred or Felix gets assigned a polar truth value in a given context, then it is natural to expect that being bald also gets assigned a polar truth value in the same context. As we have seen, supervaluations directly contravene that rule. Hence, at least intuitively, we may conclude that the supervaluationist semantics for 'definitely' is incorrect in its present form.

The diagnosis given above generalizes across all versions of non-classical treatments of borderline cases. Friends of paraconsistent logic and degree theorists are surely not in a much better position with respect to the clarification of the role of the technical term 'definitely.' These approaches have a common core in that they use the definitely operator in order to demonstrate that borderline applications of vague predicates must be associated with some kind of non-classical truth value. But this does not follow immediately from the intuitive meaning of 'definitely.' Nor does it follow that borderline application cases must give rise to definiteness in any sense. It would then seem better to have an interpretation of the definitely operator which does not involve third type truth values.

One attractive option in this regard is the epistemicist view worked out in details by Timothy Williamson and Patrick Greenough.[4] Epistemicism is well-known for its full preservation of classical logic and its bivalent semantics for

[3] It has to be noted that 'definitely' may be used either as a predicate modifier (*a* is definitely *F*) or as part of a sentence operator (it is definitely the case that *Fa*). Although it is not entirely self-evident, I will assume below that 'definitely' produces exactly the same semantic effects in both cases.

[4] See Timothy Williamson, *Vagueness* (London: Routledge, 1994), and "Reply to McGee and McLaughlin," *Linguistics and Philosophy* 27 (2004): 113–122, and Patrick Greenough, "Vagueness: A Minimal Theory," *Mind* 112 (2003): 235–281. For the critique of the epistemicist position see Stephen Schiffer, "The Epistemic Theory of Vagueness," *Philosophical Perspectives* 13 (1999): 481-503, and Zoltán Vecsey, "Epistemic Approaches to Vagueness," *Dialogue* 49, 2 (2010): 295–307.

vague discourse. Since the usual principles of classical reasoning leave no conceptual room for postulating third type truth values in the semantic machinery, epistemicists are in a position to provide a conservative-style explanation for the definitely operator. According to their view, the phenomenon of definiteness, as it appears in ordinary epistemic situations, can be exhaustively explained in terms of knowledge. The basic idea may be roughly stated as follows:[5]

> DEFINITENESS: *a*'s being definitely *F* consists in the absence of obstacles to knowing that *a* is *F*.

Reflecting on this proposal, one may wonder whether "It is definitely the case that *Fa*" has the same epistemic status as "It is known that *Fa*." It depends. Williamson himself would argue against the identification of 'definitely' and 'knowably.' His ground for this is that in reasoning with vague predicates, a certain kind of epistemic uncertainty becomes inevitable. The source of the uncertainty is that we are not able to discriminate between cases of *F*-ness that are only marginally different. If two objects are so similar that we do not have any chance to distinguish them with respect to the instantiation of the property of *F*-ness, then we justly believe that both are *F*. But in cases where one of the indistinguishable objects is in fact *F* and the other is not-*F*, our beliefs are not reliable enough to count as knowledge. Williamson thus comes to the conclusion that some of the obstacles to knowing that *a* is *F* may prove ineliminable, even under optimal epistemic conditions. It seems, then, that we have to add a significant restriction to the explanation of the definitely operator:

> RESTRICTED DEFINITENESS: Since some of the obstacles to knowing are ineliminable, "It is definitely the case that *Fa*" cannot be epistemically equivalent to "It is known that *Fa*."

One question immediately arises: If 'definitely' does not collapse into 'knowably,' then why should we think that definiteness can be exhaustively, or at least adequately, explicated in terms of knowledge? Williamson's answer would be that RESTRICTED DEFINITENESS is deliberately vague, because definiteness itself is a vague phenomenon.[6] And this is why it would be folly to try to describe the relationship between definiteness and knowledge in a more rigorous or transparent manner. But now the same basic question arises again: If 'definitely' is

[5] Cf. Williamson , "Reply," 118.
[6] Williamson , "Reply," 118.

indeed a vague expression, then how can it be effectively applied in the epistemicist's analysis of borderline cases? In order to circumvent the difficulty implicit in this question, one may perhaps argue, following Greenough's minimal theory, that DEFINITENESS is superior to RESTRICTED DEFINITENESS, since it enables to express the epistemicist's central insight in a more theory-neutral way. Greenough contends, contra Williamson, that 'definitely' and 'knowably' may be taken to have the same meaning.[7] This is tantamount to acknowledging that truth is not entirely beyond our cognitive reach in undisputably clear application cases of vague predicates. Yet, interestingly enough, the theory Greenough advances does not sanction the acceptance of DEFINITENESS. Rather, it proposes to dispense with the definitely operator altogether. The supposed advantage of this move is that in this way it may become possible to elaborate a minimal conception of vagueness in purely epistemic terms. Perhaps Greenough is not completely wrong on this latter point. But in my view, the rejection of DEFINITENESS leads in the end to an incomplete and hence unsatisfactory theory of vagueness.

The reason why we should insist on the interchangeability of 'definitely' and 'knowably' is remarkably simple. When we say that "It is definitely the case that *Fa*," we are assuming that *a* instantiates a certain set of properties that are jointly necessary and sufficient for being *F*. It would be inconvenient to apply 'is definitely *F*' to *a*, if we were not entirely confident that there is no room for error about *a*'s instantiation of *F*-ness. This may be regarded as the default epistemic assumption concerning our ordinary criteria for the application of 'definitely.' Consider again Fred, who has zero hairs on his head. Given that he is an adult male who has lost all of his hairs because of the natural process of aging, it would be quite pointless to debate that he is definitely bald. In situations like this, where *a*'s instantiation of *F*-ness is beyond any reasonable doubt, the role of the definitely operator consists in ascribing epistemic necessity to *Fa*, and this, in turn, indicates that we are in a position to know that *Fa* is the case.[8] Therefore, if we are entirely confident that 'is definitely *F*' can be applied correctly to *a*, then we know that *a* is *F*.

One might object that the expressions 'entirely confident' and 'correctly' are obviously vague in the last sentence, so there is still no reason to defend the epistemic equivalence between "It is definitely the case that *Fa*" and "It is known that *Fa*." The objection can be easily answered by stating the argument in a less

[7] Greenough, "Vagueness," 251–252.

[8] Cf. Michael Huemer, "Epistemic Possibility," *Synthese* 156 (2007): 120.

informal way. Let us say that 'definitely' and 'knowably' are equivalent epistemically if and only if each of the following conditions is satisfied:

> i. Speakers of a community *c* are competent in using and understanding statements containing the predicate 'is definitely *F*.'
>
> ii. The predicate 'is definitely *F* is used in a transparent epistemic situation.[9]
>
> iii. There is no doubt on the part of the speakers of *c* that 'is definitely *F* applies to *a*.

Of course, one might continue to worry about the presence of implicit vagueness in the extensions of such terms as 'community,' 'competent' and 'understanding', etc. Moreover, one might complain that with the possible exception of the vocabulary of arithmetic every other expression, including 'vague,' is inherently vague.

The best reply to the first worry is to note that it is far from self-evident that the vagueness of 'community' and the likes is of the same semantic kind as the vagueness of 'bald.' The extension of 'community,' for example, seems to lack sharp boundaries, but it would be quite difficult to use it in a typical sorites argument. With respect to second complaint, it can be noted that conceiving vagueness as a pervasive phenomenon endemic to the vocabulary of most languages would reduce dramatically the prospects of coherent theorizing in this domain of research. If our semantic apparatus would indeed be thoroughly vague, then even such theoretical statements were infected with vagueness which are intended to express the pervasive vagueness of vague languages. That would be a bad consequence. I think this line of reply is persuasive enough to reject the above complaints as ill-motivated. Therefore, I take it for granted that there is no vagueness in the conditions i–iii.

So far it has been argued that in a broadly epistemicist framework undisputably clear application cases of vague predicates may be taken as implying knowledge. But nothing has been said about how this insight can help us in the task of characterizing borderlineness. The first step in this direction would be to draw an accurate distinction between cases where *F* definitely applies to *a* and cases where it is indeterminate or indefinite whether or not *F* applies to *a*. In

[9] Under 'transparent epistemic situation' I mean a situation which is not threatened by knowledge-scepticism and Gettier-free.

drawing this distinction, we may rely on those observations which we have already made on the equivalence between 'definitely' and 'knowably.' So we can say that in cases where it is indeterminate or indefinite whether or not *F* applies to *a*, conditions i and ii are satisfied, but condition iii is not satisfied. This means that speakers of a community *c* are competent in using and understanding the predicate 'is definitely *F*,' but they are uncertain whether or not it applies to *a* despite the fact that the epistemic situation they are in is sufficiently transparent. The presence of uncertainty concerning the applicability of 'is definitely *F* to *a* signals explicitly that *a* is not known to be *F* in *c*. On this ground, it can be argued that the difference between clear application cases of *F* and indeterminate or indefinite application cases of *F* depends ultimately on the presence or absence of knowledge on the speakers' part.

At this point, a potential misunderstanding needs to be avoided. It seems reasonable to assume that indeterminate or indefinite application cases of the predicate *F* may be interpreted as borderline cases of *F*-ness. It also seems reasonable to assume that in borderline cases speakers become uncertain of the applicability of 'is definitely *F* to *a* because they do not know whether or not *a* is *F*. In light of this, it is tempting to try to characterize borderline cases in terms of absence of knowledge.[10] But this is not the same as trying to provide a clear definition of borderlineness. Borderline cases cannot be defined on the ground of a clear definition of clear cases, since it would mean that there is a sharp tripartite division between *F*-ness, not-*F*-ness and a middle category in between. Such sharp divisions would be in conflict with the acknowledged seamlessness of the sorites transitions. In this respect, I am agreeing with Diana Raffman, who insists that there can be no definitely borderline cases of *F*-ness.[11] Despite this, we are able to recognize *a* as a borderline case, because we know that we are uncertain whether

[10] It must be added that many theories of vagueness refuse to equate borderline cases with absence of knowledge. The reasons behind the refusal remain, however, in most cases rather obscure. For example, adherents of the psychological theory of vagueness contend that borderline cases have to be explained in terms of the quandary state of speakers. According to this view, quandary is a state of ambivalence rather than ignorance. Unfortunately, *it is not quite clear why* the psychological state of ambivalence should be seen as different in kind from the mental state of ignorance. See Wright, "On Being in a Quandary," and Stephen Schiffer, "Vague Properties," in *Cuts and Clouds*, 109-130.

[11] Diana Raffman, "Demoting Higher-Order Vagueness," in *Cuts and Clouds*, 513. In this paper, Raffman argues that from the impossibility of definitely borderline cases of *F*-ness it follows that there are also no borderline borderline cases of *F*-ness. Raffman's argument, in my opinion, is effective against any theory which would go beyond first-order vagueness.

or not the predicate *F* applies to it. Being a borderline case of *F*-ness in this way presents itself as an elusive property that cannot be clearly defined in terms of positive or negative definite cases of *F*-ness.

The last remark, I must admit, reveals relatively little about how borderline statements should be evaluated in the present framework. If borderlineness is really an elusive property generated by speakers' ignorance, then it is not evident how can statements about borderline cases be assigned any classical semantic value. And if it turns out that these statements cannot be known to be true or false in the classical sense, then the question arises as to how to handle them without invoking non-classical truth values. These are the issues I will deal with in some detail in the next section.

III. Borderline statements and epistemic possibilities

Let us turn back to the example of Fred who has zero hairs on his head. Suppose conditions i–iii are satisfied. Then the statement "Fred is definitely bald" should be assigned the polar truth value true in community *c*. And given that the inference from 'definitely *F*' to '*F*' is valid in standard epistemic logic, the statement 'Fred is bald' will be also true in *c*.

But now let us focus on the case of Felix who has 1025 hairs on his head. Since having 1025 hairs on one's head is not a clear instantiation of the property of baldness, condition iii will obviously fail to hold in this case: speakers of *c* cannot be entirely confident that 'is definitely bald' applies to Felix. Nor can they be entirely confident that it does not apply to Felix. As a result of the arising uncertainty, the statement "Felix is definitely bald" will occupy a borderline status in *c*. The borderline status of "Felix is bald" can then be immediately deduced by using the above inference rule. But note, again, that it would be fallacious to conclude from this that "Felix is bald" ought to be regarded as a definitely borderline statement. Uncertainty does not create a sharp demarcation between non-borderline and borderline statements. What speakers of *c* are supposed to be uncertain of is the correctness of the applicability of the predicate 'is bald' to Felix. This is not the same as to suppose that they are forced to think that the statement 'Felix is bald' is definitely incompatible with polar truth values. What we need is exactly the opposite of that supposition. Namely, we may plausibly hold that borderline statements must be thought to be compatible with truth and falsity. Although speakers of *c* are in doubt whether or not 'is bald' applies to Felix, they have no reason to exclude the possibility that 'Felix is bald' may be evaluated as true. And similarly, they cannot exclude the possibility that 'Felix is bald' may be evaluated as false. If this were not so, we would have to maintain

that they are entirely confident that no polar truth value can be assigned to "Felix is bald." In this case, however, the statement "Felix is bald" would not qualify as borderline in *c*.

So it appears that without leaving open the possibility of its being true or false, *Fa* cannot be recognized as having a borderline status. Borderline statements may therefore be considered as subject to the following modal convention:

OPEN POSSIBILITY: '*Fa*' is true or '*Fa*' is false.

The presence of disjunction in OPEN POSSIBILITY indicates that the borderline status of *Fa* is compatible both with truth and falsity. This may be *prima facie* puzzling, since truth and falsity are defined not only as exhaustive but also as exclusive semantic values in the present framework. And surely, if "'*Fa*' is true" and "'*Fa*' is false" are equally compatible with the borderline status of *Fa*, then these statements must also be compatible with each other, which would involve a contradiction given the exclusivity of truth and falsity. What is the solution to this puzzle?

The proposal of the present paper is the following. The statements "'*Fa*' is true" and "'*Fa*' is false" can be presumed to be truth-functional in OPEN POSSIBILITY. This is correct, however, only if *Fa* itself has to be evaluated truth-functionally. I think there is reason for doubt. Remember that *Fa* counts as borderline in *c* because of its negative epistemic status, that is, because its truth value is not known to competent speakers of *c*. For the same reason, speakers of *c* are not in a position to know whether or not *Fa* is a truth-functional statement. In this situation, the most they are warranted in claiming to know is that *Fa* is compatible with a truth-functional evaluation. But because the truth value of *Fa* is, as a matter of fact, not known in *c*, the non-truth-functional evaluation may be taken to be epistemically privileged. Thus, when speakers of *c* apply the predicate *F* to *a* and *a* belongs to the borderline area of *F*-ness, the resulting statement will be non-truth-functional.[12]

Following this line of thought, the puzzle posed by OPEN POSSIBILITY can be dissolved. We must simply concede that *Fa* does not state a fact about how the world is. It does not state that *a* is in fact *F*. Rather, it states that *a*'s being *F* is

[12] Saying that a particular statement is non-truth-functional does not imply, of course, that it is incorrect or nonsensical to assert it. On the assertability conditions of non-truth-functional statements in general, see Fredrik Stjernberg, "Restricting Factiveness," *Philosophical Studies* 146 (2009): 29-48, and Allan Hazlett, "The Myth of Factive Verbs," *Philosophy and Phenomenological Research* 3 (2010): 497-522.

an open epistemic possibility that speakers of *c* cannot eliminate.[13] In general, then, it seems more appropriate to suppose that borderline statements are governed by the following convention:

OPEN EPISTEMIC POSSIBILITY: *a* might be *F* or *a* might be not-*F*.

In contrast to OPEN POSSIBILITY, no truth-functional contradiction arises here: the epistemic possibilities of *a*'s being *F* and *a*'s being not-*F* are compatible with each other. Speakers of *c* do not possess enough epistemic information to decide whether or not having 1025 hairs on one's head counts as a clear instantiation of baldness. Thus, for all they know, it might turn out both that Felix is bald and that Felix is not bald. But they are not in a position to know which possibility is the actual one.

Conclusion

The approach delineated in the previous chapters allows us to preserve two widely held beliefs about predicate vagueness. On the one hand, it is maintained that there are specific cases of language use where the application of a vague predicate is definitely true or definitely false. On the other hand, it is agreed that in borderline cases the truth value of a vague statement remains unknown even to otherwise competent speakers. The suggested explanation for the latter fact is the non-truth-functionality of borderline statements. In using such statements, speakers do not aim at gaining or expressing pieces of factual knowledge: what they are actually aiming at is only potential knowledge. In this sense, the present approach can be regarded as a third possibility view of vagueness.

But how can a third possibility view of vague predicates do justice to the seamlessness of the sorites transitions? The most serious obstacle is removed, because no tripartite division is posited at the level of semantic values. There are only true and false application cases of vague predicates and, in addition, there are cases that resist the classical truth-functional evaluation. And because every borderline statement may be conceived as epistemically compatible with both

[13] There is an important analogy to the theory of epistemic modality here. According to some versions of the theory, epistemic modal statements do not serve to describe the state of affairs of the world. It is held, therefore, that no truth-functional semantics, bivalent or otherwise, is adequate for representing them. See, for example, Seth Yalcin, "Epistemic Modals," *Mind* 116 (2007): 983–1026, and Eric Swanson, "How Not to Theorize about the Language of Subjective Uncertainty," forthcoming in *Epistemic Modality*, eds. Andy Egan and Brian Weatherson (Oxford: Oxford University Press, 2011).

polar truth values, no sharp boundary can be drawn between the adjacent statements in a sorites series. Or, with other words, there is no borderline statement which can be known to be definitely incompatible with one of the two polar truth values. This is enough to rule out the existence of sharp transitions in typical sorites series.

The present theoretical approach has two further advantages. First, the non-truth-functional status of borderline statements seems to be in full accordance with our ordinary epistemic convictions. When *a* is recognized as a borderline case of *F*-ness, we are usually aware that we cannot acquire sufficient evidence or warrant to decide the question whether or not *a* is *F*. That is why we are inclined to regard the debate about such cases as unresolvable. Second, the approach also corresponds to ordinary ontic intuitions regarding the distribution of properties as they actually are in the material world. When we say that borderline cases cannot be clearly defined in terms of contrasting clear cases, we just say what we think in ordinary situations of reasoning, namely that being a borderline case of a particular property presents itself as an elusive phenomenon.[14]

[14] This paper was supported by the *Research Group for Theoretical Linguistics of the Hungarian Academy of Sciences*.

Debate

IN DEFENSE OF EPISTEMIC ABSTEMIOUSNESS

Alex BUNDY

ABSTRACT: The *principle of suspension* says that when you disagree with an epistemic peer about p, you should suspend judgment about p. In "Epistemic Abstainers, Epistemic Martyrs, and Epistemic Converts," Scott F. Aikin, Michael Harbour, Jonathan Neufeld, and Robert B. Talisse argue against the principle of suspension, claiming that it "is deeply at odds with how we view ourselves as cognitive agents." I argue that their arguments do not succeed.

KEYWORDS: epistemology, disagreement, higher-order evidence

In "Epistemic Abstainers, Epistemic Martyrs, and Epistemic Converts," Scott F. Aikin, Michael Harbour, Jonathan Neufeld, and Robert B. Talisse (hereafter AHNT) argue that the *principle of suspension* "is deeply at odds with how we view ourselves as cognitive agents."[1] Here is the principle:

> **(PS)** If S disagrees with an epistemic peer about p, then S should suspend judgment about p.

They attribute PS to Richard Feldman.[2] While Feldman never explicitly states such a principle, it is clear that PS *is* the sort of principle defended in those works.[3] And, as AHNT nicely show, something like PS *is* regularly invoked in the philosophical literature on disagreement.

AHNT argue against PS by trying to show that by following PS you will (a) potentially mislead others into holding their beliefs more strongly than they

---

[1] Scott F. Aikin, Michael Harbour, Jonathan Neufeld, and Robert B. Talisse, "Epistemic Abstainers, Epistemic Martyrs, and Epistemic Converts," *Logos & Episteme* 2 (2010): 219.

[2] Richard Feldman, "Epistemological Puzzles About Disagreement," in *Epistemology Futures*, ed. Stephen Hetherington (Oxford: Oxford University Press, 2006), 216-36; Richard Feldman, "Evidentialism, Higher-Order Evidence, and Disagreement," *Episteme* 6, 3 (2009): 294-312.

[3] In "Evidentialism, Higher-Order Evidence, and Disagreement," Feldman argues that there are "no special general principles about justified responses to disagreements," but that "there are facts about the evidential impact of disagreements" (295). Feldman thinks that something like PS follows from evidentialism and other plausible epistemic principles.

should, and (b) be setting yourself up for being misled by others who do not follow PS.

I argue that these arguments do not succeed. In reply to (a), I argue first that this is not a reason to think that PS is a bad epistemic rule, and second that PS doesn't even have this consequence. In response to (b), I argue that PS has no such consequence, since in the imagined cases you will have reason to believe that the person you disagree with is not a peer.

I. Preliminary notes

In the literature on peer disagreement, *peerhood* is characterized differently by different authors. Some characterize two people as peers if, roughly, they have the same intellectual aptitude.[4] PS would clearly be false on this understanding of peerhood. It would be false because peers can have different evidence, and so the obviously correct thing to do in a case where you believe p and your peer believes not-p, but you have more and better evidence than your peer, is to continue to believe p. On this account of peerhood, for a PS-like principle to be plausible it would have to be changed to apply to epistemic peers with the same, or equally good, evidence.

In light of this consideration, I suggest that for the purpose of discussing AHNT's article, we regard two people as being peers regarding some proposition p if they are just as likely to be right about p.[5] This way of understanding peerhood is nice, because it generates the puzzles about disagreement without getting into issues of whether it ever makes sense to say that two people have the same relevant evidence, or have the same intellectual aptitude. On this understanding of peerhood, two people having different intellectual aptitude, or having different evidence, does not *entail* that they are not peers, although it might provide evidence that they are not peers. With peerhood so understood, PS seems like a plausible principle.

PS also does not exactly characterize the principle invoked by Feldman. The reason is that it is clear that Feldman is discussing the problem of what one should believe when one is *aware* that one disagrees with someone one regards as a peer. I do think that AHNT understand PS in this way – as applying to cases of

[4] E.g. Feldman, "Evidentialism," 294.

[5] This is Adam Elga's preferred way of understanding peerhood; Adam Elga, "Reflection and Disagreement," *Noûs* 41 (2007): 478-502. See 487 and especially fn21.

acknowledged peerhood.[6] In light of this consideration, I suggest that the following is an unobjectionable modification to PS:

> **(PS+)** If S disagrees with an epistemic peer about p, S is aware of the disagreement, and S is justified in thinking her peer is a peer, then S should suspend judgment about p.

II. First argument: Misleading Alf

Both of AHNT's arguments are based on the following case:

> Betty and Alf are epistemic peers, and they disagree about p – Betty believes p, and Alf believes not-p. They have discussed each other's reasons thoroughly, so that it is clear that they both have approximately the same relevant evidence. And they judge one another to be just as good at evaluating the relevant evidence. Thus they are peers and consider one another to be peers, and so according to PS each should suspend judgment. However, in the given case only Betty suspends judgment. Alf continues to believe not-p, although according to PS this is irrational.[7]

AHNT claim that if Alf keeps his belief that not-p, while Betty suspends judgment regarding p, then Alf may take that as evidence that Betty's evidence is not as good as his. That Alf could gain evidence in this way is supposed to be a mark against PS. I think there are two ways this case might be thought to tell against PS. Both seem to be suggested by AHNT.

The first possibility is that the case is supposed to tell against PS because by following PS, Betty provides Alf with misleading evidence for not-p. The primary problem with *this* claim is that whether Alf is better or worse off epistemically as the result of Betty following PS has no bearing whatsoever on whether PS is a true epistemic principle. That someone may be misled if you follow some suggested norm of rationality does not necessarily show that the norm is a bad one – for such norms are standardly understood as having to do with the *individual's* beliefs, not the beliefs of others. Of course, we can talk about norms that lead to social epistemic goods; but these are not the kinds of norms in question in the literature on disagreement. Norms like PS are not evaluated according to how they contribute to the epistemic good of some other person or sets of people.

---

[6] For example, the fact that they set up their case so that the two characters, Betty and Alf, have shared their evidence with one another indicates that they think it is important that Alf and Betty consider each other as peers.

[7] My paraphrase.

The second possibility is that the case is supposed to tell against PS because *Alf*'s behavior is somehow endorsed by PS. In discussing this case, AHNT claim that "if Betty must take Alf's *immovability* about p as evidence about the insufficiency of her own evidence, then Alf may likewise take Betty's *movability* as evidence in favor of the *strength* of his evidence."[8] Alf is led to take Betty's movability as evidence that he is right by the following line of reasoning, which AHNT endorse:

> If a peer's disagreement is enough to defeat one's reasons, then my peer's movement from full-bore disagreement to suspension of judgment should also be an indicator of the (insufficient) quality my peer's reasons.[9]

In other words, PS entails the following *suspension as evidence* principle:

> **SAE** If S is a peer regarding p, then S's suspension of judgment regarding p is evidence that S's reasons for p are insufficient.

I'm not sure if PS entails SAE but I am willing to grant that it does for the sake of argument. Given SAE, Alf can take Betty's suspension of judgment regarding p as evidence that her evidence is insufficient for her to reasonably believe p. This does not, however, provide any indication that Alf's reasons *are* sufficient; it does not provide Alf with new evidence for not-p. And in the given case, if Alf knows that Betty suspended because of her disagreement with him, it seems obvious that Alf should not take Betty's suspension as an indication that his own reasons *are* sufficient. For him to take her suspension as evidence that he is better positioned evidentially would be for him to double-count his own reasoning and evidence. And even if Alf does *not* know that Betty's suspension was as a result of applying PS, SAE does not say that Betty's suspending would give Alf an additional reason for thinking that his evidence *is* sufficient.

True, if Betty suspends judgment regarding p, then PS no longer applies to the case, and so PS will no longer say that Alf should suspend. But it does not follow from the fact that PS does not apply that the correct response for Alf is to become more confident that not-p. In fact, given the assumption that Alf and Mary have shared most of their relevant evidence, their evidence will be about the same, and

---

[8] Aikin et al., "Epistemic Abstainers," 216. Strictly speaking, it is not Alf's *immovability* that Betty takes as evidence, but merely the fact that Alf believes not-p. At the point in the case where PS applies – the point where Betty believes p and Alf believes not-p – nobody's beliefs have moved.

[9] Aikin et al., "Epistemic Abstainers," 216.

so if Alf really takes Betty to be a peer, he should take her suspension of belief as evidence that *he* should suspend as well.[10]

III. Second argument: Mary's descent

The next objection has to do with the effect that PS has on someone who follows it – i.e. Mary. For suppose that Mary has suspended belief regarding p based on the facts that she believed p, Alf believed not-p, and her and Alf's epistemic peerage. And suppose that Alf continues to believe not-p. Now we have another disagreement – a disagreement where Betty withholds belief regarding p, and Alf believes not-p. AHNT point out that PS does not now apply here, because PS only applies when one peer believes p and the other believes not-p.[11] But, they point out, there is a higher-level disagreement now – one about what the proper doxastic attitude towards p is given their evidence. Mary thinks it is withholding belief regarding p, and Alf thinks it is to believe not-p. If Betty applies PS to this new disagreement, then she will think that the correct doxastic attitude to p is a bit closer to believing not-p than believing p. And assuming a plausible principle that says your belief in p should be in accordance with your take on how the evidence bears on p, Mary should now be closer to believing not-p. But now we have yet another disagreement to which PS must be applied, which will lead to a further disagreement, and so on. The limiting point for Mary, obviously, is the belief that the correct attitude to take towards p will be *believing* not-p. Thus, AHNT claim, PS must be a bad principle, since it is obvious that it is irrational for Mary to come to believe not-p in such a way.

One way to reply to this objection is to claim that by not suspending judgment, Alf is no longer a peer when it comes to p. It seems like it is possible for Betty to reasonably come to this conclusion. After all, as the case is specified Alf and Betty started out as peers, with more or less the same evidence and same reasoning abilities. But now Betty has good evidence that Alf is not living up to his epistemic duties – namely, he is not conforming to PS! So after the first adjustment

---

[10] Thanks to Phil Atkins, Tony Brueckner, Tim Butzer, and Timothy Linehan for help with this last argument.

[11] Principles like PS are sometimes stated in terms of degrees of belief rather than in terms of belief, disbelief, and suspension of belief. A degrees-of-belief version of PS *would* apply to this new disagreement, since Alf and Betty still have different credences regarding p, and so still have a disagreement about p. AHNT's argument here could just as easily be stated against a degrees-of-belief version of PS. And my reply would work just as well to this modified version of the argument.

of her belief regarding p, Betty does not have to continue to apply PS until she ends up believing not-p.[12]

AHNT might reply that *sometimes* when an acknowledged peer is stubborn, that is evidence that the peer has better reasons than you do. And this is surely true – sometimes a peer's being stubborn *is* reason for you to adjust your belief. But if Betty thinks Alf is being stubborn because he believes he has better evidence than her, then again we no longer have a case of peerhood, and PS does not apply. And if PS does not apply, it is not responsible for Betty's epistemic conversion to believing not-p.

Thus, PS is not responsible for Mary's epistemic martyrdom.

[12] Note that Betty's judgment does not seem to involve violation of the principle that David Christensen calls *independence*, because she is not downgrading her evaluation of Alf based on her reasoning that led her to believe p. David Christensen, "Disagreement as Evidence: The Epistemology of Controversy," *Philosophy Compass* 4 (2009): 758.

DISCUSSION NOTES

A PUZZLE FOR DOGMATISM

Mark MCBRIDE

ABSTRACT: I want to consider a puzzle in the realm of confirmation theory. The puzzle arises from consideration of reasoning with an argument, given certain epistemological commitments. Here is the argument (preceded by the stipulated justification for the first premise):

(JUSTIFICATION FOR 1) The table looks red.

(EK) (1) The table is red.

(2) If the table is red, then it is not white with red lights shining on it.

(3) The table is not white with red lights shining on it.

(EK) – the easy knowledge argument – has received much epistemological scrutiny of late. My aim, in this discussion note, is to set out an example, leading to the puzzle, putatively troubling for *dogmatism*. The puzzle takes the form of a pair of arguments which I take to be extractable from the recent work of a number of prominent epistemologists. My aim is modest: I seek not novelty, but rather merely to tie together accessibly some interesting recent work towards the formal end of epistemology which bears on cruxes at the heart of traditional epistemology.

KEYWORDS: dogmatism, perceptual justification, perceptual knowledge

0.1 I want to consider a puzzle in the realm of confirmation theory. The puzzle arises from consideration of reasoning with an argument, given certain epistemological commitments. Here is the argument (preceded by the stipulated justification for the first premise):

(JUSTIFICATION FOR 1) The table looks red.

(EK) (1) The table is red.

(2) If the table is red, then it is not white with red lights shining on it.

(3) The table is not white with red lights shining on it.

© LOGOS & EPISTEME II, 2 (2011): 295–302

(EK) – the easy knowledge argument – has received much epistemological scrutiny of late.[1]

0.2 The plan: First, I set out the epistemological commitments in play. Second, I set out an example, leading to the puzzle, putatively troubling for *dogmatism*. Finally, I consider the implications of the puzzle for dogmatism.

I. Epistemological Commitments

I.1. Suppose that the *mere having* of the experience described in (JUSTIFICATION FOR 1) can give one defeasible perceptual *justification*[2] to believe (1) – that is, it is the subject's having the experience, rather than the subject's beliefs about the experience, that makes it epistemically appropriate for the subject to believe (1).

---

[1] See, notably, Stewart Cohen "Basic Knowledge and the Problem of Easy Knowledge," *Philosophy and Phenomenological Research* 65 (2002): 309-29, and "Why Basic Knowledge is Easy Knowledge," *Philosophy and Phenomenological Research* 70 (2005): 417-30. The following structurally similar argument has also received much scrutiny (see, notably, Martin Davies, "Epistemic Entitlement, Warrant Transmission and Easy Knowledge," *Aristotelian Society Supplementary Volume* 78 (2004): 213-45, and "Two Purposes of Arguing and Two Epistemic Projects," in *Minds, Ethics, and Conditionals: Themes from the Philosophy of Frank Jackson*, ed. Ian Ravenscroft (Oxford: Oxford University Press, 2009), 337-83, James Pryor, "What's Wrong with Moore's Argument?," *Philosophical Issues* 14 (2004): 349-78, and "When Warrant Transmits," forthcoming in *Mind, Meaning and Knowledge: Themes from the Philosophy of Crispin Wright*, ed. Annalisa Coliva (Oxford: Oxford University Press), and Crispin Wright, "Facts and Certainty," *Proceedings of the British Academy* 71 (1985): 429-72, "Warrant for Nothing (And Foundations for Free?)." *Aristotelian Society Supplementary Volume* 78 (2004): 167-212, "The Perils of Dogmatism," in *Themes from G.E. Moore: New Essays in Epistemology and Ethics*, eds. Susana Nuccetelli and Gary Seay (Oxford: Oxford University Press, 2007), 25-48, and "Internal-External: Doxastic Norms and Defusing of Sceptical Paradoxes," *Journal of Philosophy* 105 (2008): 501-17):

> (JUSTIFICATION FOR 1) I am having a visual experience as of having hands.
> (MOORE) (1) I have hands.
> (2) If I have hands an external world exists/If I have hands I'm not a handless BIV having pseudo-perceptual experiences as of hands.
> (3) An external world exists/I'm not a handless BIV having pseudo-perceptual experiences as of hands.

The puzzle to come can, *mutatis mutandis*, be run apropos of (MOORE).

[2] 'Justification' is used in this paper as a broad term of epistemic appraisal and is interchangeable with 'warrant.'

And we might go further in claiming that this justification can suffice to give one *knowledge* of (1). This supposition and claim are distinctive features of dogmatist accounts of justification and knowledge respectively.[3] To refer specifically to dogmatism about justification I'll use 'j-dogmatism,' to refer to dogmatism about knowledge I'll use 'k-dogmatism,' and to refer to dogmatism generically I'll use 'dogmatism.' I take it the truth of k-dogmatism entails the truth of j-dogmatism; but the converse entailment does not hold.

Dogmatists are (necessarily?) *fallibilists* about knowledge: "[W]e can have knowledge on the basis of defeasible justification, justification that does not *guarantee* that our beliefs are correct."[4] It's the defining feature of dogmatism that the justification one gets for (1) is *immediate*: you don't need antecedent justification for any other propositions in order for the having of the experience described in (JUSTIFICATION FOR 1) to give one justification for (1). Some find dogmatism an appealing way to think of perceptual justification and knowledge. So let's suppose, *pro tem*, we're fallibilists, in this dogmatist sense.[5]

I.2 At a highly general level, it seems that dogmatists must give some account of the defectiveness of (certain instances of) reasoning by means of (EK). Why so? Here's the worry: On a dogmatist view, the mere having of a perceptual experience (giving justification for and, say, knowledge of, (1)), combined with some elementary logical reasoning (via (2)), can seemingly lead us – *all too easily* – to knowledge of the falsity of certain sceptical hypotheses ((3)). Thus the *problem of easy knowledge*. Our ensuing puzzle for dogmatism may be viewed as a specific

[3] See James Pryor, "The Skeptic and the Dogmatist," *Nous* 34 (2000): 517-49, and "What's Wrong with Moore's Argument?"

[4] Pryor, "The Skeptic and the Dogmatist," 518. If one wants to frame fallibilism in terms of conditional probabilities (cf. Pryor, "Uncertainty and Undermining," available at http://www.jimpryor.net/research/papers/Uncertainty.pdf), one will claim that a subject, *S*, can know a proposition, *p*, when the probability of *p* conditional on *S*'s evidence, *e*, is less than 1. Note that conditional probabilities involve two propositions: one about the world, *p*, and one about the subject's evidence, *e*. But the subject does not have to believe the proposition about evidence in order to possess the evidence.

[5] This supposition keeps things manageable. Our puzzle assumes fallibilism. But note one can (see John Hawthorne, *Knowledge and Lotteries* (Oxford: Clarendon, 2004), 75-7) give a rendering of a similar puzzle on the assumption of *infallibilism* (fallibilism's negation).

way of framing a puzzle *in the region of* this worry using tools from confirmation theory.[6]

II. Example, Leading to the Puzzle, for Dogmatism

II.1 *Example*: Let us, for simplicity, consider only red tables and white tables,[7] and only red light and white (natural) light. Suppose that the prior probabilities are divided equally between red table (RT) (0.5) and white table (WT) (0.5) and in the ratio 1:2 between red light (RL) (0.33) and white light (WL) (0.67). So the prior probabilities of the four hypotheses (assuming the table colour and the light colour are independent) are: (RT&RL) 0.167; (RT&WL) 0.33; (WT&RL) 0.167; (WT&WL) 0.33.[8] Now I have a visual experience as of a red table. We know that the posterior probabilities of the four hypotheses are proportional to the product of the prior probability and the *likelihood* (that is, the probability of the evidence given the hypothesis). Keeping things simple, suppose that the probability of a table looking red is the same given (RT&RL), or given (RT&WL), or given (WT&RL). And suppose (idealising) that the probability of a table looking red given (WT&WL) is zero. Then the posterior probabilities are: (RT&RL) 0.25; (RT&WL) 0.5; (WT&RL) 0.25; (WT&WL) 0. So, given the evidence described in (JUSTIFICATION FOR 1), the probability of premise (1) [that is, red table with either red light or white light] is raised from 0.5 to 0.75; the probability of premise (2) is 1 because it is a priori true; and the probability of the conclusion (3) [~(WT&RL)] is decreased from 0.833 to 0.75. That is, the probability of the 'sceptical' hypothesis, (WT&RL), is increased from 0.167 to 0.25 (essentially because one of the hypotheses, (WT&WL), has been eliminated by the evidence and its share of the prior probability has been redistributed amongst the remaining three hypotheses).

---

[6] It is not clear to me *how similar* the ensuing problem for dogmatism is to the problem of easy knowledge. The more similar it is the more I might expect an answer to it to be found in some reconfigurement of my proposed solution to the problem of easy knowledge (Mark McBride, "Towards a Complete Solution to the Problem of Easy Knowledge," Unpublished paper). However, at this point, a method of implementing any such reconfigurement is not obvious to me.

[7] One could, making things more realistic, generate a similar example by considering, say, 10 (equi-probable) colours the table might be.

[8] Note: the prior probability assigned to the 'sceptical hypothesis,' (WT&RL), is low. It might seem like a *reasonable* prior, but it would not be acceptable to the (local) sceptic (cf. Wright, "The Perils of Dogmatism," and "Internal-External").

2.2 The foregoing worked example, though simplified and idealised, serves to support premise (iii) in the following argument against j-dogmatism, viz. getting (JUSTIFICATION FOR 1) diminishes the credence one ought to have in (3).[9] Similarly, the worked example serves to support premise (iii*) in the subsequent argument against k-dogmatism. Here, first, is the argument against j-dogmatism:

> (i) If one has justification to believe (1) after getting (JUSTIFICATION FOR 1), one has justification to believe (3) after getting (JUSTIFICATION FOR 1).
>
> (ii) If having a certain experience diminishes the credence one ought to have in a proposition then, if one has justification to believe the proposition after having the experience, one must have had justification to believe the proposition antecedently to the experience.

---

[9] In itself, that a piece of evidence disconfirms a hypothesis (known to be) entailed by a hypothesis which the evidence confirms is not problematic. Consider the following thesis (cf. Carl G. Hempel, "Studies in the Logic of Confirmation," *Mind* 54 (1945): 1-26, 97-121):

> (CC) If *E* confirms *H* and *H* entails *H´*, then *E* confirms *H´*.

Due to counterexample(s), however, we have good reason to reject (CC). Consider: *E* = card is black, *H* = card is the ace of spades, and *H´* = card is an ace. Clearly, *H* entails *H´* while *E* confirms *H* but not *H´*. Note the following weaker thesis, however:

> (CC*) If *E* confirms *H* and *H* entails *H´*, then *E* doesn't disconfirm *H´*.

The counterexample we considered to (CC) is not a counterexample to (CC*). Consider the following case, however:

> Suppose you start with its being 80% likely for you that Clio's pet is a dog. Then you're informed that Clio's pet has no hair. One effect of this information is to raise the likelihood that her pet is an American Hairless Terrier, which hypothesis entails that it's a dog. But the information also decreases the total likelihood that Clio's pet is a dog. It makes it more likely that she owns a fish or a bird. So: evidence can give you more justification to believe P than you had before, you can know P to entail Q, and yet your evidence make you less justified in believing Q than you were before. (Pryor, "What's Wrong," 350-1.)

Our *puzzle* for dogmatism, however, is (in part) generated by the fact that for dogmatists *getting (JUSTIFICATION FOR 1) alone* putatively justifies, or confers knowledge of, (1) (unlike, *mutatis mutandis*, the foregoing two cases).

(iii) Getting (JUSTIFICATION FOR 1) diminishes the credence one ought to have in (3).

(iv) Therefore, if one has justification to believe (1) after getting (JUSTIFICATION FOR 1), one must have had justification to believe (3) antecedently to getting (JUSTIFICATION FOR 1).

(v) Therefore j-dogmatism is false: (JUSTIFICATION FOR 1)'s ability to provide justification to believe (1) is *not* independent of whether one has antecedent justification to believe (3).[10]

The argument against k-dogmatism is similar:

(i*) If one knows (1) after getting (JUSTIFICATION FOR 1), one is in a position to know (3) after getting (JUSTIFICATION FOR 1).

(ii*) If having a certain experience diminishes the credence one ought to have in a proposition, then if one is in a position to know the proposition after having the experience, one must have been in a position to know the proposition antecedently to the experience.

(iii*) Getting (JUSTIFICATION FOR 1) diminishes the credence one ought to have in (3).

(iv*) Therefore, if one knows (1) after getting (JUSTIFICATION FOR 1), one must have been in a position to know (3) antecedently to getting (JUSTIFICATION FOR 1).

[10] I take something like this argument to be extractable from Roger White "Problems for Dogmatism," *Philosophical Studies* 131 (2006): 525-557, whose focus is specifically on j-dogmatism. Stephen Schiffer, "Skepticism and the Vagaries of Justified Belief," *Philosophical Studies* 119 (2004): 161-84, and Wright, "The Perils of Dogmatism." Note premises (i) and (i*) each rest on a *closure* principle – I explore this further in section 3. Note also premises (iii) and (iii*) can form the basis for an explication of the phenomenon of *transmission failure* (cf. Samir Okasha, "Wright on the Transmission of Support: a Bayesian Analysis," *Analysis* 64 (2004): 139–146, Jake Chandler, "The Transmission of Support: a Bayesian Re-analysis," *Synthese* 176 (2010): 333-43, and Luca Moretti, "Wright, Okasha and Chandler on Transmission Failure," forthcoming in *Synthese*). And note finally that if one added a further premise to these arguments that *we don't in fact have* justification to believe – *aren't in fact in* a position to know – (3) prior to experiencing (JUSTIFICATION FOR 1), one would have the makings of a full-fledged argument for scepticism.

> (v*) Therefore k-dogmatism is false: (JUSTIFICATION FOR 1)'s ability to confer knowledge of (1) is *not* independent of whether one is antecedently in a position to know (3).[11]

Note that this second argument contains the locution 'in a position to know' at several junctures. I take it that one is in such a position just in case[12] one has (evidential) justification for the true proposition in question, and some anti-luck condition is fulfilled thwarting *Gettierisation*. Admittedly this account is vague and context-dependent at a number of points,[13] but this working definition will do for our purposes.

I take it that, with these two arguments, we've fingered *the* major *puzzle* in confirmation theory for dogmatism. They purport to establish, *contra* dogmatism, that the role of a perceptual experience (of the table looking red) in providing justification to believe (1), and ultimately knowledge of (1), depends on an antecedently available justification to believe (3), or on being antecedently in a position to know (3). Each argument has three premises. Unless there is some flaw in the reasoning that takes us from the three premises to the interim conclusion, and thence to the conclusion, the dogmatist must finger a false premise. Each of the premises, however, is plausible.

III. Implications of the Puzzle

III.1 The arguments comprising our *puzzle* for dogmatism (see 2.2) are valid, so let's isolate a premise on which some doubt might be cast. An obvious move at this stage, given the apparent security of the second and third premises, is to flag premises (i) and (i*):

> (i) If one has justification to believe (EK1) after getting (JUSTIFICATION FOR 1), one has justification to believe (EK3) after getting (JUSTIFICATION FOR 1).

> (i*) If one knows (EK1) after getting (JUSTIFICATION FOR 1), one is in a position to know (EK3) after getting (JUSTIFICATION FOR 1).

---

[11] I take something like this argument to be extractable from Hawthorne, *Knowledge and Lotteries*, 73-5, whose (effective) focus is specifically on k-dogmatism. Cf. also Cohen, "Why Basic Knowledge is Easy Knowledge."

[12] I follow a standard philosophical practice of using 'just in case' as interchangeable with 'if and only if.'

[13] Cf. Timothy Williamson, *Knowledge and Its Limits* (Oxford: Oxford University Press, 2000), 95.

Each premise, respectively, presupposes (something like) the following (single-premise) closure principles:

> (*J-Closure*) If one has justification to believe P and can tell that P entails Q then – *ceteris paribus* – one has justification to believe Q.
>
> (*K-Closure*) If one knows P and competently deduces Q from P, thereby coming to believe Q, while retaining one's knowledge that P, one comes to know that Q.

A defender of j-dogmatism or k-dogmatism wanting to question the truth of (i) or (i*) should offer reasons to reject (*J-Closure*) or (*K-Closure*), respectively.[14] But it's then noted that these are highly plausible closure principles. Thus dogmatism is – or seems very likely to be – false.[15]

[14] See Maria Lasonen-Aarnio, "Single Premise Deduction and Risk," *Philosophical Studies* 141 (2008): 157-73, for an interesting exploration of so-called *deductive risk* – a phenomenon which provides a novel basis for questioning (*K-Closure*). Elsewhere (Mark McBride, "Is Knowledge Closed Under Known Entailment? The Strange Case of Hawthorne's 'Heavyweight Conjuct' (and Other Strange Cases," *Theoria* 75 (2009): 117-28, and Mark McBride, Lee Walters, "Sensitivity and Closure," Unpublished paper), I have also questioned (*K-Closure*).

[15] This work drew heavily on conversations with Martin Davies and Cian Dorr, who between them provided much of the material for this paper.

Reviews

Juan Manuel Torres (ed.): *On Kuhn's Philosophy and its Legacy*, Lisboa: Cadernas de Filosofia das Ciências da Universidade de Lisboa, CFCUL, 8, 2010

Reviewed by Dan Chițoiu*

The volume coordinated by Juan Manuel Torres is the eighth in the series of *Cadernas de Filosofia das Ciências* (*Notebooks of Philosophy of Science*) edited by the Center for Philosophy of Science, University of Lisbon. It includes studies on Thomas Kuhn's thought and its present influence on philosophy, history of science and science. It contains, among other themes, neglected or forgotten areas such as the influence of the Kuhnean doctrine on the dynamics of change in biology, the structuralist view of theories and Friedman's Kantian ideas.

In spite of the general agreement that the Kuhnean theses were obtained in the philosophical field, there still are controversies about how they should be understood. In order to verify this remaining problem, it is enough to mention that two doctrines so different in methods and perspectives, such as the strong program by David Bloor and the structuralist view of theories by Wolfgang Stegmüller, point to Kuhn as a decisive antecedent of their own views. This volume contains developments and perspectives of Kuhn's philosophy providing answers to the question: what is the right analysis for his legacy?

Here are some of the perspectives presented in the volume:

Antonio Bereijo (University of La Coruña), in "Kuhn's Influence on the Sciences of the Artificial: Analysis of the Repercussions on Information Science," asserts that Thomas Kuhn has exerted his influence in an area that he did not consider explicitly: the Science of Artificial. His philosophical and methodological proposals have influenced the field of Information Science understood as a Design Applied Science. Indeed, there is an interest aroused by Kuhn's thought in areas not explicitly considered by the author of *The Structure of Scientific Revolution*. This implies that one accepts the general character of his philosophical and methodological proposal, which in that case would be valid for talking about the

* ACKNOWLEDGEMENT: This paper was made within *The Knowledge Based Society Project* supported by the Sectorial Operational Program Human Resources Development (SOP HRD), Financed by the European Social Fund and by the Romanian Government under the contract POSDRU/89/1.5/S/56815.

artificial. This means that his proposal can be legitimately used for Applied Sciences, disciplines where – as in Information Science – goals, processes and results are involved. Consequently, their approach is given some degree of validity in relation to any empirical Science, including Applied Sciences directly related with information and documentation. Secondly, there is the issue of how the authors of Information Science have understood the philosophical and methodological approaches of Kuhn, concerning both the structural level (the 'paradigms,' 'disciplinary matrix,' etc.) and the dynamic aspect ('normal science,' 'revolutionary science,' etc.). This is the key to asserting their degree of influence in Information Science, especially as it has been relatively common to give a different interpretation of the Kuhnian texts that the genuine thought of their author. After considering the legitimacy of Kuhn's schemes and the question of how Kuhnian categories are interpreted – on the structural and dynamic levels – a third aspect must be considered: the projection of his approach that is, how Kuhn's characterization is used in order to reinterpret Information Science activity.

Those who accept this projection assume that the structural Kuhnian categories, designed especially for Basic Science of Nature, serve to illustrate the scientific development of the artificial, thus contributing to a Design Applied Science such as Information Science. Also, by incorporating the Kuhnian philosophical and methodological approaches, the dynamic aspect can be understood from the perspective of the historicity of the scientific activity, using notions such as 'paradigms,' 'disciplinary matrix,' etc. Thus, a Design Applied Science, such as Information Science, which was born through the 'scientification' of professional practice, could be seen from a Kuhnian perspective. The authors considered by Antonio Bereijo – Francis Miksa, Rafael Capurro and Birger Hjørland – assume *de facto* the legitimacy to sustain the artificial and applied field since they understand that Kuhn's philosophical and methodological approaches can cover the various aspects involved.

In the study "Appropriating Kuhn's Philosophical Legacy. Three Attempts: Logical Empiricism, Structuralism, and Neokantianism," the authors – Andony Ibarra and Thomas Mormann (University of the Basque Country) – discuss three examples of the appropriation of Kuhn's ideas in the philosophy of science. They consider as a first example the classical logical empiricism. Perhaps, somewhat surprisingly, Carnap considered Kuhn's socio-historical account as a useful complementation, and not as a threat of the philosophy of science for the logical empiricism. The second example in this respect is the attempt of the so-called structuralist philosophy of science to provide a 'rational reconstruction' of Kuhn's approach. Finally, the authors discuss Friedman's proposal to apply Kuhn's ideas to

the formulation of a modernized, historically enlightened Kantian approach based on the concept of a non-apodictic constitutive and historically moving *a priori*. The authors conclude that even if there is no unanimously agreement upon Kuhn's legacy for the philosophy of science, at the very minimum one may say that Kuhn played a pre-eminent role in the endeavor of reminding the philosophers of science the indispensable role of history for understanding scientific rationality. However, as many different attempts of appropriating Kuhn's ideas show, it is far from clear how this role for history is to be conceived. It may well be the case that this problem has no unique solution, and certainly that the three proposals that have been discussed here will not be the last words on this issue.

In "Retrieving Axiological Incommensurability" Ana Rosa Pérez Ransaz (Instituto de Investgationes Filosóficas, UNAM) assumes as a starting point the 'lack of a common standard of measurement' among rival theories, which she considers to be the hard core of Kuhn's idea of incommensurability. Ransaz draws a distinction between *semantic* (or more properly, *onto-semantic*) *incommensurability* and *axiological incommensurability*, in order to show that this distinction makes it possible to reconstruct the process of theory choice with greater precision, thus making visibile some ways of reaching consensus so far little explored in the philosophy of science. While in the 1970's Thomas Kuhn restricted the analysis of incommensurability to its semantic dimension, anchoring it in the phenomenon of conceptual change, Ransaz considers that it is worth recovering the axiological dimension, which has to do with the different relative weights given to the epistemic values shared by a scientific community. She argues that the both dimensions of incommensurability form the basis for a genuine epistemological pluralism, foreign to sterile relativism.

Another interesting paper by Linda van Speybroeck and Danny Da Waele (Ghent University/FWO Flanders), entitled "Paradigm Lost? Scrutinizing the Veracity of Systems Biology's Paradigm Shift," discusses the application of the Kuhnian 'paradigm shift' to the current developments in the biological sciences. *In casu*, systems biology is promoted as representing a paradigm shift in the study of living organisms. After introducing systems biology's practical ins and outs the authors analyze how its identity is constructed by claims stressing what is 'at the heart' of systems biology and what it purports to 'revolutionize.' These claims indicate that the envisaged paradigm shift of systems biology is about 'going beyond' molecular biology. The veracity of this paradigm shift rests on the perception of molecular biology as being atomistic and gene-centered. As this perception appears to be highly questionable, Speybroeck and Waele conclude

that systems biology forms an evolution within the range of normal science, rather than a truly Kuhnian paradigm shift. That today Kuhn's legacy is handled loosely, and that an inappropriate use of the term 'paradigm shift' may indicate a 'accent shift' in the historiography of a scientific discipline, is hereby demonstrated. This leads to question whether the very concept of paradigm is outmoded to capture the dynamics in current biological sciences, so the authors ask a further question: is Kuhn's paradigm 'lost'?

Carlos Gustavo Wolff Neto, in "Incommensurability without Paradigms: the Epistemological Revolution of Thomas Kuhn," focuses on how Thomas Kuhn's thoughts regarding his original proposal and its embracement were modified. Some important aspects are discussed: the terminological redimension and the subsequent abandon of the concept of paradigm; the structure of scientific community that, it the end, is characterized by taxonomy and lexical structure shared by its members; the scientific revolutions that are not considered abrupt events anymore, as they were considered in the beginning; the incommensurability of scientific theories – where questions related to translation are opened to discussion of the language philosophy – becoming, eventually, locally delimited. In their analysis of Kuhn's trajectory, the authors note his movement from the history of science to epistemology and ontology, providing him with a self-definition as a 'post-Darwin Kantian.' The article concludes with a comparative table between Kuhn of *The Structure of Scientific Revolution* until the 1980's and Kuhn of this decade (Kuhn from *The Road Since Structure*).

Other contributions are signed by Juan Ernesto Calderón (Universidad Nacional de Cuyo, CONICET), Sandra Laugier (Université de Picardie Jules Verne, France), Raúl A. Milone (Universidad Nacional de Cuyo – Mendoza, Argentina), Hassan Tahiri (Universidade de Lisboa – Centro de Filosofia das Ciéncias), Juan Manuel Torres (Universidad Nacional de Cuyo – Argentina), Erik Weber and Dunja Šešelja (Centre for Logic and Philosophy of Science, Ghent University).

J.R. Croca and J.E.F. Araújo (eds.), *A New Vision on Physis. Eurhythmy, Emergence and Nonlnearity*, Lisaboa: Center for Philosophy of Science University of Lisbon, 2010

Reviewed by Gerard Leonid Stan*

The study of ontology and of the theory of scientific knowledge has been radically changed by the massive theoretical accumulations in 20th century physics. Gradually, the pure speculation was replaced by a systematic, synthesising and integrative reflection on the most relevant results of physics and other natural sciences.

The volume *A New Vision on Physis. Eurhythmy, Emergence and Nonlnearity* published by the members of the Center for Philosophy of Science University of Lisabon is inscribed in the trajectory of a philosophy of nature seeking to integrate, unify and resignify knowledge offered by present-day physics. The authors see their endeavour as a *Manifesto* for a new way of looking at natural phenomena. The concepts they use and the formal apparatus they propose are seen as instruments capable of constructing an alternative to the traditional, Cartesian, linear method of looking at nature and its phenomena. The approach they advance is a global nonlinear picture of the natural world. They start from the idea that the natural phenomenon, considered as a whole, is altogether different from the sum of its parts; the emerging entities behave in a way that cannot be inferred from the properties of the component parts. The mathematical formulation of this new approach is consonant with the principle of Eurhythmy.

The first paper in the volume, J.R. Croca's *Hyperphysis, The Unification of Physics*, provides the philosophical, conceptual and general-formal framework for this new approach of natural phenomena. The framework he proposes does not aspire only to be conducive to a unification of physics, but also to a clearer and deeper understanding of physical reality. On the basis of the proposed nonlinear approach, guided by the principle of Eurhythmy, quantum and relativistic physics can have a unitary and causal description. The unity of the world should be reflected in unitary explanations and descriptions. According to J. R. Croca, this

* ACKNOWLEDGEMENT: This paper was made within *The Knowledge Based Society Project* supported by the Sectorial Operational Program Human Resources Development (SOP HRD), Financed by the European Social Fund and by the Romanian Government under the contract POSDRU/89/1.5/S/56815.

aspiration of physics towards unity can be traced to the principle of Eurhythmy and to several other fundamental assumptions, shared by all researchers taking part in this reconstructive effort. The principle of Eurhythmy comes from the Greek word *euritmia*, meaning "the adequate path, the good path, the good way, the right way, the golden path." (p. 5) This principle tells us something essential about the general tendency of complex entities of persisting in their existence.

According to J.R. Croca, the unification of physics can be accomplished if it is based on the following five assumptions: (1) The metaphysical principle of realism: "there is an objective Reality. This is observer-independent, yet, it is understood that the observer interacts with the very same reality being able to change it and of course of being changed in a greater or lesser degree." (2) The postulate of the existence of a subquantum medium: "there is a basic physical natural chaotic medium named the subquantum medium. All physical processes occur in this natural chaotic medium." (3) The postulate of the existence of physical entities: "what are called physical entities that is, the particles, fields and so on, are more or less stable local organizations of the basic chaotic subquantum medium." (4) The postulate of organisation: "in general the complex particles, stable organizations of the subquantum medium, are composed of an extended region, the so called theta wave, and inside it there is a kind of very small localized structure, the acron." (5) The principle of eurhythmy: "the acron inside the theta waves follows a stochastic path that in average leads it to the regions were the intensity of the theta wave field is greater." (p. 9) As J. R. Croca explains, the principle of Eurhythmy essentially postulates that "the acron possesses a kind of extended sensorium, its theta wave, with which it feels the surrounding medium." (p. 11)

Based on these principles, the author goes on to elaborate sets of equations capable of describing complex particles (the concept of complex quantum particle is extended from quantum physics to the entire physics), the stability of a particle, the movement of an acron, the speed of an acron in different fields of Theta waves, the movements of several acrons. Eventually, he deduces a fundamental equation of nonlinear evolutions. The *Conclusion* emphasises the fact that all these formal developments are only a preliminary version of ongoing research. The final purpose of this paper would be to describe adequately the complex interactions between nonlinear phenomena. This would require devising an entire language, *i.e.* a new branch of mathematics which would assume from the beginning the deep interdependence of physical systems.

The first paper of the volume, which also has a programatic role for the entire research project undertaken by the members of the Center for Philosophy

of Science University of Lisabon, is followed by a series of investigations consisting of computer simulations of the behaviour of a single particle ('acron') inside a Theta wave, as well as of the interactions between this type of particles (Mário Gatta), the description of elementary nonlinear mechanics for localised fields (Amaro Rica da Silva), the study of the symmetry generated by the solutions to Schrödinger's nonlinear equation (Amaro Rica da Silva), presenting a mathematical solution to a nonlinear equation for a particular idealised case (J.E.F. Araújo). João L. Cordovil's paper attempts to indicate a complete set of *a priori* principles of natural philosophy, more precisely, a set of propositions concerning the nature of physical objects. However, principles such as "All physical objects have the same nature," "All physical objects are quantum objects," "A physical object is a wavebody," or "All physical reality is subquantum medium" (pp. 250-254) are rather hard to accept as 'evident truths' of natural philosophy. There are enough arguments preventing us from considering any physical object as a quantum object. If we consider this equivalence as improper, we can explain neither its concrete structure, characteristic to the level of reality it belongs to, nor the laws governing it. A quantum understanding of nature as a whole gives us a picture of homogeneity, uniformity and unity. But the world we see is heterogeneous, organised on relatively autonomous levels, with phenomena that are essentially irreductible to quantum phenomena.

The papers in the final part of the volume present the crisis in the natural sciences, drawing attention to the evolutional processes seen as the result of a weak teleology (a consequence of the principle of Eurhythmy) (Rui Moreira), apply this principle to different sciences, trying to explain the increasing degree of order and complexity encountered in nature (Gildo Magalhães), discuss the concept of emergence and the origins of the nonlinear mode of understanding natural phenomena (G.C. Santos), present several theses meant to contribute towards a new natural philosophy (P. Alves) or offer a discussion of the concept of time seen as one of the fundamental concepts necessary for the understanding of physical reality (J.R. Croca and M.M. Silva).

Stephen Hawking confessed that when he published *A Brief History of Time. From the Big Bang to the Black Holes*, somebody told him that every equation he included in the book would scare off half of its potential readers. He didn't neglect this piece of advice and included only Einstein's famous equation $E=mc^2$. Taking into account the number of equations in *A New Vision on Physis*, and assuming the advice Stephen Hawking received was correct, the readers of this volume cannot be very numerous. Those who will really benefit from reading this book are the physicists interested in the philosophical signification their

theories hold and in the idea of a unitary vision which could be found beyond the diversity of natural phenomena and theories of physics. Most of the potential readers with a philosophical background might have real difficulties in following the mathematical framework proposed for the description of natural phenomena. On the other hand, they would seriously doubt the real philosophical value of these mathematical developments. Also, philosophers would have been much more interested in more explicit arguments in favour of the unity of the physical world, of ontological reductionism (which is considered as self-evident by the authors), of epistemological reductionism or arguments for the possibility of devising a mathematical language which could provide a unitary description of natural phenomena. Moreover, we believe the reasons for which the authors formulated and included the principle of Eurhythmy should have been more thoroughly explained and also that it would have been necessary to evince the real problems solved through this principle and to emphasise more on the reasons for their belief in its productivity on a philosophical level.

As Pedro Alvarez warned in his paper (p. 369), before propounding a physical reconstruction of reality, we should analyse our powers to provide authentic knowledge of the physical world. In the absence of this preliminary critical analysis on the power of the sciences to provide knowledge, on the epistemic limitations of certain theories (like the quantum theory), we always risk replacing one set of classical dogmas with a new set of dogmas, an outdated perspective of the 'divine eye' with an updated one.

Robert B. Talisse, *Democracy and Moral Conflict* (Cambridge: Cambridge University Press, 2009).

Reviewed by Viorel Ţuţui

In the book *Democracy and Moral Conflict*, Robert B. Talisse addresses one of the most important subjects in contemporary political philosophy: the problem of the legitimacy of democratic decisions in the context of the pluralist societies. He develops an epistemic theory of democracy which is supported by an authentic and detailed epistemological foundation that makes it a significant contribution not only for political philosophy, but also for contemporary epistemology.

The book is structured in five chapters: "The Problem of Deep Politics," "Against the Politics of Omission," "Folk Epistemology," "Justifying Democracy," and "Epistemic Perfectionism." In one of the most systematic and unitary accounts on this subject I know of, he presents the significance of the problem he wants to address, rejects the main alternative solutions, develops his own view on the matter and responds to some significant objections that could be raised against his theory. However, after a brief presentation of the main theses he defends, I will mention some objections that, in my opinion, still affect his view.

The first chapter starts with the analysis of what he believes to be the legitimacy crisis in modern democracies: citizens are devoted to different values they take to be fundamental and hence non-negotiable and they are not willing to bargain these values in order to reach a common and legitimate political decision. Moreover, a democratic regime that would constrain them to abandon this commitment would be regarded as illegitimate and they could rightfully adopt against it one of the following four strategies: *relocation*, *rebellion*, *civil disobedience* and *petition*. Since only the last two strategies are democratic, the main problem to be solved is how we can justify the thesis that every citizen should prefer democratic to non-democratic strategies, without appealing to the Hobbesian response to this problem according to which one should always sustain democracy because the costs of the non-democratic strategies would be too high. This is what he calls "the problem of deep politics." (pp. 36-38)

The first solution to the problem of deep politics that Talisse analyzes and rejects is represented by the doctrine of proceduralism, a theory according to which democracy is based on a fair aggregative voting procedure. The procedure assures every citizen an equal right to participate and cast his vote in conformity with his interests and preferences and with his comprehensive doctrine. The

decisions are established by the majority rule on which some constrains are placed in order to avoid the tyranny of majority.

However, Talisse argues that, in spite of its intuitive plausibility, this theory presupposes that citizens are willing to view their commitments as *fungible* items that can be exchanged and bargained with. But, he believes that people are not capable of adopting this kind of attitude with reference to their commitments. On the contrary, they tend to see them as non-negotiable, non-quantifiable, and not fungible. So, the parties in the conflict over ultimate values could reject the procedural framework and choose the non-peaceful alternative. (pp. 27-31)

The second chapter of Talisse's book concentrates on another important contemporary solution to the problem of deep politics: the theory of public reason developed by John Rawls. This is what Talisse calls a "freestanding political theory," according to which the comprehensive disagreements could be solved if the legitimate decisions would be established by an overlapping consensus between the defenders of different comprehensive doctrines: everyone will support the decision for reasons that are specific to his own comprehensive doctrine. But, if such an overlapping consensus is to be possible, then the decisions must be only *compatible* with all those different comprehensive views, but they should not presuppose any one of them in particular. So, in supporting a certain policy, citizens must not appeal to their religious, moral, and philosophical convictions. They have to adopt the principles of "public reason": they should explain the basis of their actions to one another in terms that others might endorse as consistent with their freedom and equality. This restricts the political reasoning by not allowing citizens to consult their moral, philosophical, and religious conceptions, and by constraining them to use only those arguments that could reasonable be accepted by everyone. This is the reason why Rawls defended a "politics of omission" which consists in the following two rules: the subjects that are especially divisive are not admitted in the political debate, and the terms of deliberation should not depend upon particular comprehensive principles.

In Talisse's view, the main problem of this conception of public reason is that it excludes *reasons* associated with the different comprehensive doctrines. It does not recognize those reasons *as* reasons even if an irrefutable proof of those doctrines would be provided. And he adds: "this is due to the fact of reasonable pluralism, which has it that a sound demonstration of x is insufficient for a proof of the falsity of all views inconsistent with x." (p. 55)

To those who will want to deny that it is possible to develop a decisive argument in favor of any specific comprehensive doctrine, Talisse responds that such a thesis would presuppose a commitment to moral skepticism. But moral

skepticism is as controversial and contestable as any other moral doctrine and it does not represent a "freestanding response to the problem of deep politics." And if we will assume the view, supported by Bruce Ackerman and Charles Larmore, according to which the omission is justified only conversationally (people should restrain from saying anything about the deepest moral disagreements), then, in Talisse's opinion, this would presuppose a commitment to the general subordination of the epistemic to the political. (pp. 50-51)

In the third chapter he develops his theory of *dialogical democracy* based on his view regarding folk epistemology. He distinguishes his view from the contemporary theories of deliberative democracy according to which the democratic decisions are established by a process of public deliberation that provides a moral basis for democracy: the fact that citizens should be treated as autonomous citizens who take part in the governance of their own society. Talisse believes that there is a general problem with all the moral versions of deliberative democracy: the moral ground from which such views begin is always controversial, so any such conception of the deliberative process will strike some citizens as inappropriate, unfair or 'rigged' to favor some political outcomes. (p. 129) This is the reason why he thinks that these moral conceptions beg the question posed by deep politics: they do not provide reasons for deeply divided citizens to sustain their democratic commitments, but they address only the citizens that are already committed to deliberative democracy.

In the attempt to avoid these problems, Talisse argues for an epistemic version of deliberative democracy that is not based on some controversial moral principles, but on a set of epistemic principles. In his opinion there is an epistemic analogue to the folk psychology from the philosophy of mind: folk epistemology. He mentions five principles of folk epistemology: 1) To believe some proposition *p* is to hold that *p* is true; 2) To hold *p* true is generally to hold that the best reasons support *p*; 3) To hold that *p* is supported by the best reasons is to hold that *p* is *assertable*; 4) To assert that *p* is to enter into a social process of reason *exchange*; and 5) To engage in a social process of reason exchange is to at least implicitly adopt certain cognitive and dispositional norms related to one's epistemic character. (p. 87-88) Hence, the commitment to democracy is based on our general commitment to the five principles of what is to properly believe something (according to our folk epistemology). Talisse affirms that these principles are implicit in the ordinary practice of political discourse of rational creatures and that this commitment entails a further commitment to democratic political norms and institutions: to what he calls "dialogical democracy."

In the fifth and final chapter of his book Talisse addresses some objections that could be raised against his theory of dialogical democracy: the problem of the ignorance of the citizens, the problem of uninterested citizens, and the problem of discursive failure. All these problems have in common the idea that dialogical democracy is too demanding a theory: it asks too much from ordinary citizens by insisting on the fact that they must be epistemically capable of rational discourse on complex subjects like those concerning the political life of a community. However, these objections underline the fact that citizens are ignorant, uninterested or manipulated, and therefore they do not posses the necessary epistemic capabilities.

Talisse's general response to these objections is that the ignorance and the discourse failure are caused by deficient democratic institutions and by "pseudo-deliberations" which need to be criticized and repaired, and not by the irremediable incompetence of the citizens. (pp. 159-167) So, in his opinion there are no serious doubts regarding the citizens' ability to engage and perform the relatively difficult epistemic tasks presupposed by his theory of dialogical democracy.

In what comes next I will try to argue that, despite his elaborate and systematic argumentation, there are some objections that could be raised against his theory. I will start by pointing out the fact that, in my opinion, his objection against proceduralism is *implausibly strong*: it could be used to reject not only the justification of democracy, but also the justification of any other peaceful way of solving deep comprehensive disagreements. Any democratic or non-democratic political and social framework that would presuppose the slightest compromise from the part of the defender of a moral or religious view could be rightfully rejected by him: he could always prefer open war. And, if this would be the case, then we might have to settle for a more modest epistemological project: to provide a justification for democracy that will convince only those citizens that already prefer a peaceful way of dealing with the deep moral and religious commitments. But, if this would be true, then Talisse's objection against proceduralism would lose its force: a fair procedure could be, in principle, as good as any peaceful procedure of solving moral and religious conflicts.

Another objection could be raised against his critique of the theory of public reason. I don't see how "the fact of reasonable pluralism" would constrain us to exclude reasons associated with the comprehensive doctrines even in the case in which an irrefutable proof of these views would be provided. An irrefutable proof is, by hypothesis, a proof which would be recognized as such by every citizen regardless of the comprehensive doctrine he favors. In my opinion,

the author who defends reasonable pluralism will not affirm that a sound demonstration of *x* is insufficient for a proof of the falsity of all views inconsistent with *x,* as Talisse suggests, but rather that no sound demonstration of that particular thesis is available: for example a pro-choice thesis on the subject of abortion is not capable of convincing everybody. Hence, this theory does not subordinate the epistemic to the political: the pragmatic decision to restrain from the public debate on those subjects on which there are some deep moral disagreements intervenes only when no epistemically sound proof is available.

And if the two objections I mentioned above are right, then Talisse didn't really succeeded in proving that his theory is the best available solution to the problem of deep politics. However, I believe that these are not the only or the most important problems of his argumentation. I think that the following objections underline the fact the main theses he defends are also problematic. First, if we analyze his view according to which our commitment to democracy is based on our commitment to the principles of what is to properly believe something, I believe we should note that his theory confuses two dimensions of the reasoning process: the very *general*, *normative* and *formal* rules that govern any process of believing something, and the substantial epistemic standards that prescribe what are the conditions of the *correct* beliefs. We could admit that the five epistemic principles of folk epistemology help us understand if we could speak about the *existence* of a belief in a particular case, but they do not specify the epistemic standards of the *correctness* of that belief. They say only if a belief exists and not if it is the correct belief. This latter task is accomplished by substantial epistemic standards that specify how we can reason in a correct manner, what is an argument, which arguments are the most compelling, and so on. But, these standards of the correct reasoning are not established from a first-personal epistemic point of view, as Talisse suggested, but from an intersubjective epistemic perspective. So, the concept of correctly believing something presupposes the concept of the proper social epistemic activity like that associated with democracy. In this case, we could say that deliberative democracy is not based on folk epistemology, but that things are the other way around.

Moreover, I believe that the theory of dialogical democracy doesn't solve the problem of deep politics either. The real issue behind this problem was to find an authentic motivation, for the followers of the comprehensive doctrines that always lose in the process of public debate, to adopt the democratic and not the non-democratic strategies mentioned above. But, even if it would be true that the defenders of two different comprehensive views (for example pro-life and pro-choice defenders) should adopt democratic strategies as long as their doctrines

could be recognized as the right ones by democratic means, as soon as they will realize that every such debate is unsuccessful they will lose the motivation to adopt democratic strategies. Therefore, on Talisse's own account, they could rightfully adopt non-democratic strategies like rebellion or relocation. Consequently, the problem of deep politics will stand.

Finally, regarding his response to the citizens' ignorance and discourse failure objections, I think Talisse would be right if only he would address the most important issue concerning these objections and especially the problem of discourse failure. He mentioned the fact that the expression of "discourse failure" was proposed by Guido Pincione and Fernando Teson, but he didn't analyzed what I think it is the most important argument they employ: the argument of "the rational ignorance" of ordinary citizens concerning political matters. In the view endorsed by Pincione and Teson, the state of ignorance is not always natural or caused by some epistemic deficiency of the deliberative procedures which are developed in contemporary democratic societies, as Talisse suggests. On the contrary, in their view, citizens often *choose* to remain ignorant on these political matters because they are aware of the high cost they have to face in order to become acquainted with reliable social science and they are aware of the fact that every individual vote is non-decisive on the outcome of an election. So, they would have to spend a great amount of resources although their vote would practically make no real difference. Therefore, their *rational* choice would be to remain ignorant. This is the one of the reasons why their opinions are usually wrong and they could become the victims of political manipulation.

But, if this is true, then the theory of dialogical democracy is indeed too demanding: it asks from the citizens of a democratic society to invest a great amount of resources in order to participate in a political process from which they will have very little to gain. And this would also reinforce the other two objections he addressed in the final chapter of the book: we could explain the public ignorance and the badly oriented political interest by the idea that this is the rational choice that ordinary citizens have to make.

Nevertheless, if we put aside these objections, Robert B. Talisse's book, *Democracy and Moral Conflict*, remains one of the most important attempts to solve the problem of democratic legitimacy in the context of the pluralism that characterizes modern society. Unlike many other contemporary epistemic conceptions of democracy, which settle for more modest objectives, Talisse's theory addresses the difficult task of offering a detailed epistemological explanation of what is the epistemic foundation of democracy and how it is supposed to work.

NOTES ON THE CONTRIBUTORS

Alex Bundy is Ph.D. candidate at the University of California, Santa Barbara, Department of Philosophy, with a dissertation on "The Epistemic Significance of Belief." He received, in 2006, the Alfred B. Glathe Award at California State University, Los Angeles. While his main area of specialization is epistemology, his academic interests include ethics, philosophy of language, metaphysics, and philosophy of mind. He is the author of "On 'Epistemic Permissivism'" (with Anthony Brueckner), forthcoming in *Synthese*, 2011. Contact: bundy@umail.ucsb.edu

Richard David-Rus is researcher at the Institute of Anthropology *Francisc I. Rainer* of the Romanian Academy, Bucharest. He obtained his Ph. D. title at the University of Munich, with a dissertation entitled *Explanation and Understanding through Scientific Models. Perspectives for a New Approach to Scientific Explanation*. His main philosophical interests are in general philosophy of science, especially the topic of scientific explanation and understanding, and its problems in the field of biological and social sciences. He is also active as a statistician in the field of life sciences. Contact: rusdavid@gmail.com

Jeremy Fantl is Associate Professor at the Department of Philosophy, University of Calgary. His main area of specialization is epistemology. Other areas of interest are ethics, metaphysics and early British philosophy. He is the authors of two books: *Knowledge in an Uncertain World*, with Matthew McGrath (2009) and *Epistemology: An Anthology* with Ernest Sosa, Jaegwon Kim, and Matthew McGrath (2000, 2nd edition 2008) and of many articles among which "Is Metaethics Morally Neutral?" (*Pacific Philosophical Quarterly*, 2006), "Analysis of the *A Priori* and *A Posteriori*" (*Acta Analytica*, 2003), and "Modest Infinitism" (*Canadian Journal of Philosophy*, 2003). Contact: jfantl@ucalgary.ca

John F. Hendricks is Professor of Formal Philosophy in the Department of Media, Cognition and Communication/Section for Philosophy, University of Copenhagen, Denmark. Besides formal philosophy, he is interested in logic, epistemology, and philosophy of science. He is Director of *FLOG - The Danish Network for Philosophical Logic and Its Applications*, and founding editor-in-chief of *FNEWS – The Free Newsletter for Philosophical Logic and Its Applications* published by

Springer. His publications include *Mainstream and Formal Epistemology* (2006), *Thought2Talk: A Crash Course in Reflection and Expression* (2006), *The Convergence of Scientific Knowledge – a view from the limit* (2001). Contact: vincent@hum.ku.dk

Mark McBride is a Ph.D. candidate at the Faculty of Philosophy, Oxford University and a Ph. D. in Law at Cambridge University, Trinity Hall. His areas of specialization are epistemology, philosophy of mind, ethics, and legal philosophy. His publications include "Is Knowledge Closed under Known Entailment? The Strange Case of Hawthorne's 'Heavyweight Conjunct' (and Other Strange Cases)" (*Theoria*, 2009), "A Challenge for New Defenders of the Doctrine of Double Effect" (*Journal of Ethics & Social Philosophy*, 2010), and "Reply to Pardo: Unsafe Legal Knowledge?" (*Legal Theory*, 2011). Contact: markmcbride1978@gmail.com

Rhys Mckinnon is Ph. D. candidate and lecturer in the Department of Philosophy at the University of Waterloo, Ontario, Canada. His main areas of specialization are epistemology, philosophy of language, norms of assertion and rational decision theory. He is also interested in ethics, meta-ethics and metaphysics. His papers include "Giving Warrant Credit in Warranted Assertibility," presented to Amsterdam Graduate Philosophy Conference, Universiteit van Amsterdam, 2010 and "Rationalizing Non-Rational Choice Situations," presented to Canadian Philosophical Association, York University, 2006. Contact: rmckinno@uwaterloo.ca

Nicholas Rescher is Distinguished University Professor of Philosophy at be University of Pittsburgh where he is currently Chairman of the Center for Philosophy of Science. He wrote more than one hundred books ranging over all areas of philosophy (sixteen of them translated from English into eight other languages). Among these: *The Coherence Theory of Truth* (1973, 2nd edition 1982), *Dialectics: A Controversy-Oriented Approach to the Theory of Knowledge* (1977), *Satisfying Reason: Studies in the Theory of Knowledge* (1995), *Epistemology: On the Scope and Limits of Knowledge* (2003), and *Common Sense: A New Look at an Old Tradition* (2005), etc. Contact: rescher@pitt.edu

Gerard Leonid Stan is Associate Professor at the Faculty of Philosophy and Socio-Political Sciences, Alexandru Ioan Cuza University of Iaşi. As of April 2011 he is a Postdoctoral Fellow of the Romanian Academy. His academic interests include epistemology, philosophy of science and the cognitive foundations of

communication. He is the author of two books: *Knowledge and Truth* (2006, in Romanian) and *The Natural Order and the Laws of Science* (2004, in Romanian). Contact: gstan@uaic.ro

John Symons is Associate Professor and Chair of the Department of Philosophy at the University of Texas at El Paso, USA. His main interests are metaphysics, philosophy of mind, philosophy of science, and epistemology. His recent work focuses on the problem of emergence as it figures in traditional metaphysics and in the sciences. Since 2002 he is Editor-in-Chief of *Synthese. An International Journal for Epistemology, Methodology and Philosophy of Science*. His books include *On Dennett* (2002, translated into Slovak and French), *Logic and Reasoning* (forthcoming) and *The Science of Seeing: Vision after Marr* (forthcoming). He is also preparing a book called *Novelty: Laws, Models and Emergent Properties in Science.* Contact: jsymons@utep.edu

Zoltán Vecsey is a member of the Research Group for Theoretical Linguistics at the Hungarian Academy of Sciences, where he received, in 2007, the Bolyai Research Prize. His main academic interests are theoretical linguistics, semiotics, and epistemology. He is the author of *Names and Objects. On the Semantics of Proper Names* (2007, in Hungarian), the editor of: *Dimensions of Meaning. Modal Theories after Kripke* (2003, in Hungarian) and co-editor of: *Who was Sherlock Holmes? Studies on the Semantics of Proper Names* (2005), *Frege Today* (2006) and *Perspective and Meaning* (2010) – all of them in Hungarian. Contact: vecseyz@freemail.hu

LOGOS & EPISTEME
Aims & Scope

Logos & Episteme is a quarterly open-access international journal of epistemology that appears in printed and on-line version in March, June, September, and December. Its fundamental mission is to support and publish various current reflections and researches that aim at investigating, analyzing, interpreting or philosophically explaining the human knowledge in all its aspects, forms, types, dimensions or practices.

For this purpose, the journal will publish articles, reviews or discussion notes focused as well on problems concerning the general theory of knowledge, as on problems specific to the philosophy, methodology and ethics of science, philosophical logic, metaphilosophy, moral epistemology, epistemology of art, epistemology of religion, social or political epistemology, epistemology of communication. Studies in the history of science and of the philosophy of knowledge, or studies in the sociology of knowledge, cognitive psychology, and cognitive science are also welcome.

The journal intends to promote all methods, perspectives and traditions in the philosophical analysis of knowledge, from the normative to the naturalistic and experimental, and from the Anglo-American to the continental or Eastern.

The journal accepts for publication texts in English, French and German, which satisfy the norms of clarity and rigour in exposition and argumentation.

NOTES TO CONTRIBUTORS

1. Accepted Papers

The journal accepts for publication articles, discussion notes and book reviews submitted exclusively to *Logos & Episteme* and not published, in whole or substantial part, elsewhere. The editors of *Logos & Episteme* reserve the right to refuse all future papers belonging to the authors who fail to observe this rule.

2. Submission Address

Please submit your manuscripts electronically at: logosandepisteme@yahoo.com. Authors will receive an e-mail confirming the submission. All subsequent correspondence with the authors will be carried via e-mail. When a paper is co-written, only one author should be identified as the corresponding author.

3. Paper Size

The articles should normally not exceed 12000 words in length, including footnotes and references. Articles exceeding 12000 words will be accepted only occasionally and upon a reasonable justification from their authors. The discussion notes must be no longer than 3000 words and the book reviews must not exceed 4000 words, including footnotes and references. The editors reserve the right to ask the authors to shorten their texts when necessary.

4. Manuscript Format

Manuscripts should be formatted in Rich Text Format file (*rtf) or Microsoft Word document (*docx) and must be double-spaced, including quotes and footnotes, in 12 point Times New Roman font. Where manuscripts contain special symbols, characters and diagrams, the authors are advised to also submit their paper in PDF format. Each page must be numbered and footnotes should be numbered consecutively in the main body of the text and appear at footer of page. For all references authors must use the Humanities style, as it is presented in The Chicago Manual of Style, 15th edition. Large quotations should be set off clearly, by indenting the left margin of the manuscript or by using a smaller font size. Double

quotation marks should be used for direct quotations and single quotation marks should be used for quotations within quotations and for words or phrases used in a special sense.

5. Official Languages

The official languages of the journal are: English, French and German. Authors who submit papers not written in their native language are advised to have the article checked for style and grammar by a native speaker. Articles which are not linguistically acceptable may be rejected.

6. Abstract

All submitted articles must have a short abstract not exceeding 200 words in English and 3 to 6 keywords. The abstract must not contain any undefined abbreviations or unspecified references. Authors are asked to compile their manuscripts in the following order: title; abstract; keywords; main text; appendices (as appropriate); references.

7. Author's CV

A short CV including the author`s affiliation and professional address must be sent in a separate file. All special acknowledgements on behalf of the authors must not appear in the submitted text and should be sent in the separate file. When the manuscript is accepted for publication in the journal, the special acknowledgement will be included in a footnote on the first page of the paper.

8. Review Process

The reason for these requests is that all articles, with the exception of articles from the invited contributors, will be subject to a strict blind-review process. Therefore the authors should avoid in their manuscripts any mention to their previous work or use an impersonal or neutral form when referring to it. The review process is intended to take no more than six months. Authors not receiving any answer during the mentioned period are kindly asked to get in contact with the editors. Processing of papers in languages other than English may take longer. The authors will be notified by the editors via e-mail about the acceptance or rejection of their papers. The editors reserve their right to ask the authors to revise their papers and the right to require reformatting of accepted manuscripts if they do not meet the norms of the journal.

9. Acceptance of the Papers

The editorial committee has the final decision on the acceptance of the papers. Papers accepted will be published, as far as possible, in the order in which they are received and they will appear in the journal in the alphabetical order of their authors.

10. Responsibilities

Authors bear full responsibility for the contents of their own contributions. The opinions expressed in the texts published do not necessarily express the views of the editors. It is the responsibility of the author to obtain written permission for quotations from unpublished material, or for all quotations that exceed the limits provided in the copyright regulations. The papers containing racist and sexist opinions assumed by their authors will be rejected. The presence in texts of sexist or racist terms is accepted only if they occur in quotations or as examples.

11. Checking Proofs

Authors should retain a copy of their paper against which to check proofs. The final proofs will be sent to the corresponding author in PDF format. The author must send an answer within 3 days. Only minor corrections are accepted and should be sent in a separate file as an e-mail attachment.

12. Reviews

Authors who wish to have their books reviewed in the journal should send them at the following address: Institutul de Cercetări Economice şi Sociale „Gh. Zane" Academia Română, Filiala Iaşi, Str. Teodor Codrescu, Nr. 2, 700481, Iaşi, România. The authors of the books are asked to give a valid e-mail address where they will be notified concerning the publishing of a review of their book in our journal. The editors do not guarantee that all the books sent will be reviewed in the journal. The books sent for reviews will not be returned.

13. Property & Royalties

Articles accepted for publication will become the property of *Logos & Episteme* and may not be reprinted or translated without the previous notification to the editors. No manuscripts will be returned to their authors. The journal does not pay royalties. Authors of accepted manuscripts will receive free of charge two copies of the issue containing their papers.

14. Permissions

Authors have the right to use their papers in whole and in part for non-commercial purposes. They do not need to ask permission to re-publish their papers but they are kindly asked to inform the Editorial Board of their intention and to provide acknowledgement of the original publication in *Logos & Episteme*, including the title of the article, the journal name, volume, issue number, page number and year of publication. All articles are free for anybody to read and download. They can also be distributed, copied and transmitted on the web, but only for non-commercial purposes, and provided that the journal copyright is acknowledged.