

New Perspectives on Quine's “Word and Object”

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This special issue collects a selection of the papers presented at the International Colloquium *Word and Object, 50 Years Later*, which took place in Rome on May 28-29, 2010. In the fiftieth year since the publication of *Word and Object*, the conference aimed at celebrating one of the most famous and influential philosophers and mathematicians of the 20th Century: Willard Van Orman Quine. The purpose of the conference, organised by the University of Rome ‘La Sapienza’ and the Research Group APhEx (Analytical and Philosophical Explanation), was to discuss and explore some of the major Quinean theses. This volume collects the contributions of Marianna Antonutti Marfori, Jacob Busch & Andrea Sereni, Alberto Voltolini, Stephen White, and Giancarlo Zanet, who were speakers at the conference. The papers are unified by a common thread that is represented by the Quinean philosophical heritage and take their stance within the different areas of the current philosophical debate on this issue.

Quinean theses marked several fields of philosophy and, since its publication, *Word and Object* has become a landmark in the canon of analytical philosophy. During the two-day conference, at least three subjects came to the foreground, especially for their potential to still unveil, after half a century, some common prejudices in the philosophy of language. First, the thesis of indeterminacy of translation, which questioned the definition of the object of translation itself, the notion of a translation manual and the concept of translation equivalence. Second, Quine’s holistic view of language, which highlighted the problem of determining the locus (if any) and the function of the units constitutive of meaning. As is well known, Quine came to doubt the very notion of meaning on account of his holistic view.

Holism, the view that the sentences of a language have to face the “tribunal of experience” together, has still a deep influence on the philosophical understanding of the way language is connected to reality. Third, his thesis of the inscrutability of reference puzzled analytical philosophers on the nature of their ontological commitments, the criteria of identity of extension, the role of conceptual schemes, etc. and put into question the validity of the very notion of reference.

However, the Quinean contribution is not limited to the philosophy of language. His work sheds light on many important topics in the philosophy of mind and the philosophy of mathematics. He is celebrated for introducing a naturalised conception of epistemology, which emphasised the role of the natural scientific method in determining the processes of knowledge acquisition. On the one hand, this view brought him to embrace behaviourism, the thesis that psychological terms do not refer to inner mental states and instead are to be analysed in terms of speakers’ dispositions to verbal behaviour. On the other hand, he rejected the traditional philosophical study of scientific knowledge and mathematics in particular, because of its failure to reduce mathematics to pure logic.

The papers presented in this volume show that Quine’s philosophy is not yet a matter for history, but on the contrary has marked a path of inquiry still resourceful nowadays: his indispensability argument in mathematics, his critique of modal logic, his conception of naturalism and the ways it is applied in contemporary philosophy of mind, and his remarks on the notion of translation. Marianna Antonutti Marfori’s paper (‘Naturalising Mathematics: A Critical Look at the Quine-Maddy Debate’) focuses on the possibility of naturalising mathematical practice and offers a positive attempt to elucidate Penelope Maddy’s strategy for naturalising mathematics in a Quinean perspective. As it is known, according to Quine, mathematics is part of our best overall theory of the world because it is indispensable to scientific theories. Confirmational holism guarantees that indispensable mathematics is empirically confirmed in virtue of its successful application in scientific practice. This view entails the so-called Indispensability Argument (which seeks to establish the conclusion that we ought to believe in the existence of entities, i.e. mathematical entities, that are indispensable to scientific theories). According to this Quinean line of argument, it follows that unapplied mathematics has to be rejected as frivolous and we should adopt a strong revisionary

approach to mathematical practice. Maddy argues against such a position. An important motivation behind her view is to account for the methodological autonomy of mathematics. In her paper, Antonutti Marfori illustrates Maddy's position in detail in order to assess whether it can accommodate an anti-revisionary stance on mathematics within Quinean naturalism. More specifically, Antonutti explains Maddy's grounds for rejecting the conclusion of the Indispensability Argument while maintaining that mathematics is indispensable to science. She then shows how Maddy can avoid the objection that her view entails that mathematics and pseudo-science are on a par, and reformulates the objection so that it cannot be avoided. Finally, Antonutti argues that Maddy's view faces a dilemma, and ultimately fails to account for the methodological autonomy of mathematics within Quinean naturalism.

The paper by Jacob Busch and Andrea Sereni, ('Indispensability Arguments and their Quinean Heritage'), also focuses on philosophy of mathematics. It concerns the Indispensability Arguments for mathematical Platonism in connection with Quine's thesis. Quine's Indispensability Argument for mathematics is considered by many to be the strongest argument for mathematical realism: mathematical entities (i.e. sets, numbers, functions, etc.) are indispensable to our best scientific theories, therefore we ought to be ontologically committed to their existence. Bush and Sereni present some of the most discussed versions of the Indispensability Argument and show both theoretical and exegetical problems with Quine's view. They propose a different approach to indispensability that emphasizes the theoretical contributions of mathematics and Quine's remarks on unobservable entities. They suggest two minimal versions of indispensability, which they label the 'logical' and the 'theory-contribution' points of view. From the logical point of view, the notion of indispensability is understood in terms of the expressive power of theories: mathematics is indispensable to science because of nominalised theories. From the theory-contribution point of view, mathematical entities contribute to scientific theories in relevantly similar ways as theoretical entities do. The minimal version of indispensability they propose is an instance of the Inference to the Best Explanation.

Alberto Voltolini's paper ('All the existences that there are') concerns the question whether existence has to be taken either as a first-

order or as a second-order property. He proposes a three-fold notion of existence: as a second-order property, typically expressed by the *particular* quantifier; as a *substantive* first-order property of individuals, i.e. having a certain weight, being human, being Italian; and as a *blanket* property of individuals, i.e. the property of being (identical with) something. Voltolini explains what these properties are and why we need them all for our ontological purposes. Relying on this assumption, he vindicates a Meinongian position that endorses both first-order properties by giving arguments in favour of this view as the correct position in ontology; he further explores the limits of linguistic approaches to the ontology of existence, by means of a descriptive analysis of the behaviour of “there is” and “exists”.

In ‘The indeterminacy of translation: Fifty years later’, Stephen White offers a critical perspective on Quine’s thesis of indeterminacy of translation. White presents Quine’s well-known ‘Gavagai’ mental experiment and shows the difficulties that arise when we want to equate referring expressions or predicates in the language being translated and our own. In his thought experiment of radical translation, Quine proposes a situation in which both the linguist and the native speaker see a rabbit. Quine imagines that the native speaker pronounces ‘Gavagai’ in seeing the rabbit, and that the linguist notices this behaviour whenever the rabbit is present. In observing a strong correlation between the presence of a rabbit and the expression ‘Gavagai’ pronounced by the native speaker, the linguist will infer that ‘Gavagai’ means ‘rabbit’: ‘Gavagai’ is a one-word sentence with the same *stimulus meaning* as our sentence ‘There is a rabbit’. In fact, the meaning of a sentence as a stimulus to verbal behaviour is defined by what type of response it arouses in the native. Namely, stimulus meaning is a good approximation to meaning, as it is intuitively understood. However – Quine argues – if the linguist assigns the term ‘Gavagai’ to rabbits, it does not mean that the native could not use that term to refer to a ‘rabbit-stage’ or a ‘rabbit-phase’. If this were so, ‘Gavagai’ could refer, for example, to an ‘undetached rabbit part’. According to the Quinean thesis of inscrutability of reference, equally correct translation manuals might translate the same words using completely different references. Therefore, as White points out, the difficulty arises because we can associate ‘Gavagai’, construed as a term, with two very different terms in English and preserve the stimulus meaning of ‘Gavagai’ construed as a one-word sentence. Thus, White explores whether non-trivial examples of

indeterminacy are achievable by considering some objections to the appeal to verbal dispositions in characterising meaning and offers an alternative framework.

In 'Quine and the Contemporary Debate on Mindreading', Giancarlo Zanet explores the main features of Quine's account of mindreading within the current debate between theory-theorists, rationality-theorists, and hybrid-theorists. The concern is with the broad issue of how to characterize the way we understand people and how we attribute to them propositional attitudes (i.e. beliefs, desires, and intentions). Moreover, he points out that the role that folk psychology plays in Quine's philosophy is crucial. Such a role enlightens the difference between Quine and Davidson on rationality. According to Zanet, a theory of rationality can be captured in a Quinean perspective if we take him as a proponent of a kind of hybrid theory, a blend of theory-theory and simulation. In conclusion, he proposes a blend of rationality-based and hybrid view-based strategies to explain mindreading in a Quinean context.

We would like to conclude this brief introduction by thanking the authors of the papers collected in this volume for their willingness to cooperate during the whole review process. We also thank all the participants at the conference "Word and Object, 50 years later", the Organising Committee (Marianna Antonutti Marfori, Daniele Santoro, Stefano Vaselli, Pierluigi Graziani, Carlo Tataschiere, Stefano Poggi and the Italian Society for Philosophy) and the Scientific Committee (Massimiliano Carrara, Carlo Cellucci, Roberto Cordeschi, Mario De Caro, Leon Horsten, Teresa Marques, Marco Santambrogio, Celia Teixeira). We are grateful to the Editorial Committee of *Disputatio*, the editors Teresa Marques and João Branquinho, for all the support given to the publication of this volume. Last but not least, we profoundly appreciate the efforts of the referees in reviewing the papers. Without their help, this special issue would not exist.

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Naturalising Mathematics: A Critical Look at the Quine-Maddy Debate

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Abstract

This paper considers Maddy's strategy for naturalising mathematics in the context of Quine's scientific naturalism. The aim of this proposal is to account for the acceptability of mathematics on scientific grounds without committing to revisionism about mathematical practice entailed by the Quine-Putnam indispensability argument. It has been argued that Maddy's mathematical naturalism makes inconsistent assumptions on the role of mathematics in scientific explanations to the effect that it cannot distinguish mathematics from pseudo-science. I shall clarify Maddy's arguments and show that the objection can be overcome. I shall then reformulate a novel version of the objection and consider a possible answer, and I shall conclude that mathematical naturalism does not ultimately provide a viable strategy for accommodating an anti-revisionary stance on mathematics within a Quinean naturalist framework.

Keywords

Quinean scientific naturalism, confirmational holism, indispensability argument, revisionism, mathematical naturalism.

Introduction *

In *Second Philosophy. A Naturalistic Method* (2007), Penelope Maddy puts forward a naturalised account of mathematics which purports to

* Earlier versions of this paper were presented at the conferences *Word and Object*, *50 Years Later* and SIFA 2010. I thank both audiences – and particularly Jacob Busch, Stathis Psillos and Andrea Sereni – for interesting questions. Many thanks to Leon Horsten and two anonymous referees for this Journal for helpful comments. I gratefully acknowledge the financial support of the Department of Philosophy of the University of Bristol.

do justice to the special status of mathematics while maintaining a Quinean naturalist framework.

Quinean Scientific Naturalism can be characterised as the thesis that science is not answerable to any extra-scientific tribunal and is the ultimate arbiter of truth. In Quine's words, 'it is within science itself, and not in some prior philosophy, that reality is to be identified and described' (1981: 21). According to Quinean scientific naturalism, mathematics is part of our overall theory of the world and, as such, is continuous with science both on the ontological and methodological level; our best scientific theories dictate both what we should include in our ontology, and what are the legitimate ways of enquiring into the natural world.

Quinean naturalists are committed to the so-called *Quine-Putnam indispensability argument*, according to which we ought to commit to the existence of all and only those entities that are indispensable to our best scientific theories. A significant consequence of this view for mathematics is that only those bits of mathematics which actually figure in our best scientific theories should be regarded as justified. This consequence is taken to be problematic because it implies a revisionary stance towards mathematical practice. That is, it implies that certain aspects of mathematical practice, namely those that do not (aim to) contribute to our best scientific theories, are not legitimate.

Such an undesirable consequence is what motivates Maddy to reject the indispensability argument and to propose a novel strategy for the naturalisation of mathematics. Maddy's naturalism is a particularly attractive position because it seems to constitute the only option available to a Quinean naturalist who is uncomfortable with both revisionism about mathematical practice and a nominalist stance on mathematics. The guiding strategy of Maddy's mathematical naturalist is to avoid the commitment to revisionism by rejecting the conclusion of the indispensability argument, while at the same time maintaining that mathematics plays a crucial role in our understanding of the world because it is indispensable to scientific practice.

The aim of this paper is to assess whether there is a viable way of combining Quinean scientific naturalism with Maddy's non-revisionary stance towards mathematical practice. The structure of the paper is the following. In the first section I present the standard version of the indispensability argument and Quine's revisionism about mathematical practice, and make a few remarks on some mat-

ters of interest in the context of the present discussion. I then present Maddy's own view on the indispensability argument and her solution to avoid revisionism. Her position is subtle, so it is important to consider Maddy's objections to the indispensability argument, and her motivations for her positive view of mathematics, in detail. In the third section an important objection against mathematical naturalism made by Dieterle (1999) is considered, and it is argued that the objection is not conclusive. In the following section I outline Maddy's reply and show how it could successfully counter Dieterle's objection if cashed out in more detail. The analysis will suggest that Maddy's reply crucially relies on indispensability considerations but leaves the appeal to indispensability unargued, so I shall formulate a new objection which is more charitable to mathematical naturalism. Finally, in section five, I shall outline a possible response on behalf of the mathematical naturalist and I shall argue that such a strategy is not available to the mathematical naturalist due to an underlying tension in Maddy's position. I shall conclude that this tension leads to a dilemma that forces the mathematical naturalist to either embrace Quinean scientific naturalism along with a revisionary stance on mathematical practice, or to accept the undesirable consequence of mathematics being indistinguishable from pseudo-science. Either way, the moral to be drawn is that Maddy's mathematical naturalism is not a viable way of combining anti-revisionism about mathematical practice and Quinean scientific naturalism.

Naturalism and Indispensability

A standard characterisation of the indispensability argument is given by Colyvan (2001):

1. We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories
2. Mathematical entities are indispensable to our best scientific theories

Therefore,

3. We ought to have ontological commitment to mathematical entities.

The thesis that we ought to commit to the truth of the statements that feature in our best scientific theories taken at face value (premise 1) is *scientific realism*. In a Quinean framework, scientific realism results from the conjunction of (i) Quinean scientific naturalism (the thesis that science is the ultimate arbiter of truth) and (ii) *confirmational holism* (the thesis that scientific theories face the tribunal of experience as a whole).

The indispensability argument is considered the strongest naturalistic argument for mathematical realism (see e.g. Field 1980: 5). If sound, it allows us to draw ontological conclusions from the successful application of mathematics in the empirical sciences to the confirmation of the existence of mathematical entities. Indeed mathematical entities, on Quine's view, are on a par with theoretical entities in scientific theories: since there is no non-arbitrary way of discerning the support conferred by empirical evidence to theoretical entities referred to in scientific theories, and mathematical and theoretical entities are equally indispensable to scientific theories, it follows that mathematical entities are empirically confirmed in the same way as theoretical entities are (see Quine 1969: 97-98 and 1981:149-151).

Analogously, on this view, mathematics and science are also epistemologically on a par: when a scientific theory is confirmed, the mathematics which is required in the formulation of the theory also gets confirmed. More precisely, the soundness of each methodology employed in scientific practice is empirically tested along with the existence of the theoretical entities referred to in those theories. Scientific method as a whole determines what further methodologies are accepted as legitimate in our pursuit of knowledge of the world and what justificatory standards are authoritative in adjudicating among competing theories. Since mathematical methods are indispensable to scientific practice, the Quinean naturalist takes them to be legitimate tools in our pursuit of knowledge.

Maddy is a Quinean scientific naturalist insofar as she thinks that existence questions are ultimately settled by science and that scientific method is the most fundamental justification for determining what exists. However, she notices that in mathematical and scientific practice the questions about the existence of mathematical entities are not meant to be questions about the physical existence of those entities. Scientists are neither concerned about identifying the sort of

empirical evidence that would confirm the existence of mathematical entities (such as e.g. real numbers), nor are they concerned about the lack of confirmation of their existence (2007: 317). Conversely, it could be argued that mathematicians are not driven by scientific method in adjudicating questions about mathematical existence. Considerations about what mathematical objects there are, and such objects' properties, are typically settled by mathematical methods. On the basis of these considerations, questions about ontological commitment and proper method can be tackled separately.¹ Maddy recommends neutrality with respect to mathematical ontology, and focuses on methodological questions such as what makes for an acceptable axiom, a dependable method of proof, and so on.² Accordingly in this paper I shall follow Maddy's concern for methodological issues.

Given this focus on methodological issues, I reformulate the indispensability argument as follows:

- 1'a. We ought to commit to the truth of all and only those theoretical hypotheses that are confirmed according to our best scientific theories' standards of confirmation (Quinean scientific naturalism)
- 1'b. Scientific statements are not confirmed individually, but only as a corporate body (confirmational holism)
- 2'. Mathematics is indispensable to our best scientific theories

Therefore,

- 3'. We ought to commit to the truth of mathematical statements.

Our best scientific theories jointly constitute our best overall theory of the world. The Quinean scientific naturalist commits to the thesis that we are justified in believing an overall theory of the world

¹ In particular, the epistemological question of what justificatory standards are authoritative in mathematics becomes primary once we deny confirmational holism; more on this will be said in the next section.

² Questions about justificatory standards are both epistemological and methodological questions, so I shall use the terms interchangeably when talking about issues concerning justification of mathematical statements.

to the extent that it is sanctioned by our current scientific standards, and earns the conclusion that we are justified in believing indispensable mathematics to be true. The mathematical naturalist holds that mathematics is to be accepted insofar as it is part of our best overall theory of the world, but denies premise 1'b of the argument. If confirmation does not holistically extend among different components of scientific theories, 1'a does not entail that mathematical statements are to be believed literally true of the world. So the mathematical naturalist is committed to believe all empirically confirmed scientific statements to be true, but does not have to commit to the claim that *only* empirically confirmed statements are to be accepted.³ Thus the mathematical naturalist endorses a weaker version of scientific realism, according to which we ought to accept (as opposed to *believe to be true*) all (as opposed to *all and only*) those theoretical hypotheses that are confirmed according to our best scientific theories' standards of confirmation.

To support the view that a good (or the best, as the naturalist would rather say) overall theory of the world is one which is at least compatible with our scientific standards, with the view that scientific theories are to be believed on the basis of their being part of our best overall theory of the world is obviously circular. However, there is shared agreement on the thesis that naturalism is not a philosophical doctrine which can be established by means of compelling arguments, but rather a meta-philosophical attitude which can only be recommended. In this sense, this kind of circularity does not represent a dangerous worry for the scientific and mathematical naturalist alike insofar as it is already implicit in the naturalistic core tenet commending philosophical modesty (see Maddy 2007: 235).

Indispensability and Revisionism

Maddy wants to block the problematic consequence of the indispensability argument that only the part of mathematics which is applied in natural science has a legitimate epistemological status. She does that by rejecting confirmational holism (premise 1'b) on the basis of a

³ See § 2 for an account of why the denial of confirmational holism allows the mathematical naturalist to hold different epistemic attitudes towards different components of scientific theories.

historical analysis of scientific practice. According to Maddy, such analysis shows that scientific theories are not regarded as a homogeneous whole up for confirmation as a unit, but that

The mere presence, even indispensable presence, of a posit in our theory of the world is not enough to warrant the conclusion that its existence has been established. ... [T]he mathematical posits appear in descriptions that we don't regard as true, from which it would be inappropriate to draw ontological morals of any kind (2007: 315-316).⁴

For the sake of the argument, I shall assume that confirmational holism can be successfully denied.

The indispensability argument crucially relies on confirmational holism for the inference from the empirical confirmation gained by successful scientific theories to the confirmation of the bits of mathematics which are indispensable to formulate those theories. If confirmation does not holistically extend among different theoretical components of any scientific theories, it also does not extend from any scientific theory to a mathematical theory indispensable to its formulation. Therefore, if confirmational holism is denied, we are not entitled to draw ontological and epistemological conclusions about mathematics on the grounds of successful application of (part of) mathematics to natural science. Furthermore, we are allowed to have different epistemic attitudes towards different components of our theory of the world and, in particular, about the empirical and mathematical components of a theory. Hence, the revisionary stance on mathematical practice can be avoided.

Maddy's rejection of the indispensability argument for mathematics parallels her rejection of confirmational holism for science. In both cases the grounds for rejection are the misrepresentation of the actual practice of science and mathematics offered in the corresponding argument, where an accurate account of scientific and mathematical practice are the primary concern of the naturalist. In particular, she argues that scientific practice, *contra* confirmational holism, shows that scientists help themselves to whatever mathematics best suits their purposes. They are not concerned about the ontological status of

⁴ The historical case study that Maddy analyses is the gradual acceptance of the existence of atoms; for an extensive discussion of Quinean holism see Maddy (2007; I.6; IV.2.i).

the theoretical entities involved, and simply regard mathematical statements either as useful tools or even as literally false idealisations (*ibid.*). In the same way, *contra* the indispensability argument, mathematicians do not see their activity as constrained or even guided by the necessity of application (2007: 345). The grounds for believing theorems are deduction from axioms, appeal to mathematical intuition, or other intra-mathematical considerations. Such intra-mathematical considerations drive mathematicians' pursuit of novel, pure mathematical theories.⁵ So just as Maddy's criticism to confirmational holism points at an implicit tension between the holistic principle – that scientific theories are up for confirmation as a whole – and actual scientific practice, her criticism to the indispensability argument points at an implicit tension between the claim that only applied mathematics matters and actual mathematical practice. That is, the indispensability argument portrays mathematical practice in a misleading way by claiming that only empirically confirmed parts of mathematics are granted a legitimate epistemological status. Thus, ultimately, the indispensability argument should be rejected on naturalistic grounds.

A truly naturalistic attitude towards mathematical practice, on Maddy's view, also recommends the naturalist philosopher not to be revisionary about mathematical practice itself. If the commitment to indispensable mathematics had been the prevailing guiding principle of scientific and mathematical practice, many mathematical theories – initially pursued for purely mathematical reasons, but later shown to be of fundamental importance for natural science – would not have been developed (see e.g. the case of group theory for quantum mechanics, 2007: 347). A careful historical analysis of the practices involved shows that mathematics can best serve the needs of science when left flourishing without any extra-mathematical constraints. Therefore the mathematical naturalist is not entitled to reject some parts of accepted mathematical practice, which do not feature in scientific explanations, sanctioning only those parts of mathematics that receive empirical confirmation in virtue of their indispensability to successful scientific theories.

⁵ By *pure mathematics* Maddy means 'mathematics pursued for its own reasons, using its own methods, quite independent of [the naturalist's] well-honed arsenal of observation, experiment, theory formation and so on', and 'away from the necessity of application' (2007: 345).

A question may arise as to what extent confirmational holism is in fact revisionary about mathematical practice. In his reply to Parsons, Quine (1986: 400) suggests that the scientific naturalist is not compelled to regard only strictly indispensable mathematics as confirmed, but can consider as confirmed whatever mathematics is required for 'rounding out' indispensable mathematics. On this account, e.g., much of set theory would turn out to be included in confirmed mathematics in virtue of the fact that it underwrites most contemporary mathematics (see also Colyvan 2007), and only higher reaches of set theory would be considered as purely recreational. Ultimately, a very small fragment of mathematics is so isolated from the rest that it does not appear at any point of a chain of applications which bottoms out with applications in empirical science. So the price to pay in terms of methodological autonomy of mathematics for endorsing confirmational holism is after all not high. If confirmational holism is not revisionary in a substantive sense, the mathematical naturalist may not be able to reject the indispensability argument on naturalistic grounds.

However, Maddy's mathematical naturalism is not affected by this move. Even if indispensable mathematics is extended to include most of pure mathematics, Maddy's concern is precisely to argue that mathematics as *a whole* has a legitimate epistemological status. This includes absolutely unapplied mathematics, such as the higher reaches of set theory, one of Maddy's primary concerns in both her 1997 and 2007. So regardless of how revisionary the indispensability argument is, the mathematical naturalist would reject it on the grounds that mathematical practice is not conducted as if the indispensability argument was true.

Such considerations motivate Maddy to depart from Quinean scientific naturalism and reformulate the naturalist's meta-philosophical attitude towards mathematics. As an extremely successful enterprise, mathematics should be understood as it is practiced. Again, a good starting point would be looking at the history of mathematics. This reveals that until the eighteenth century mathematics was entrenched with science and philosophy. From the nineteenth century mathematics started differentiating its standards and goals from those of the other practices, freed itself from the purposes of serving science, and claimed its autonomy in pursuing purely mathematical problems by means of purely mathematical methods. Thus, according to Maddy, the naturalistic philosopher should, on naturalistic grounds, grant to

mathematics the same respect that Quinean scientific naturalism grants to science. Mathematics should be understood and evaluated on its own terms and according to its own standards. The methodological autonomy of mathematics is the core of Maddy's *mathematical naturalism*:

Mathematics should be understood and evaluated in its own terms, and should not be subject to criticism from, and does not stand in need of support from, some external, supposedly higher point of view (be it scientific or philosophical).⁶

Astrological and Theological Naturalism

While maintaining Quinean scientific naturalism with respect to science, the naturalistic philosopher *à la* Maddy has three important advantages over the naturalistic philosopher *à la* Quine. Firstly, in virtue of her rejection of confirmational holism she can consistently avoid adopting a revisionary stance towards the methodology of currently accepted mathematical practice. Secondly, for the same reason, she can also be a scientific naturalist without being committed to the literal truth of every statement that features our best overall theory of the world. Thirdly, she can account for the special status of mathematical methodology with respect to scientific methodological standards.

It is not clear, however, that the methodological autonomy of mathematics comes at a lesser price than its curtailment in the light of experimental evidence. Mathematical naturalism seems to yield the problematic consequence that if mathematical justificatory standards are independent from scientific standards of confirmation, mathematics is on a par with lots of other intellectual enterprises whose results we would count as pseudo-science. For example, it may be argued

⁶ The thesis as presented here is to be found in Maddy (1997:184). In 2007, Maddy modifies and reformulates many aspects of her 1997 view (including a substantial shift in terminology, from *mathematical naturalism* to *second philosophy of mathematics*) but maintains the core tenets. For ease of exposition, I shall here stick to the original terminology but shall refer to her recent work (Maddy 2007) unless otherwise stated.

The plausibility of mathematical naturalism arguably relies on the methodological unity of mathematics, which is itself a highly problematic issue. However, for reasons of space, I shall not consider this point here.

that astrology is such an enterprise: it has its own methodological standards and ontological commitments which are assessed and justified, as in the case of mathematics, on the basis of its own standards.

Maddy's reply to this objection is in line with Quinean scientific naturalism. For astrology makes causal claims about spatio-temporal reality, the naturalistic philosopher would simply treat astrological claims as scientific claims and test them against scientific method, eventually showing that they are not adequately supported by empirical evidence (1997: 203-5, 2007: 107-9). However, even though scientific testing undermines astrological empirical predictions it tells us nothing about the status of mathematics. The mathematical naturalist, unlike the Quinean naturalist, would not consider mathematical statements as saying anything about the spatio-temporal realm because of the way the practitioners themselves regard mathematical statements in their ordinary practice, and she would not consider any mathematical statements as up for confirmation by scientific testing.

Maddy acknowledges that the same worry may be reformulated at a more abstract level to include the case of not empirically testable disciplines. For example, we may think of a discipline, *pure theology*, not making any causal claims but describing the interactions of God and angels in an abstract realm (2007: 346). As pure mathematics, this discipline would not straightforwardly be a part of science, though it would be in principle liable to figure in scientific explanations. This discipline would be in all relevant respects analogous to pure mathematics, including its methodological autonomy and unrevisability in the light of scientific standards.⁷ In this case the mathematical naturalist has apparently no grounds for rejecting *theological naturalism* as unscientific.

Dieterle (1999) argued that the mathematical naturalist cannot meet this objection because the argument in support of mathematical

⁷ The idea that mathematics is not counterfactually bound to states of affairs in the world is implicit throughout all of Maddy's discourse. Though in the context of mathematical naturalism the unrevisability of mathematics cannot be explained by appealing to the centrality of mathematics in the web of beliefs as in Quinean scientific naturalism, Maddy never explicitly defends this claim nor considers why even applied mathematics would be unrevisable on scientific grounds (see also Tennant 2000). For reasons of space, I shall not address this issue here. For a discussion of the debate over the question whether Quinean scientific naturalism holds mathematics to be on a par with other parts of scientific theories in terms of confirmation but not in terms of falsification, see Busch forthcoming.

naturalism ultimately rests on indispensability considerations, and without the appeal to indispensability the Quinean naturalist cannot reject astrological or theological naturalism as unscientific.

More specifically, Dieterle argues that if mathematics was just a tool for scientists (as Maddy seems to suggest), then unapplied mathematics and theology are on a par and do not figure in scientific explanations, and the Quinean scientific naturalist only has to accept those parts of mathematics that are applied. This view, however, just is Quinean scientific naturalism. If, on the other hand, mathematics is more than a mere tool, then the fact that mathematical statements figure in scientific explanations means that mathematical statements say something about the spatio-temporal realm. If it was so, then applied mathematics would be on a par with astrology and pure mathematics would be on a par with pure theology. The reason for the first analogy is that both applied mathematics and astrology would make causal claims about the spatio-temporal realm, but would be equally unrevisable in the light of scientific standards. In the second case, mathematical and theological statements would be equally a-causal but liable to figure in scientific explanations. Therefore

‘Maddy’s use of indispensability considerations in the defence of mathematical naturalism ultimately either (1) undermines mathematical naturalism itself, leaving us with only scientific naturalism, or (2) leaves open the possibility of other unpalatable naturalisms’ (1999: 131).

Despite representing an important threat to mathematical naturalism, this objection is not conclusive. Indeed, it takes Quinean scientific naturalism as the background view in line with Maddy, but implicitly relies on confirmational holism – a thesis that, as seen before, the mathematical naturalist can consistently deny. This allows the mathematical naturalist to further deny that indispensably figuring in scientific explanations entails empirical confirmation. Thus, the mathematical naturalist is entitled to grant mathematics methodological autonomy and a legitimate epistemological status without committing her to the ontological and epistemological consequences of the indispensability argument.

In the next section I shall analyse the mathematical naturalist’s grounds for a response to Dieterle’s objection. I shall then articulate a further worry also originating from the appeal to indispensability

considerations and, in the final section, I shall outline and assess a possible argument in support of mathematical naturalism.

Maddy's reply

Maddy denies that applied mathematics has a different epistemological status from pure mathematics just in virtue of the former's successful application to science. However, this does not amount to having established that mathematics as a whole has a legitimate epistemological status independently of scientific standards. According to Maddy, it is possible to ground the legitimate epistemological status of mathematics as a whole in the role that mathematics plays in our overall theory of the world and in its importance in our pursuit of truth.

To support this point, Maddy considers the following thought experiment. Though providing tools for natural science is not the primary aim of pure mathematics, it is still one of its main aims. If mathematicians decided to pursue utterly different goals, Maddy contends, then mathematics would become irrelevant to science, and science would have to replace it with a different tool, say *mathematics**. So if old mathematics was like pure theology its role would also be similar, and the new tool, *mathematics**, would play the role that mathematics played before in the naturalistic philosopher's investigation (2007: 150-151; 2007: 350). If, conversely, parts of pure theology were discovered to be applicable, then pure theology would be functioning as the old pure mathematics, and it would have the same role the mathematical naturalist grants to actual pure mathematics.

The thought experiment purports to suggest that what confers a legitimate epistemological status to mathematics is not its methodological autonomy, but its indispensability as a tool in the pursuit of our understanding of the world. Thus the objection that the mathematical naturalist is compelled to deny either the usefulness of mathematics or its methodological autonomy can be resisted by appealing to the place that mathematics occupies in our overall theory of the world.

More generally, on this view, what distinguishes mathematics from science is its methodological solipsism; pure mathematics develops by pursuing purely mathematical goals, and is not subject to revisability in the light of scientific standards of confirmation. On the

other hand, what distinguishes mathematics from other forms of non-scientific enquiry is not its methodological solipsism, but the indispensable role that it plays in our overall theory of the world. However, is the indispensability of mathematics a consideration that the mathematical naturalist can legitimately appeal to in order to discern mathematics from pseudo-science?

Again, Maddy does not explicitly argue in support of this move, but an argument on behalf of the mathematical naturalist could be outlined in the following way. Quinean scientific naturalism holds that we are justified in accepting an overall theory of the world to the extent that it is confirmed by empirical evidence. Accordingly, we ought to accept any theory which is part of our best overall theory of the world, whereby such a theory is recognised as scientific. As mathematics is indispensable to scientific theories, it is also indispensable to our best overall theory of the world (the indispensability relation being transitive). Hence, we ought to accept mathematics.

This argument seems to run into an obvious objection, again targeted at the appeal to indispensability considerations. Because of her rejection of the indispensability argument, the mathematical naturalist cannot appeal to the application of mathematics in natural science in order to ground the acceptance of mathematics (call this the *applicability objection*).⁸ Maddy is aware of this objection:

[The appeal to the usefulness of mathematics to natural science] is not a reversion to a Quinean indispensability argument, because the conclusion is only that mathematics is different from pure astrology, not that mathematics is confirmed (2007: 346, footnote 4).

In other words, what is being denied by the mathematical naturalist is not the claim that mathematics is indispensable to scientific practice, but the inference from the indispensable role that applied mathematics plays in scientific explanations to the confirmation of the existence of mathematical entities and the legitimacy of applied mathematical methods. The mathematical naturalist can recognise the autonomy of mathematics with respect to scientific practice while not denying its crucial role within our best overall theory of the world. This crucial role in fact makes mathematics as a whole different from pseudo-

⁸ Maddy uses the terms *indispensability* and *usefulness* of mathematics with respects to scientific practice interchangeably, so I shall here conform to her use.

science, and not – as the indispensability argument purports to establish – the indirect empirical confirmation received by some mathematical statements through their successful application in scientific practice.

How Indispensable is the Indispensability Argument?

Despite the appeal of this proposal, I shall argue that the strategy outlined above is unavailable to the mathematical naturalist. If the argument is correct, then mathematical naturalism cannot meet the applicability objection and thus ultimately fails to explain the difference between mathematics and pseudo-science.

It is important to notice that the criterion for discerning the success of any practice is ultimately scientific, for both the scientific and the mathematical naturalist. The success of a practice is determined by the role it plays in our best overall theory of the world. Since (in line with Quinean scientific naturalism) our best overall theory of the world is adjudicated by our current scientific standards, it trivially follows that any practice that plays a crucial role in our current best overall theory of the world is ultimately accepted on the grounds of the very same standards. As seen before, astrology is dismissed as pseudo-science by empirically testing its causal claims, and pure theology by observing that it does not play any role in our best overall theory of the world.

Only one part of mathematics, however, is involved in scientific explanations and empirical predictions. So there is an asymmetry in the standards of acceptance for pure and applied mathematics: applied mathematical methodology is both accepted on scientific grounds and sanctioned by mathematical standards, whereas pure mathematical methodology is just sanctioned by mathematical standards. Mathematical naturalism does not warrant the acceptance of pure mathematics by scientific standards.⁹ In order to conclude that mathematics

⁹ It follows from this that the mathematical naturalist cannot even claim that mathematics is a successful enterprise as a whole simply on the basis of historical study and careful observation of mathematical practice. The criterion for a practice to be successful is still a scientific one, and pure mathematics cannot be judged successful on scientific grounds in the absence of a positive argument in support of this claim. So the mathematical naturalist can only claim that mathematics is a successful enterprise as a whole conditionally upon the soundness of Maddy's reply to the applicability objection.

as a whole ought to be accepted, the mathematical naturalist must be able to warrant the inference from the usefulness of a part of mathematics to the acceptance of mathematics as a whole. Therefore, some sort of *holistic extension* of acceptance is needed in order to establish the conclusion that pure mathematical standards ought to be accepted on the basis of scientific standards (i.e., that mathematics as a whole is accepted on scientific grounds) because of the successful application that part of mathematics receives in scientific practice.

At this point the mathematical naturalist may decide to bite the bullet and support this strategy, still without committing to confirmational holism. She has in fact strong reasons not to accept the ontological consequences of the indispensability argument – the consequences violate the naturalistic maxim not to be revisionary about a successful practice. She has, however, no principled reasons against a weaker version of holism which only focuses on the grounds for acceptability of scientific methodologies, and which respects the tenets of mathematical naturalism with respect to ontological matters. The idea of a *methodological holism* may be formulated as follows: the soundness of the methodological maxims employed in our pursuit of knowledge of the external world is not tested individually, but methodologies as a whole receive epistemological legitimacy through successful application in scientific practice. Given the central tenet of mathematical naturalism, according to which mathematical methodology should not be subject to extra-mathematical criticism, if the mathematical naturalist assumes methodological holism, then for pure mathematics to be part of our best overall theory of the world just is for it to be sanctioned by its own methods and standards. On the assumptions that, firstly, a theory is recognised as scientific if it is part of our best overall theory of the world, and that, secondly, mathematics is indispensable to our best overall theory of the world, we can grant mathematics as a whole the status of a scientific theory even though it is sanctioned by its own methods and standards.

By appealing to methodological holism the mathematical naturalist can draw the desired epistemological conclusion that mathematical methodology ought to be accepted as a whole. This follows from the vindication of the methodology of applied mathematics through the indispensable role of applied mathematics in science. She can thus maintain that the credibility of mathematics rests on a scientific basis even though mathematics is not subject to scientific criticism. This provides the mathematical naturalist with the desired criterion to

distinguish pure mathematical methodology from pseudo-scientific methodologies on the basis of the indispensable role that mathematics as a whole plays in our best overall theory of the world. Furthermore, not only does methodological holism respect the ontological neutrality of the mathematical naturalist, but it also justifies the acceptance of mathematics *regardless* of any stance a naturalist may have with respect to mathematical ontology. Methodological holism allows the naturalist to remain faithful to Quinean scientific naturalism for what concerns ultimate matters of existence of mathematical entities, but at the same time allows her to vindicate the methodological autonomy of mathematics and its special epistemological status among sciences.

However, is methodological holism a viable option for the mathematical naturalist? Since the naturalistic standards for acceptance are scientific, the extension of the acceptability from applied mathematical methodology to pure mathematical methodology is only warranted on the grounds of a possible future application of pure mathematical methods in scientific practice. As such, adopting methodological holism seems to be in tension with the mathematical naturalistic prescription not to take applied mathematical methodology as a guide to mathematical methodology as a whole. Yet, granting epistemic value to pure mathematics regardless of any possible application is precisely the motivation behind mathematical naturalism. As briefly seen above, Maddy argues that the history of mathematics shows how from the nineteenth century mathematics started differentiating its standards and goals from those of other scientific practices, and providing useful tools for scientific practice ceased to be the main concern of mathematicians. If it had not been so, many mathematical theories initially pursued out of purely mathematical interest and devoid of any application would not have been developed. Hence, endorsing methodological holism would result in the failure to appreciate the methodological autonomy of mathematics, and in the adoption of a revisionary stance towards pure mathematical practice, which the mathematical naturalist forcefully rejects.

In summary, the claim that mathematics as a whole is acceptable on scientific grounds only follows if methodological holism is assumed, but methodological holism cannot be assumed because it is incompatible with the mathematical naturalistic prescription not to be revisionary about mathematical practice. Therefore the mathematical naturalist faces a dilemma: either she accepts methodological holism at the price of a revisionary stance on mathematical practice, or she

rejects methodological holism at the price of losing a criterion to distinguish pure mathematics from pseudo-science.

More specifically, we have seen that the assumption of methodological holism allows the mathematical naturalist to (i) justify pure mathematical methodology on scientific grounds, and (ii) to distinguish pure mathematics from pseudo-science. However, methodological holism commits the mathematical naturalist to making scientific standards authoritative in the assessment of mathematical methodology, in line with Quinean scientific naturalism. This ultimately forces the mathematical naturalist to deny her maxim that mathematics should not be subject to extra-mathematical standards, and to deny the scientific status of pure mathematics, resulting in the adoption of a revisionary stance on mathematical practice. Thus if the mathematical naturalist accepts methodological holism, her position will be indistinguishable from Quinean scientific naturalism.

On the other hand, if the mathematical naturalist does not give up on the methodological solipsism of mathematics but rejects methodological holism, she cannot appeal to the indispensable role that mathematics plays in our overall theory of the world to justify the scientific status of pure mathematics. Without an holistic extension of acceptance from applied to pure mathematical methodology, pure mathematics is only sanctioned by mathematical standards. This makes mathematics completely devoid of application not indispensable to science, and accordingly not a discipline whose methodology is to be included among the methodologies of the scientific theories which constitute our best overall theory of the world. Thus if the mathematical naturalist rejects methodological holism, she is left with no criterion to discern pure mathematical methodology from pseudo-scientific ones. Therefore, she has to conclude that pure mathematics and pure theology are methodologically on a par, and she has to grant theological naturalism the same respect that she grants mathematical naturalism.

Conclusions

In this paper, I have considered one of the most influential contemporary views on the naturalisation of mathematics and I have argued that despite its advantages, it ultimately falls short of accounting for the special status of mathematical practice within the framework of

Quinean scientific naturalism. I considered an important objection to mathematical naturalism which focuses on the appeal to the usefulness of mathematics and showed that the mathematical naturalist can resist it because of her rejection of confirmational holism. I then reformulated the worry by taking into account Maddy's (2007) reply, and argued that her rejection of confirmational holism does not allow the mathematical naturalist to make the inference from the acceptability of the methodology of applied mathematics to the acceptability of the methodology implicit in mathematical practice as a whole. Therefore mathematical naturalism fails to accommodate the methodological autonomy of mathematics within Quinean scientific naturalism.

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Indispensability Arguments and Their Quinean Heritage

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Abstract

Indispensability arguments (IA) for mathematical realism are commonly traced back to Quine. We identify two different Quinean strands in the interpretation of IA, what we label the ‘logical point of view’ and the ‘theory-contribution’ point of view. Focusing on each of the latter, we offer two minimal versions of IA. These both dispense with a number of theoretical assumptions commonly thought to be relevant to IA (most notably confirmational holism and naturalism). We then show that the attribution of both minimal arguments to Quine is controversial, and stress the extent to which this is so in both cases, in order to attain a better appreciation of the Quinean heritage of IA.

Keywords

Quine's philosophy of mathematics, indispensability arguments, Platonism, naturalism, inference to the best explanation.

Introduction and aims*

Word and Object (Quine 1960) contains an illuminating footnote concerning its author's views on mathematical platonism. While stressing the advantages of his ‘canonical notation of quantification’, and ‘the restoration of law and order’ that it allows in ontological

* The authors would like to thank the audience at the *Word and Object, 50 Years Later* conference (Rome, 28-29 May 2010) for fruitful discussion, and the audience at the *Trends in the Philosophy of Mathematics* conference (Frankfurt, 1-4 September 2009) for discussion on an earlier draft. Many thanks also to Mario De Caro, Patrick Greenough, Marco Panza, and two anonymous referees.

disputes (Quine 1960: 242), Quine feels the need for the following clarification:

A more accountable misapprehension is that I am a nominalist. I must correct it; [...] In all books and most papers I have appealed to classes and recognized them as abstract objects. I have indeed inveighed against making and imputing platonistic assumptions gratuitously, but equally against obscuring them. Where I have speculated on what can be got from a nominalistic basis, I have stressed the difficulties and limitations. (*ibidem*: 243, fn. 5)

Clearly this had not always been the case, as the complete renunciation of abstract entities declared in the first lines of Quine's and Goodman's 'Steps Towards a Constructive Nominalism' makes clear.¹ But that declaration, as Quine himself stresses, 'needs demotion to the status of a mere statement of conditions for the construction in hand' (*ibidem*). Furthermore, Mancosu 2008 has recently shown that Quine's doubts on the feasibility of that construction and the consequent complete renunciation of (mathematical) abstract entities emerged as early as in 1948.

It is now clear that Quine's stance with respect to the problem of the existence of mathematical objects is far from being a nominalist one, despite Quine's empiricist and naturalist framework. The reasons offered by Quine for this have been regimented in the form of an argument, today widely renowned as the Indispensability Argument (IA). In a nutshell, the argument appeals to the uncontroversial fact that mathematical theories are commonly and usefully applied in most of our scientific theories. Then, from the assumptions that we are justified in taking those scientific theories to be true, and that if they are true then so are the mathematical theories that we cannot avoid using in formulating them, we are led to conclude that these indispensable mathematical theories are true and that their mathematical objects exist.

Quine suggested the basic ideas underlying IA, but he offered no definite formulation of it. And from Putnam's *Philosophy of Logic* (1971) onwards, it is rare to find two formulations that completely

¹ Cf. Goodman, Quine (1947: 105): 'We do not believe in abstract entities. No one supposes that abstract entities—classes, relations, properties, etc.—exist in space-time; but we mean more than this. We renounce them altogether'.

square with each other. Nevertheless, from both *Word and Object* and a vast number of Quine's essays, it is possible to single out partly overlapping clusters of theses and notions that, in different forms, constitute the Quinean heritage of the various versions of IA available on the market.

At least two different strands in the post-Quinean discussion of IA are, according to us, identifiable. On the one hand, there is the traditional analytic attention to theories' formulation and expressive power, focusing on logico-syntactical considerations regarding the form of scientific and mathematical theories, the notion of reference to mathematical objects, and the adjudication of a proper criterion of ontological commitment. Call this *the logical point of view*. On the other hand, we find considerations stemming from the philosophy of science regarding how scientific theories work and how they are confirmed, what forms of argument are appropriate for justifying belief in those theories, and how different posited entities contribute to the overall epistemic and semantic evaluation of a given theory. Call this *the theoretical contribution point of view*. Most of the notions and theses to which many formulations of IA are currently thought to appeal – ontological commitment, indispensability, naturalism, and confirmational holism – merge aspects of both these two strands.

It is our contention that most of the available formulations of IA can be thought of as organized in a spectrum. At both ends of the spectrum lie minimal versions of the argument; minimal, that is, insofar as they feature the fewest or least controversial conceptual ingredients that are required in order to derive the desired conclusion. The arguments at each extreme are representative, respectively, of the two points of view just described. Various central shades of the spectrum are given by different versions of IA, obtained through the addition or subtraction of one or more assumptions.

It is not our present aim to review the overall structure of this spectrum. Rather, we want to show that there are both theoretical and exegetical problems in tracing both minimal versions of IA back to Quine's positions, and that these problems raise a number of concerns both regarding Quine's own way of reflecting on the issue and regarding the structure of the current debate.

From a logical point of view²

Colyvan's version of IA (Colyvan 2001) is a suitable representative of the sort of arguments that we would locate between the two extremes of our suggested spectrum:

[CIA] Colyvan's Indispensability Argument

- i) We ought to have ontological commitment to all and only those entities that are indispensable to our best scientific theories;
- ii) Mathematical entities are indispensable to our best scientific theories;

[CIA] -----

- iii) We ought to have ontological commitment to mathematical entities.

According to Colyvan, the first biconditional premise 'follows from the doctrines of *naturalism* and *holism*' (Colyvan 2001: 12) – as regards, respectively, the 'only' direction and the 'all' direction of the biconditional. Let us state these two theses in a way convenient for our aims:³

[NAT] *Naturalism*: scientific theories are the only source of genuine knowledge. As a consequence, with respect to ontology, we are justified in acknowledging the existence *only* of those entities that are quantified over in our true or well-confirmed scientific theories.

[CH] *Confirmational Holism*: empirical evidence does not confirm scientific hypotheses in isolation, but rather scientific theories as a whole. As a consequence, with respects to ontology, we are justified in acknowledging the existence of *all* those entities that are quantified over in our true or well-confirmed scientific theories.

² Many of the issues in this section are explored in more details in Panza, Sereni, Fothcoming; cf. also Panza, Sereni, Unpublished.

³ This implies radical simplifications in the formulation of these controversial theses. Naturalism, in particular, could be given milder readings. The version offered here, however, seems required in order for the 'only' direction of [CIA]'s first premise to follow from it.

Colyvan's argument is meant to support mathematical platonism – the thesis that certain mathematical objects exist – rather than what we might call *semantic realism* – the thesis that certain statements (or theories) are true, without commitment to the idea that it is objects (or anything else in particular) that make them true. Reference to ontological commitment makes this point explicit, and Colyvan is indeed assuming Quine's criterion of ontological commitment as the suitable one. Roughly speaking, and skipping some details:

[QC] Quine's Criterion of Ontological Commitment

The ontological commitment of a theory is given by the objects that must be counted among the values of the variables of the existentially quantified statements that are entailed by the theory.

[QC] applies to theories when they have been regimented in canonical notation and when it has been established what expressions must be indispensably employed in the reformulation thus obtained. Its application therefore requires a clear characterization of the notion of indispensability.

If we want to adhere to a Quinean formulation, as emerged at least from 'Designation and Existence' (1939) through 'On What There Is' (1948), and is clearly presented in *Word and Object*, indispensability has to be interpreted, at least partly, as a logico-syntactical feature pertaining to the formulation of a theory. Quantification (over a given sort of entity) is what is deemed indispensable when we rewrite our theories in canonical notation, if it is not possible to dispense with it by means of paraphrase and contextual definitions.

However, indispensability is a relative notion: when we want to rewrite a given theory in order to evaluate whether quantification over a given sort of entities is or isn't indispensable, we first need to establish what features of the original theory our reformulation must preserve. In other words, which specific equivalence relation allows us to say that our new theory is equivalent to the original one. Moreover, the resulting theory must *per se* enjoy a number of features that, intuitively speaking, make it a good theory. Thus a proper general clarification of the notion of (in)dispensability, restricted to quantification,⁴ should take the following form:

⁴ Attention to quantification is due to the attempt to evaluate the Quinean character of IA. A more general version would remain neutral as to which is the proper logico-linguistic device by which (in)dispensability is established.

[IND] (In)dispensability: Quantification over entities X is dispensable from a scientific theory S if and only if there is a scientific theory T in which quantification over X is absent and such that:

- i) T is ε -equivalent to S , where ε -equivalence is an appropriate relation of equivalence among theories;
- ii) T is equally or more virtuous than S , given an appropriate criterion for the virtuosity of theories;

If S quantifies over X , and there is no scientific theory T satisfying conditions (i)-(ii) above, then quantification over X is indispensable for S .⁵

Not only does this characterization show in what sense (in)dispensability is a relative, aim-specific notion (nothing is indispensable *per se*, but only relative to a specific theoretical purpose), but it also allows us to specify (and, above all, forces one to declare) in the definition of the notion, what specific theoretical feature one expects scientific theories to preserve: e.g. observational content, empirical content, expressive power, explanatory power, and so on (whether each of these is exclusive with respect to all others is a further issue). Arguably, a good candidate for ε -equivalence for what we labelled the logical point of view is ‘having the same expressive power’: what we are interested in is whether some mathematical vocabulary necessarily has to be employed in order to *state* certain scientific laws.

Let us come back to [CIA]. We now see that the latter is a version of IA that relies on the following four assumptions: [NAT], [CH], [QC] and an indispensability thesis based on [IND]. These are all theses or notions with Quinean origins. But do we need all this theoretical machinery in order to gain the desired conclusion? Many have stressed (Resnik 1995, Dieveny 2007, Liggins 2008) that IA can go through even without [CH] – as Colyvan himself suggests.⁶ What usually goes unnoticed is that IA can go through even without [NAT]. If we try to formulate IA in a less theoretically committed way, what we get is an argument of the following form:

⁵ Selection of an appropriate criterion of virtuosity might be aim-specific, and might sacrifice other features of theories commonly thought to be epistemic virtues. It is clear, for example, that Field’s (1980) nominalized version of Newtonian Gravitation Theory praises ontological parsimony and sacrifices simplicity.

⁶ Cf. Colyvan (2001:37): ‘As a matter of fact, the argument can be made to stand without confirmational holism: it’s just that it is more secure with holism’.

[MA] *Minimal argument*

- i) We are justified in believing some scientific theories to be true;⁷
[We are justified in believing T to be true]
- ii) Among them, some are such that some mathematical theories are indispensable to them;
[M is indispensable to T]
- iii) We are justified in believing true these scientific theories only if we are justified in believing true the mathematical theories that are indispensable to them;
[We are justified in believing T to be true only if we are justified in believing M to be true]

[MA_a]-----

- iv) We are justified in believing true the mathematical theories indispensable to these scientific theories.
[We are justified in believing M true]
- v) We are justified in believing true a mathematical theory only if we are justified in believing the objects it is about to exist;
[We are justified in believing M to be true only if we are justified in believing the objects it is about to exist]

[MA_b]-----

- vi) We are justified in believing the objects which the indispensable mathematical theories are about to exist.
[We are justified in believing the objects M is about to exist]

The argument, if sound, establishes for given mathematical theories what Field (1982:501) calls *theoretical indispensability*. It claims that (we are justified in believing that) mathematical theories are true (or mathematical objects exist) on the grounds of considerations about the proper formulation or expressive power of theories. Such an argument – relying, among the aforementioned theses, only on

⁷ [MA] is stated in epistemic terms. It speaks of our justification in believing certain theories to be true and certain objects to exist – as is the case in Colyvan's argument and others. Notice that also a non-epistemic version of the two arguments can be offered by reformulating steps (i), (iii), (iv) and (v) in such a way that justification is not mentioned in them. This difference hinges upon two different conceptions of ontology, as respectively either a descriptive or a normative discipline. In the end, this leaves us with four different (minimal, though for different reasons) versions of IA: [MA_a], [MA_b], and the non-epistemic versions of both.

[QC] and an indispensability thesis based on [IND] – shows several advantages over Colyvan's.

One is that it shows that two different conclusions might be reached by different versions of IA: [MA_a] is an argument for semantic realism, from which it is possible (but not mandatory) to get to platonism if one adds premise (v), which in fact can be taken to express (a generalizes version of) [QC].

Secondly, notice that what is needed in order to justify premise (i) is some form of scientific realism, and this is a weaker position than [NAT]. Scientific realism sees scientific theories as *a* genuine source of knowledge, but need not consider them as *the only* genuine source of knowledge. If IA is made to appeal to both [CH] *and* [NAT], it provides sufficient *and* necessary conditions for its conclusion(s). Thus neither the semantic realist nor the platonist conclusion cannot be reached for all those theories (and their objects) that do not find application in true or well-confirmed scientific theories. Quine accepted this conclusion⁸, but a more plausible version of IA might want to avoid it, as does [MA]. [MA] is not by itself inconsistent, for example, with the belief that we can gain mathematical knowledge (about mathematical statements or mathematical objects) through *a priori* arguments.

Scientific realism is something Quine clearly championed⁹, but it seems that the whole complex of Quine's theses overdetermines a proper version of IA: some version will follow if both [CH] and [NAT] are assumed, but they need not be assumed in order for *any* version of IA to be offered. That [CH] and [NAT] are not indispensable to IA was already implicit in Putnam's formulation in *Philosophy of Logic* (Putnam 1971:347):

So far I have been developing an argument for realism roughly along the following lines: quantification over mathematical entities is indispensable for science, both formal and physical, therefore we should accept such quantification; but this commits us to accepting the existence of the mathematical entities in question.

⁸ With varying attitudes: cf. Quine (1986:400) and Quine (1995:56-57). Maddy (1992) has argued against this consequence of a Quinean version of IA (cf. also Parsons 1978, Maddy 1992, Leng 2002, Colyvan 2007).

⁹ Issues with indeterminacy and ontological relativity apart: cf. Putnam (1988).

No mention is made here of naturalism nor holism.¹⁰ If we follow Putnam, we can still have a proper version of IA and yet consider the part of the Quinean heritage consisting of [NAT] and [CH] as dispensable from the argument – unless held for independent reasons and for these reasons alone made to bear on IA.

The theory-contribution point of view

Let us move to the other extreme of our suggested spectrum, and focus on another form of reasoning that is often appealed to in connection with IA. We can take our clue from a passage of Quine's often suggested to be a statement of IA:

Ordinary interpreted scientific discourse is as irredeemably committed to abstract objects – to nations, species, numbers, functions, sets – as it is to apples and other bodies. All these things figure as values of the variables in our overall system of the world. The numbers and functions contribute just as genuinely to physical theory as do hypothetical particles. (Quine 1981: 149-50)

We take Quine's argument to be the following: if it can be argued that mathematical entities contribute to scientific theories in a relevantly similar way to how theoretical entities contribute to those theories, then there is (either in the positive or in the negative sense) as much reason to believe that the latter exist as there is reason to believe that the former exist.

Even though [CH] might have been a working hypothesis of Quine's throughout his works, there is no explicit mention of it in the quotation above. Colyvan (2001) and Baker (2009) have accordingly suggested a reading of IA based on the notion of theory contribution that is independent of [CH] (Colyvan explicitly takes this formulation to be of Quinean heritage). This is obtained by stressing that IA seems intimately connected with arguments for scientific realism about theoretical entities. Theory contribution is seen in both cases as evidence for existence, and mathematical entities are thus argued for by means of an inference to the best explanation (IBE). The generic

¹⁰ As concerns naturalism and the 'only' direction in [CIA], Putnam recently stressed this point again. Cf Putnam Forthcoming.

form of IBE can be expressed as follows (where T is a theory and X is a set of data):

- i) X
 - ii) T, T_2, \dots, T_n are explanations of X
 - iii) T explains X better than $T_2 \dots T_n$
 - iv) We ought rationally to believe the theory that best explains X
- [IBE]-----
- v) We ought rationally to believe T

Scientific realists argue that if the postulation of theoretical entities contribute towards a given explanation being best, we have reason to believe that they exist. If one believes, as Quine apparently does, that mathematical entities contribute to the (explanatory) goodness of scientific theories in just the same way that theoretical entities do, then we ought to, by considerations of consistency, be realists about mathematical entities too (Colyvan 2006). We might easily state a version of IA that argues for mathematical platonism by means of [IBE] (call it IA_{IBE}).

It is, of course, not enough to simply postulate that mathematical entities contribute to scientific theories in the same way as theoretical entities, and much work has been done in recent literature that tries to argue that mathematical entities do contribute in this way (e.g. Colyvan 2008; Baker 2005, 2009). Rather than add to that debate, we shall argue: (i) that IA_{IBE} is different from other versions of IA both in kind and in content, and (ii) that IA_{IBE} does not have the kind of Quinean heritage that Colyvan attributes to it.

It is important to notice that [IBE] is an ampliative mode of inference. We have seen that [CIA] involves four of the ingredients introduced above: [IND], [NAT], [CH] and [QC]. To what extent are any of these presupposed in the ampliative variety of IA? On an appropriate understanding of “best theories”, only one of them is.

Consider first [NAT]. IA_{IBE} presupposes only scientific realism, and it was already pointed out that scientific realism is a weaker position than naturalism.

Now consider [CH]. Appeal to theory contribution is supposed to make [CH] redundant: if mathematical theory M contributes appropriately to a scientific theory T that counts as a best explainer, we

thereby have a justification for M , and there is thus no need to adopt [CH].¹¹

The notion of indispensability therefore ceases to play any role. It is implicit that mathematics is indispensable to our current best theories, because any part of our current best theories (that ontologically commits to some kind of entity) is considered indispensable to that theory. This is partly what it means for a theory to be “best”. If rival empirically adequate theories existed, that had less ontological commitments, those theories would be considered best, everything else being equal.

How about [QC]? In order to get from the truth of any theory whatsoever to a claim about what exists something like [QC] will be needed. As all that IA_{IBE} can establish is that we have reason to believe that some theory is true, some variety of [QC] will be needed for inferring that mathematical entities (or any other entities mentioned in those theories) exist. So out of all of the ingredients above, only [QC] (or something similar) is doing any work in IA_{IBE} .

It is not at all clear what the Quinean heritage of IA_{IBE} would be, since only one of the traditional Quinean-inspired premises will be doing any work in it. Colyvan clearly understands [CIA] as Quinean in spirit (Colyvan 2001). However, the argument from theory contribution that we find in the writings of Quine is very different in nature from the way that scientific realists think about [IBE] and theory contribution as they are employed in that mode of inference.

[IBE] is argued to be a reliable mode of inference by scientific realists, in virtue of best explanations being truth tracking. As argued by van Fraassen (1980), one could easily interpret scientists as choosing some theory over others because it is more *useful* to employ certain theories over others, e.g. because certain theories are easier to work with in virtue of their simplicity or the like. This latter criterion is pragmatically motivated in that it reflects our interests and what we find useful: it is thus not to be related to issues of truth at all. For those scientific realists who argue for realism by use of [IBE] (who are those who would potentially be persuaded by IA_{IBE}) it is imperative that a pragmatic reading of [IBE] is ruled out. Rather surprisingly,

¹¹ [CH] could be used to argue for scientific realism, but it is actually at odds with current varieties of scientific realism, since these hold that only parts of our scientific theories are confirmed by empirical evidence. See Folina (1999).

however, Quine understood theoretical virtues in a way that is incompatible with the required realist understanding of [IBE].

Quine's argument for mathematical platonism by way of theory contribution.

Since there is no direct evidence to support the hypothesis that theoretical entities exist, Quine sensibly suggests that we need to look for what might count as 'indirect' evidence for their existence. A look at scientific practice shows that everything else being equal, simple theories are judged to be better theories than complex theories:

The molecular physicist is, like us, concerned with commonplace reality, and merely finds that he can simplify his laws by positing an esoteric supplement to the esoteric universe (...) No matter if physics makes molecules or other insensible particles seem more fundamental than the objects of common sense, the particles are postulated for the sake of simple physics. (Quine 1966: 236-241)

Quine makes two observations here. The first is that in one sense scientists are, like 'us' ordinary people, concerned with commonplace reality. The second is that scientists postulate the existence of entities like molecules for the sake of simplifying laws. Thus far, this latter claim looks rather ontologically innocent, amounting only to a description of how physicists practice physics, and it is ambiguous between a realist and an antirealist account of theoretical entities. When we turn to Quine's view on how we come to form beliefs about objects of common-sense reality the above observations become significant:

If we have evidence for the existence of the bodies of common sense, we have it only in the way in which we may be said to have evidence for the existence of molecules. The positing of either sort of body is good science insofar as it helps us formulate our laws. (Quine 1966: 237)

All of the evidence that we consider as relevant to the existence of visible objects is in fact evidence in the same sense of 'evidence' relevant to the positing of molecules. Furthermore, we have here an indication that Quine actually thinks that positing bodies makes for 'simpler' theories, in the sense that doing so is 'helpful'. In other

places, he is absolutely clear that the sense of evidence he has in mind is best construed as pragmatically motivated:

Actually I expect that tables and sheep are, in the last analysis, on much the same footing as molecules and electrons. Even these have a continuing right to a place in our conceptual scheme only by virtue of their indirect contribution to the overall simplicity of our linguistic or conceptual organization of experience; for note that even tables and sheep are not direct sensations... It would be senseless to speak of a motive for this archaic and unconscious posit [common-sense bodies], but we can significantly speak of its function and its survival value; and in these respects the hypothesis of common sense external objects is quite like that of molecules and electrons. (Quine 1953: 210).

Quine has now suggested a number of things: if we have evidence for the existence of objects of our commonplace reality we only have evidence in the same sense that we have evidence for the existence of molecules; objects of commonplace reality are theoretic in the same ways as theoretical entities like molecules because they are given within a conceptual scheme; the reasons we have for positing the existence of molecules are that doing so is 'helpful' and 'useful' for the purpose of physics, it has a 'function' and a 'survival value'.

According to Quine there is no standard of reality outside of the standards given in our conceptual scheme, or theory, of the world: 'Everything to which we concede existence is a posit from the standpoint of a description of the theory building process, and simultaneously real from the standpoint of the theory that is being built'. (Quine 1960: 22)

We cannot significantly question the reality of the external world, or deny that there is evidence of external objects in the testimony of our senses; for, to do so is simply to dissociate the terms "reality" and "evidence" from the very applications which originally did most to invest those terms with whatever intelligibility they may have for us. (Quine 1957: 2)

Because we take the kind of evidence that we have for making existence claims about objects – 'their assumption helps [man] organize experience' – to be defining of what we mean by evidence, we should by consideration of consistency also hold that this kind of evidence is sufficient for making claims about unobservables. Quine then argues that since pragmatic value is sufficient for making claims about observables, pragmatic value is also sufficient for making claims about

unobservables: ‘The benefits of the molecular doctrine which so impressed us [earlier], and the manifest benefits of the aboriginal posit of ordinary bodies, are the best evidence of reality we can ask’. (1966: 238-239)

With this rather permissive view of evidence, we can now revisit the issue of whether mathematical entities and theoretical entities are evidentially on a par. When we look to at least some of our scientific theories it is no doubt true that mathematics contributes towards the formulation of theories that are simpler (or the like) than theories formulated without the use of mathematics. Thus, by Quine’s characterisation of evidence, mathematics contributes to theories in a way that warrants belief that mathematical objects quantified over in mathematical theories exist:

I think the positivists were mistaken (...). Existence statements (...) do admit of evidence, in the sense that we can have reasons, and essentially scientific reasons, for including numbers or classes or the like in the range of the values of our variables. Numbers and classes are favoured by the power and facility they contribute to theoretical physics and other systematic discourse about nature. (Quine 1969: 97-98)

So quite independently of consideration about [CH], Quine produced an argument for believing that mathematical entities exist (a similar reconstruction of Quine can be found in Chihara 2004). Quine’s argument works by first pointing out the parity of evidence for believing that ordinary sized objects – posited in our common-sense ‘theory’ of the world exist, and for believing that molecules – posited in some of our scientific theories about the world – exist. Then it is pointed out that the evidential grounds we have for believing that molecules exist are similar to those for believing that mathematical entities exist. In each case, posits are postulated because of pragmatic and purpose-oriented reasons.

Concluding remarks

Our discussion pointed to two different directions along which the widespread claim that most current versions of IA are, in some way or other, faithful to Quine’s original ideas should be qualified. This result also emerges from the consideration that the most discussed version of IA at present, i.e. [CIA], is, despite its superficial simplicity

(or maybe just for that reason), ambiguous between two different kinds of argument, one deductive and one ampliative.

On the one hand, as regards our discussion of what we labelled the 'logical point of view' on IA, it turns out that by sticking to notions and theses that Quine respectively employed and endorsed, versions of IA can be formulated that are far less committing than most available versions. Much of the recent debate has focused on whether it is possible both to avoid problems posed by the alleged assumptions of confirmational holism and naturalism of IA, and to formulate IA without these assumptions. But it seems clear – as apparently seemed clear also to Putnam – that this formulation can be obtained, and those problems avoided, without being unfaithful to Quine's thought: it is just a matter of disregarding those theoretical ingredients that overdetermine a valid version of IA. Discussion of holism and naturalism will thus be of relevance to IA only in so far as proponents of IA independently support either thesis. By themselves, they are irrelevant to the question of whether the sought-for realist or platonist conclusion can be obtained by a version of IA that is Quinean in its essential traits.

On the other hand, our discussion of 'theory contribution' has made it clear that Quine's understanding of theory contribution and evidence, is incompatible with current scientific realist strategies for defending scientific realism by means of [IBE] as they understand it. Thus one cannot base a reading of Quine, according to which Quine endorses [IBE] as it is understood by current scientific realists, in the writings of Quine himself. One might well formulate Quine's argument as an argument with the same formal structure as an [IBE] argument, but the notion of 'best explanation' should then be defined in terms of pragmatic value. Of course, within Quine's framework this is of no consequence, since one can construct an argument for mathematical platonism by parity considerations on the basis of what Quine says. But the way in which Quine understands the idea of theory contribution is not in line with how current realists understand it.

All these issues would deserve further inquiry, and we submit that progress can be made in the understanding of the historical and philosophical import of IA once the argument's Quinean heritage is brought into sharper focus.

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The Indeterminacy of Translation: Fifty Years Later

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Abstract

The paper considers the Quinean heritage of the argument for the indeterminacy of translation. Beyond analyzing Quine's notion of stimulus meaning, the paper discusses two Kripkean arguments against the Quinean claim that dispositions can provide the basis for an account of meaning: the Normativity Argument and the Finiteness Argument. An analogy between Kripke's arguments and Hume's argument for epistemological skepticism about the external world will be drawn. The paper shows that the answer to Kripke's rule-following skepticism is analogous to the answer to Humean skepticism: our use of concepts is more basic than, and presupposed by, the statement of the skeptical problem itself.

Keywords

Indeterminacy of Translation, Kripkean Normativity Argument, Kripkean Finiteness Argument, Humean Skepticism, Naïve Realism.

Quine's argument for indeterminacy

Quine's argument for the indeterminacy of translation remains a source of fascination and controversy today, fifty years after its publication. The argument is grounded in Quine's conception of the *objective facts* to which translation is responsible. These, according to him, are strictly limited to those that make up «the overall pattern of associations of sentences with one another and with nonverbal stimulation». (Quine 1960: 27) The significance of the limitations he imposes on the facts to which translation must do justice is apparent in his famous Gavagai example. Suppose that for our informant in the group whose language is to be translated "Gavagai" is prompted by all and only the stimulations that would prompt "There is a rabbit" in us. Both would be prompted, for example, by retinal stimulations caused

by realistic fake rabbits in plausible surroundings, neither would be prompted by retinal stimulations in the presence of real rabbits that were either completely hidden or well-camouflaged, and so forth. In this case, according to Quine, "Gavagai" construed as a one-word sentence would have the same *stimulus meaning* as our sentence "There is a rabbit." And for such highly observational sentences, Quine holds that stimulus meaning is a good approximation to meaning as it is intuitively understood. (cf. Quine 1960, 44)

The problem, according to Quine, occurs when we try to equate terms (e.g. referring expressions or predicates) in the language being translated and our own. This is because the stimulus-meaning of "Gavagai" construed as a *sentence* would be preserved by either of two very different translations. According to the first translation, "gavagai" (construed as a word) corresponds to our word "rabbit." In this case, we can take our informant to mean "is the same rabbit as" when he assents to "gavagai" when we point at the head and when he assents again to "gavagai" when we point at the tail of the same rabbit. According to the second translation, "gavagai" (again construed as a word) corresponds to our "undetached rabbit part." In this case, we suppose that our informant means "belongs to the same rabbit as" when he assents to "gavagai" when we point to the head and again when we point to the tail of the same rabbit. And since Quine thinks that such physical dispositions to respond are the only objective facts there are and that they are exhausted by stimulus-meaning, he holds that translation is indeterminate. (cf. Quine 1960, 51-79)

Quine's examples are never worked out in detail, and there is some skepticism as to whether nontrivial examples of indeterminacy exist. I think, however, that a detailed example can be given based on Quine's thesis that there is no principled distinction between meaning and collateral information (at least where the latter is shared by the community).

"Bachelor" as an Observational Term Example. Imagine an informant – Smith – who applies or withholds the term we are inclined to translate as "bachelor" with complete confidence to people about whom, it seems, he could have no information as to marital status – random strangers in a large city, say. Imagine that two hypotheses occur to us.

Hypothesis (1): Smith uses "bachelor" not with the same meaning we do, but as an observational term to be applied on the basis of an observational gestalt: jaunty step, sharp dresser, air of the *bon vivant*, etc.

Hypothesis (2): Smith means what we mean, but thinks he has some clairvoyant insight as to who is and who is not a bachelor.

Can we, as translators, tell which is the correct hypothesis on the basis of the evidence Quine allows? Smith will, of course, make many mistakes if we think of him as applying "bachelor" and very few mistakes if we think of him as applying something like "stereotypical bachelor gestalt." But we can imagine that Smith's behavior in the face of "counterevidence" (marriage records, say) is the same on both hypotheses. He will be indifferent on the first hypothesis because marriage records are irrelevant to the behavioral gestalt. And we can suppose that he will be indifferent on the second hypothesis because he is so confident of his clairvoyant powers that he assumes the counterevidence must be wrong.

Moreover, it seems that all the questions we could ask to determine which hypothesis was correct might be similarly inconclusive. Suppose we ask, "Are you using "bachelor" as an observational term?" Smith might say no even on the hypothesis that he is because the existence of the theory/observation distinction is controversial and he does not believe in it, because he thinks the term is not observational except for people with special powers of observation, etc. Suppose, then, that we ask, "Do you have special powers?" Smith might say yes even on the hypothesis that he is using "bachelor" as an observational term because he thinks as before that his application of the term requires special powers of observation (not clairvoyance). And he might say no, even on the hypothesis that he means by "bachelor" what we mean because he is resolutely modest, thinks everyone is clairvoyant, etc. And – if we imagine trying to teach Smith enough of our philosophy of language, philosophy of science, etc., to allow us to discriminate between the two hypotheses, we might produce such extreme psychological changes that we would have no confidence we weren't changing the meanings of Smith's terms – including "bachelor."

Kripke's arguments that (contrary to what Quine assumes) dispositions cannot provide the basis for an account of meaning

Suppose we start with Kripke's famous example designed to show that what is involved in our following one rule rather

than another cannot be a matter of our dispositions to respond to new cases.

Plus/Quus Example. Kripke points out that all of our uses of the terms "plus" in the past have involved addends less than some finite number, say N . And he asks what fact about our past usage makes it the case that we were calculating the plus function rather than "quus" defined as follows. For addends both less than N , a quus $b = a$ plus b . Otherwise, a quus $b = 5$. (cf. Kripke 1982, 9)

Kripke gives two arguments that dispositions to behave cannot supply the answer.

Normativity Argument. Dispositions do not capture normativity. Dispositions to answer questions etc, are a matter of what we will do, but meaning is a matter of what we *should* do. (Kripke 1982: 37) This seems to generalize since there has (according to Kripke) to be a fact in virtue of which we meant plus in the past (if we did), and no fact, according to Kripke, could satisfy this normativity requirement. (Kripke 1982: 53; cf. John Mackie's "queer facts" argument in Mackie 1977: 38-42).

Finiteness Argument. We have dispositions to make mistakes, and our dispositions are finite, since our brains are finite. If it is said that we should look at what we would do if we were given more computational capacity, the reply is that this might not extend our dispositions – we might, for example, go insane. (Kripke 1982: 26-27)

This second argument is not impressive as it stands. The possible worlds at which the attempt to augment one's computational capacities has such dramatically destructive results are, it seems, obviously not the relevant ones to look at in characterizing our grasp of the plus function. Of course, they might be the "closest possible worlds" in some sense, and so in some sense definitive of what *would happen* if we tampered in this way with the brain. For it might simply be the case, possibly even a fact of nature, that trying to tinker with the computational capacities of our brains has destructive results at worlds where the physical laws are like the physical laws at this world. But it seems intuitively clear that if we are appealing to our behavior at possible worlds in order to characterize the concept of addition and what we grasp when we think we grasp the rule associated with it, then the worlds to consider will be those at which such irrelevant mishaps *do*

not accompany our attempts to idealize away from our finite limitations.

The problem, then, as Kripke (1982: 28) says explicitly, is really one of circularity. Characterizing the *relevant* possible worlds requires that we use the notion of addition. For the relevant worlds to look at are the ones at which the steps we take in idealizing away from our finite limitations have no irrelevant effects on our dispositions to respond to new problems in addition. And what is meant by 'irrelevant' in this context? It means, of course, irrelevant to our characterization of the concept of addition in virtue of being detrimental to our ability to add correctly. The relevant possible worlds, then, are the ones that introduce no new or special obstacles to our *adding correctly*.

Hume's skepticism as an analogue of Kripke's

There is, I believe, an illuminating analogy between Kripke's argument and Hume's argument for epistemological skepticism about the external world. (Hume 1772: 184) Suppose, as Hume implicitly asks one to do, one focuses on what one is immediately given in one's perceptual experience – an experience, say, *as of* a room with furniture, other people, and so forth. In such a case, it will be apparent that besides the causal explanation of the experience suggested by common sense (that one is in a room with furniture and other people), there are many alternative explanations. It might be, for example, that one is being misled by an evil demon, that one is a brain in a vat, that one is in a virtual reality setup such as *The Matrix*, etc. Thus, according to Hume, there is a logical gap between what one is given in perception and any (a posteriori) proposition about the external world – a gap that can only be bridged by an inference. Such an inference, however, could only be a posteriori, since we cannot reason a priori from effects to causes. Thus it could only be justified by some justified belief about the connections between our sense experiences and their causes in the external world. But, to suppose that we have such a justified belief begs the question against the skeptic and flies in the face of the fact that all we can get in principle is more and more perceptual experience. The conclusion is that we can never be rationally justified in preferring any hypothesis about the external causes of our perceptual experience over any other.

The relevance of Hume's argument to Kripke's discussion of rule following becomes clearer when we see that Hume's argument for epistemological skepticism leads to a version of meaning skepticism. Consider, first, that in order for our words to be meaningful, they must – in addition to being associated with other words – be associated with some extra-linguistic reality to which we have access. On the Humean picture, they would get their meanings through associations with patterns in our perceptual experience. (If I walk around this table, I will see...) But the Humean argument for skepticism about external objects also applies to sense experience. All we are given – all we have access to – are *our present* experiences (sense-data, including those associated with present memories and anticipations). Thus our words for external objects (tables, chairs, etc.) have no grounding except in our present, actual, solitary, momentary perceptual experiences, and this is too thin a basis to ground meaning.

The conclusion is that there must be some direct perceptions of external objects, since this is the only way to block Hume's argument. This is in effect a *transcendental argument* in the sense that it is an argument that we can use to counter skepticism by saying that on the skeptic's assumptions, we cannot have a meaningful language. Thus it is one we can use to depict the skeptic as cutting off the branch on which he stands. In this case, the transcendental argument is also an argument for Direct or Naïve Realism – the thesis that we are given (we perceive) some external objects directly. That is, it is an argument that sometimes we perceive external objects, and there is nothing else we *perceive* in virtue of which we perceive them. This blocks Hume's argument because the latter rests on the assumption that there is always a logical gap between what we are given in perception and any a posteriori proposition about the external world. And Naïve Realism is the negation of that assumption – that is, it is the thesis that sometimes what we are given in perception has logical implications for a posteriori propositions about the external world. Such experience gives us external objects (conditions, etc.) directly, or, to put it figuratively, "takes us all the way out to the world."

The idea of such direct perception of external objects, then, is clearly an attractive one, epistemologically speaking. (And if the transcendental argument is right, it is one that we must accept.) But we must be able to explain the possibility of the direct perception of external physical objects from different perspectives in order to deal

with the demonstrative versions of Frege's problem. Consider Evans' (1982: 84) example of such a problem.

Evans' Ship Example. Imagine someone who points out a window to his right at the bow of a ship and says, "That ship was built in Japan." He then points out a window to his left at the stern of a ship and says, "That ship was not." If, contrary to what he assumes, he has pointed to the bow and the stern of the very same ship, then he has said obviously incompatible things of one and the same object.

Such a person is ignorant of a fact, but need not be irrational. Thus we must do justice to the fact that the same ship has been given to him or her from two different perspectives. And we must do so without making the Humean assumption (which as we have seen leads to meaning skepticism) that what we are actually given (what we see) directly are sense-data and that our access to external objects is only indirect.

The explanation I have suggested elsewhere (White 2004) is in terms of basic action possibilities and *know how*. In such cases we *know how* to point to the bow of the ship, how to move closer to it for a better look, how to direct someone loading cargo onto the deck of the bow etc. The same is true of the stern. But the ship presents different basic action possibilities from the two perspectives. Such *know how*, however, does not involve representational intermediaries that could give rise to Humean skepticism. That is, in at least some cases, we are given external object's transparently – not on the basis of any sense-data that we are given more directly. The following case provides an analogy: Sometimes in a brief glance we are given a friend's distress directly – we do not consciously infer it on the basis of anything we are given more directly, such as the geometry of his or her face or posture, much less on the basis of sense-data.

The Response to Kripke and Quine

On the face of it, though, this does not seem to help with Kripke's problem, since we do not seem to perceive either meanings or mathematical entities like the plus function directly. Direct perception, however, goes with basic actions, and our basic actions are toward the object itself in cases of transparency. We know how to compute the plus function. This *know how* isn't a fact in Kripke's

sense – our know how is directed at the plus function itself, so there is no fact which is independent of the plus function and which justifies our going on in a plus-like way.

The case of meaning is somewhat different, because – to put it figuratively – we look through meanings, not at them. That is to say, we manifest our grasp of the meaning in our know how regarding the thing meant or regarding things with the feature meant, etc. For example, we know how to obey the order, "Paint all the fences," as well as the order, "Paint all the white fences." And operating on the basis of color can be a basic action – e.g., stopping on red.

The answer to Kripke's rule-following skepticism, then, is analogous to the answer to Humean skepticism about the external world. In the Humean case the answer is that we cannot coherently adopt the perspective the skeptic requires us to take – since some objects in the external world are given to us directly in perception, we cannot step back from all of our external-world commitments simultaneously and ask how an inference from our sense-experience (our sense-data) to the nature of the external world could be justified. In the case of Kripke's rule-following skepticism, the answer is that we cannot step back from all the things we mean simultaneously in order to ask what fact justifies our moving from the finite number of examples we have actually computed to the plus (as opposed to the quus) function.

As we have seen, one may have no access to the facial geometry of one's distressed friend independently of one's seeing the distress (all one can say on the basis of the brief glance is that the friend was distressed). Analogously, one may have no access to the finite sample on the basis of which one learned a concept – the most that one may be able to say is that one learned to *add*.

The conclusion, however, is actually stronger than this. For we couldn't possibly describe the finite samples that Kripke talks about independently of our using the concepts Kripke wants to call into doubt. For what right do we have to say that in the past we gave the answer 4 to $2+2$, so any function we were computing must (like plus and quus) give the same result in this case? After all, we gave the answer 4 on a Tuesday, on a day when it rained, when we were in a bad mood, in the past, etc. And there is no reason why a new computation that will necessarily take place in circumstances that are in some respects different has to involve the same answer even for $2+2$.

The point is that our use of concepts is more basic than, and pre-supposed by, the description of the finite set of examples that is

presupposed by the statement of the skeptical problem itself. In this it is analogous to the way in which our perception of some ordinary external objects is more basic than, and presupposed by, the description of the sensory experiences that is needed to state the skeptical problem regarding the external world.

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Quine and the Contemporary Debate on Mindreading

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Abstract

The paper examines some of the questions emerging from the debate on mindreading regarding Quine's legacy and contribution to a new agenda on the issue. Since mindreading is an exercise in folk-psychology, a) which role folk psychology has to play according to Quine? b) was Quine's account of mindreading closer to theory-theory, simulation theory or hybrid theory? c) was Quine a rationality theorist? d) are hybrid-theory and rationality theory incompatible as many would suggest? On the score of the answers to these questions, the paper tries to suggest a Quinean inspired blend of rationality-based and hybrid view-based strategies to explain mindreading.

Keywords

Mindreading, Simulation Theory, Empathy, Folk-psychology, Rationality.

Quine and mindreading: is it an oxymoron?*

How do we understand people? One answer is that we mindread. What is *mindreading*? Roughly, mindreading is the skill of understanding a subject, explaining and/or predicting her thoughts and actions. We manage this task attributing to our target a mind, where this means ascribing to the target mental states of various kind like per-

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ceptions, beliefs, desires, intentions, emotions. It sounds almost obvious that explaining and predicting behavior are capacities essential to human meaningful interactions, to human cognitive and social life.

Given this definition, it might seem difficult to see what could be the relationship between Quine «the behaviorist» and mindreading. Isn't Quine, just the Quine of *Word and Object*, the strongest opponent of every form of mentalism? Isn't Quine the strongest opponent of any ontological commitment to intentional states? That *prima facie* oddity, I hope, is going to become less strident in what follows.

The questions that emerge about Quine from the debate on mindreading are many. Since mindreading is an exercise in folk-psychology, a) which role folk psychology has to play according to Quine? b) was Quine's account of mindreading closer to theory-theory, simulation theory or hybrid theory? c) was Quine a rationality theorist? d) are hybrid-theory and rationality theory incompatible as Nichols and Stich (Nichols and Stich 2003) or Goldman (2006) would suggest?

On the score of the answers to these questions, the paper tries to suggest a Quinean inspired blend of rationality-based and *hybrid view*-based strategies to explain mindreading.

In the second section, the main features of the contemporary debate on mindreading are presented. The debate is understood as featuring, in the end, two main mutually exclusive options: hybrid-theory and rationality-theory. In the third section, answers to questions a) and b) are offered. Quine's argument for the indispensability of folk-psychology is reconstructed and his account of mindreading is analyzed and identified as a version of hybrid-theory in which simulation plays a major role. Quine's effort, within the framework of his naturalized epistemology, in offering a unified account of our understanding of world and other people through an examination of perception is considered as a first relevant legacy to the contemporary debate on mindreading. It suggests a way of claiming that there is no sharp divide between knowledge of the world and knowledge of the other minds and overcoming the traditional distinction between natural and social science.

In the fourth section, answers to questions c) and d) are sketched. A way to find a reconciliation between rationality and simulation is explored through a brief comparison between Davidson and Quine. It is meant to argue for the idea that the Quinean inspired hybrid theory

based on empathy does not exclude the use of normative notions and the appeal to rationality. This is identified as a second relevant legacy to the contemporary debate. Taken together, the two legacies mentioned above could constitute an interesting agenda for the future debate.

The contemporary debate on mindreading: a very brief introduction

When we ask how we do achieve the complex ability or skill to mindread, we find that the contemporary debate features, at least, three different theoretical options: theory-theory of mind, simulation theory, and rationality theory.

Each one comes in different sub-varieties. There are at least two versions of theory -theory: the child scientist theory (Gopnik and Meltzoff 1997; Gopnik, Meltzoff and Kuhl 1999) and the modularity theory (Fodor 1987; Leslie 1987, 1988, 1994; Baron-Cohen 1995). According to the theory-theory view on mindreading when you perform mindreading tasks you use a theory of mind. We have «a rich body of mentally represented information about the mind, and [...] this information plays a central role in guiding the mental mechanisms that generate our attributions, predictions, and explanations.» (Stich and Nichols 2003, 239). The two versions mainly differ in their answer to the developmental question: how do we arrive to achieve the complex skill of mindreading? Is it innate and modular? Or does it evolve from childhood to adulthood through radical changes?

There are also many versions of simulation theory. The core idea is that when performing a mind reading task you use a simulation routine consisting «in putting yourself in the target's shoes». You don't need any kind of theory-like knowledge, you need just the ability to project yourself on the target, to engage in a sort of «pretend play» and, using your own belief-formation, desire-formation and decision-making mechanisms, to see what comes out. The outcome of the simulation process counts as the mental state you will predict to be that of your target, or you will use to explain its behavior. Among the simulationists, Goldman (2006), Gordon (1986, 1995, 2005), and Gallese (Gallese 2001; Gallese, Keysers, and Rizzolatti 2004) differentiate themselves about the extent to which we can say we are using simulation, about the extent of mirroring

processes in simulation, and, above all, about the role of introspection in mind reading. A different perspective on simulation is advocated by Jane Heal (2003), because her approach is characterized by the tendency to be conducted in a less empirical and more a priori fashion and by the idea that simulation is supported by rationality.

Notwithstanding these differences between theory-theory and simulation theory, it is almost becoming a shared view that a good account of mindreading can be afforded by a *hybrid view*, a blend of theory-theory and simulation (Botterill and Carruthers 1999; Stich and Nichols 2003, Nichols and Stich 2003; Goldman 2006). People who agree on hybrid view have different accounts of the exact step in the mindreading process where the hybridization takes place. They have also different accounts of which one of the different mindreading abilities is subserved by simulation or theory.

Stich and Nichols (2003, Nichols and Stich 2003) claim that inference prediction is a mindreading skill subserved by simulation, and that desire-attribution and discrepant belief-attribution are mindreading skills that cannot be explained by simulation, but need theory-like generalization.

Goldman distinguishes between two levels of mindreading: low-level and high-level. He claims that the low-level mindreading skills, like emotions recognition, are simulation processes caused by mirroring processes, even though mirroring does not exhaust mindreading. Goldman claims that there is a high-level mindreading in which we simulate without mirroring. It could be implemented by theory (Goldman 2006: 43-46), because it is a kind of simulation in which previous experience, knowledge and memory play a major role (Goldman 2009).

If we follow this trend, we might say that just two different options are left on the field: *hybrid view* and rationality theory (also called *interpretativism*). The core idea of rationality theory, shared by Davidson (Davidson 1984, 2004) and Dennett (Dennett 1987), is that mindreading someone is to treat her as rational, namely to attribute her mental states such that she comes out as a rational thinker and agent. In his book *The Intentional Stance*, Dennett states:

However rational we are, it is the myth of our rational agenthood that structures and organizes our attributions of belief and desire to others and that regulates our own deliberations and investigations. We aspire to rationality, and without the myth of our rationality the concepts of

belief and desire would be uprooted. Folk psychology, then, is idealized in that it produces its predictions and explanations by calculating in a normative system; it predicts what we will believe, desire, and do, by determining what we ought to believe, desire, and do. (Dennett 1987: 52)

According to Goldman (2006: Chapter 3), rationality theory does not provide a plausible account of mindreading and it is not compatible with a simulationist account. One of the criticisms offered by Goldman is that we cannot find the kind of rationality imagined by rationality theorist in actual agents and thinkers. Goldman's argument are not new. They are supported by the psychological studies on the failure of rationality by Kaheneman, Tversky and others (Kahnerman, Slovic, Tvershy 1982).¹

Curiously enough, both theorists like Goldman and rationality theorists refer to Quine as a predecessor.² Goldman himself underlines places in Quine's works where he assumes a simulationist stance, but he declares that, even if Quine can be viewed as a simulationist, no influence of his thought can be traced back in simulationist theorists like himself, Gordon and Heal (Goldman 2006: 18).³ The influence of Quine on rationality theorists is quite direct and well known. Davidson and Dennett both assume the so called *principle of charity*, evoked by Quine (1960:59), as the key rule to interpret others and attribute them propositional attitudes.

¹ The negative results of these inquiries and their consequences for rationality theories have been elucidated and discussed in Thagard and Nisbett (1983) and Stich (1983, 1985, 1990). Dennett's answer to this objections is that psychological experiments deliberately provoking irrational responses induce a "pathology" (Dennett 1987: 52) in a system that, even tough is not perfect, is still "pretty" rational (Dennett 1987: 50).

² This double aspect of Quine's account have been well noticed by Stich (1983; 1985) and Dennett (1987).

³ This assumption is problematic. Goldman, in fact, quotes Quine's passages about projecting strategy from *Word and Object* in his seminal paper on simulation (Goldman 1989)

Perception, folk-psychology and simulation based hybrid-theory

Quine casts many doubts about propositional attitudes and intentionality in general. In Chapter VI of *Word and Object*, emphatically titled *Flight from intension*, in the crucial section 45 titled *The Double Standard*, Quine recognizes, with Chisholm and Brentano, that the intentional vocabulary is not reducible, because «there is no breaking out of the intentional vocabulary by explaining its members in other terms» (Quine 1960: 220). That irreducibility brings as a consequence a refusal of «an autonomous science of intention (Quine 1960: 221).

As Quine points out in *Word and Object* (Quine 1960: 219) and in more recent writings, if we look at the idiom of propositional attitudes from the austere ontological point of view of science, we face many troubles when we try to include it in our «literal and austere formulation of one's theory of the world» (Quine 1989: 351).

Nonetheless, we don't need to refuse or eliminate the intentional idiom.⁴ In the same section of *Word and Object* Quine opens the way to a different option declaring that he would not «foreswear daily use of intentional idioms, or maintain that they are practically dispensable» (Quine 1960: 221). In fact, Quine claims that the intentional idiom, exemplified by indirect quotation, is not «humanly dispensable» (Quine 1960: 218). And he sketches the psychological mechanism underlying it that we commonly use: «we project ourselves into what [...] we imagine the speaker's state of mind to have been»; we perform «an essentially dramatic act» through which we can «find ourselves attributing beliefs, wishes, and strivings even to creatures lacking the power of speech, such is our dramatic virtuosity» (Quine 1960: 219).

⁴ Quine's attitude is, in this respect, different from eliminativist's project in philosophy of mind. Eliminativists like Churchland (1989) claim that folk-psychology has to be dismissed as a bankruptcy enterprise in favor of scientific psychology. Scientific psychology must replace to some extent our common sense psychology even in every day usage. Quine's principle of ontological austerity may support such a view. But Quine himself never draws similar consequences from his attitude towards folk-psychology. Quite the contrary, he increasingly during the decades remarks its role in his account of mind and language.

Here we can find a first sketch of the structure of that psychological phenomenon Quine, later, would have called «empathy». What is empathy according to Quine? It is the ability of a subject to project itself on mental states of a target, where projecting means the ability of simulating, through imagination, in its own mind the target's mental states.

Even though Quine's reflections on the subject are present as a topic in his earlier works,⁵ a more detailed account and the very use of the notion of empathy are to be found in his latest books: *Pursuit of Truth* (1992, Chapters III and IV) and *From Stimulus To Science* (1995, Chapter VIII). Developing the idea that the intentional idiom is not «humanly dispensable», Quine claims that the command of mentalistic notion such as 'x perceives that p' seems to be «as old as language» (Quine 1992: 61) and that «the handing down of language is implemented by a continuing command, tacit at least, of the idiom 'x perceives that p'». Mentalistic idiom intrudes already at the level of observation sentences, determining an early bifurcation of physicalistic and mentalistic talk. The bifurcation reminds us that «man is a forked animal» (Quine 1992: 62), in Quine's phrase.

If the learning of language is implemented by the «virtual if not literal» mastery of mentalistic idiom (Quine 1992: 61), then it is not surprising that Quine, in *From Stimulus To Science*, claims that the «perception of another's unspoken thought» by means of instinctive empathy is «older than language» (Quine 1995: 89). A confirm of that comes, continues Quine, from child psychology: «an infant of just a few days old responds to an adult's facial expression, even to imitating it by the unlearned flexing of appropriate muscles» (Quine 1995: 89).⁶

⁵ It is worth noticing that the idea that the ability to project oneself in the place of another has to play a crucial role in translation does not appear for the first time in *Pursuit of Truth*. It can be found in *Word and Object* and traced back to *The Problem of Meaning in Linguistics* (Quine 1953) where we can read: «But, as the sentences undergoing translation get further and further from mere reports of common observations, the clarity of any possible conflict decreases; the lexicographer comes to depend increasingly on a projection of himself, with his Indo-European *Weltanschauung*, into the sandals of his Kalaba informant. He comes also to turn increasingly to that last refuge of all scientists, the appeal to internal simplicity of his growing system» (Quine 1953: 63).

⁶ Quine should be referring to the pioneeristic experiments described in Meltzoff and Moore 1977.

The explanation for this phenomenon is the instinct of empathy⁷. It works in our ascription of perceptions, for «we all have – Quine writes – an uncanny knack for empathizing another's perceptual situation, however ignorant of the physiological or optical mechanism of his perception» (Quine 1992: 42).

Empathy is a pervasive phenomenon and – as Quine remarks – it «guides the linguist still as he rises above observation sentences through his analytical hypotheses, though there he is trying to project into the native's associations and grammatical trends rather than his perceptions. And much the same must be true of the growing child» (Quine 1992: 43).⁸

In ascribing to a target that 'Tom perceives that x' we rely on the ability to detect his mental state «by the empathetic observation of the subject's facial expression and what is happening in front of him» (Quine 1992: 62). This ability plays a crucial role both in the field linguist case, for he «empathizes the native's perception that a rabbit has appeared» (Quine 1992: 62) and in the case of the child language learning from his parents. In the latter case, empathy plays a role both in child and adult. The child «does not just hear the sentence, see the reported object or event, and then associate the two. He also notes the speaker's orientation, gesture, and facial expression. In his as yet inarticulate way he perceives that the speaker perceives the object and event» (Quine 1995: 89).

The child needs to read, even though in a way, probably not entirely conscious and not even articulated in a theoretical fashion, what the adult has in mind. This is also true of the adult who, to give his assent to child's utterance, takes note of his orientation and facial expression because what he is interested in it is not the «mere truth of the utterance» but the fact that «the child has to have perceived its truth» (Quine 1995: 89).

It seems that, according to Quine, what the adult has to evaluate is not mere correspondence between world and word, but the fact that the correspondence is epistemically established in a reliable way, the fact that the adult can make a plausible psychological assumption about the child's state of mind.

⁷ See Rainone (2005, 2010) for a discussion of the role of empathy in Quine.

⁸ See note 5.

The same mechanism is at play when we try to figure out what Tom is thinking. Quine treats this case like an extension of perception's cases. When we say 'Tom perceives that the train is late', we have two ways to understand what Tom is perceiving. Tom can tell us about it or we can observe Tom's behavior. He walks impatiently, looks at the clock, looks along the track.

Then, according to Quine, «along with acquiring such habits ourselves, we have learned to observe similar manifestations on the part of others. We are ready to see our own ways replicated in another person» (Quine 1995: 63). In the same way as in child case, our ascription is based on «projecting [...] into Tom's situation and Tom's behavior pattern, and finds thereby that the sentence 'The train is late' is what comes naturally» (Quine 1995: 63).

As we move away from observation sentences, notes Quine, «ascription of perceptions call increasingly for background knowledge and conjecture on the ascriber's part» (Quine 1992: 64). This is indeed the case of ascriptions of beliefs. When we ascribe belief our evidence is similar to that of when we ascribe perceptions but is «usually more tenuous» (Quine 1992: 66). For this reason in ascribing we need to «reflect on the believer's behavior, verbal or otherwise», take into account «what we know of his past», and «conjecture that we in his place would feel prepared to assent, overtly or covertly, to the content clause» (Quine 1992: 66).

Generalizing the case of belief's ascription, Quine remarks that «empathy is why we ascribe a propositional attitude by a content clause» that is supposed «to reflect the subject's state of mind rather than the state of things». So we can say that «the quotational account reflects the empathy that invests the idioms of propositional attitudes from 'perceives that' onward» (Quine 1992: 68-69).

The conclusion of this line of argument, from an ontological point of view, is related with Quine's endorsement of anomalous monism. According to that doctrine «there is no mental substance, but there are irreducibly mental ways of grouping physical states and events» (Quine 1992: 72). Mental predicates interacting one with another engendered «age-old strategies for predicting and explaining human action», namely folk-psychology. So they «complement natural science in their incommensurable way, and are indispensable both to the social sciences and to our everyday dealings» (Quine 1992: 72-73).

Let me now summarize and schematize the main features of Quine's account of mindreading.

Quine recognizes that folk-psychology is indispensable to our mutual understanding. He is inclined to treat it as a «practical» competence (Quine 1992: 46). Its «method» is empathy (Quine 1992: 46). Under the label 'empathy' he subsumes many cognitive skills that precede language. They can be consciously (i.e. in the field linguists case) or unconsciously exercised (i.e. in the learning language child case and in the case of many adults' everyday dealings). These skills are: a) imitation; b) detection of face expression, orientation, gesture; c) joint attention; d) pretense/imagination. All of these are operating in attribution of perceptions which makes possible and support communication and language learning. Quine is inclined to think that 'perceive that' followed by an observation sentence is «the primeval idiom for ascribing a thought» (Quine 1995: 90). We have two ways to get the «perception of another's unspoken thought» (Quine 1995: 89), i.e. mindreading: a) behavior and b) language. If A tries to understand what B has in mind observing her behavior, A's activity has the following structure: i.) seeing A's ways «replicated» in B's ways, i.e. recognizing similarities between A's behavior and B's behavior; ii.) hypothesizing that B's mind is «pretty much like» A's own mind (Quine 1992: 46); iii.) projecting A on B's shoes; iv.) simulating B's situation and behavior; v.) seeing «what comes naturally» (Quine 1992: 63).

The same structure holds when A tries to ascribe to B a belief. The crucial difference is that in belief-attribution case the evidence on which A bases her attribution is weaker. A's reflections and conjectures and her knowledge of B's background and past become more and more relevant. Moreover, in belief-attribution, the «easiest way of determining» B's belief is «asking» her (Quine 1992: 66) and listening and understanding the «arguments offered in support of a belief» (Quine 1992: 67) by B. This obviously implies the use of language.

As far as I can see we are facing again solutions the core of which was already present in *Word and Object*, even though they have been growing more explicit in recent years. Quine recognizes the indispensability of mentalistic idiom and identifies in projection, replication and simulation, in one word, empathy, the strategies we use in mindreading.

The analysis of Quine's account of mindreading, in this section, is meant to give an answer to some of the questions raised in the first section. It gives an answer to question a) about the role of folk-psychology. What should be remarked is the relevance of what we might call the *indispensability* argument: folk-psychology cannot be eliminated. Moreover, it seems that it should be intended mainly as a practical form of knowledge, as simulationist would suggest, and not, strictly speaking, as a theoretical one.

It gives also an answer to question b) about Quine and the theory-theory *versus* simulation theory debate. Which option is he defending? In general, we could say that he is quite near to the *hybrid* theory, a blend of theory-theory and simulation in which simulation plays a prominent role. It is clear, even from the reconstruction of Quine's thoughts on the topic presented here, that he does not give an in depth treatment of the various notions he uses. Readers well acquainted with the contemporary debate on mindreading and social cognition can easily see that each of the notions has had a detailed treatment in the neuroscientific, psychological and philosophical literature of the last three decades.⁹ Quine envisages a way of putting them together, of seeing their relationship, of understanding how they can be integrated in a naturalized epistemology¹⁰. In fact, Quine's interest on mindreading is part and parcel of his effort to answer the crucial question of naturalized epistemology: «how we, physical denizens of the physical world, can have projected our scientific theory of that whole world from our meager contacts with it» (Quine 1995: 16). He is trying to find a unified account of our understanding of the world and other people through an examination of

⁹ In my reconstruction (third section) I have enlisted four skills for empathy in Quine's account: a) imitation ; b) detection of face expression, orientation, gesture; c) joint attention; d) pretense/imagination. Excellent studies on imitation are included in two volumes edited by Hurley and Chater (2005). On joint attention valuable contributes are included in Elia *et al.* (2005). On pretence and imagination see Nichols and Stich (2003) and Currie and Ravenscroft (2002). For an overview on these issues and their interconnections from a neuroscientific point of view see Blackmore and Frith (2003). The various aspects of simulation routine (replication, like me, projecting, simulating) have been studied by the simulationist theorists I have been quoting in the paper. They play also a crucial role in developmental psychology: Meltzoff (2009), Tomasello (1999).

¹⁰ For an introduction to Quine's naturalized epistemology and its historical roots see Zanet (2007).

perception¹¹. As far as I can see, he is inclined to think that there is no sharp divide between knowledge of the world and knowledge of the other minds, as the traditional distinction between natural and social science would suggest. This is his first relevant legacy to the contemporary debate on mindreading.

Empathy and rationality

The second legacy consists in a suggestion that needs to be developed about the way to find a reconciliation between rationality and simulation. What follows amounts also as an answer to questions c) and d) in the first section.

Quine seems to suggest there are differences among various mental states' ascriptions and these are differences in degree of the background knowledge involved. Ascription of perception is the most basic, in a double sense. It is the first one we, when we are children, exercise and it has a deep influence on our language learning. It is the one that calls less for background knowledge and information. To ascribe perceptions we need some relevant cognitive skills and exercise empathy. Beliefs ascription and thoughts ascription call for a different degree of use of background knowledge and of conjectures. The more we want to be successful in ascribing beliefs, the more we need to take into account believer's behavior and background knowledge. But one may ask what happens when we do not have such a knowledge. This is pretty the case of the linguist in the jungle and, at least in a certain sense, the case of the adult trying to understand a child. In such cases we try to project ourselves starting from our common practical knowledge of human psychology. This last consideration brings us to a crucial point. A way of recasting rationality versus simulation debate within Quine's philosophy is reflecting on the *prima facie* tension between the principle of charity and empathy. That tension comes to the fore when we look to Quine-Davidson debate about the issue.

It is well known that Davidson endorses a version of the principle of charity. He claims that a «good theory of interpretation» must

¹¹ I in this paper I focalized the theme of perception in Quine for what concerns the problem of the other minds. For a more detailed treatment of Quine's theory of perception of external world see Zanet (2009).

«optimize» agreement, from the interpreter point of view, between the interpreter and the subject of interpretation. For «it cannot be correct [a theory] that makes a man assent to very many false sentences» (Davidson 1984, 169). Consequently, Davidsonian interpreter should assume the truth of a large part of the subject's beliefs.

This assumption seems to be a direct consequence of Quine's reflections on the same point according to which the “maxim of translation” underlying our understanding of logical connectives in the jungle language is inspired to the commonsense assumption that «one's interlocutor's silliness, beyond a certain point, is less likely than bad translation -or, in the domestic case, linguistic divergence» (Quine 1960: 59).

According to Davidson the entire theory of interpretation «is built on the norms of rationality». When the interpreter applies the theory to «actual agents» he «assigns his own sentences to capture the contents of another's thoughts and utterances». This «process necessarily involves deciding which pattern of assignments makes the other intelligible (not intelligent, of course!), and this is a matter of using one's own standards of rationality to calibrate the thoughts of the other» (Davidson 2004: 129-30). What are the norms we follow in ascribing attitudes? Davidson suggests that «the semantic contents of attitudes and beliefs determine their relations to one another and to the world in ways that meet at least rough standards of consistency and correctness» (Davidson 2004: 114).

According to Davidson we project our own standards of rationality on the target of our interpretation. This seems to be a consequence of the radical interpretation approach: if we don't have any previous knowledge of the subject's language, desires, belief, then we can do nothing but projecting our structure of norms. But even if we concede that we can make a clear sense of what kind of rationality standards we employ, one question is left open: is this the correct picture of projection?

Quine expresses some perplexities about Davidson's picture when he writes that in translation what we want to maximize is «not truth or agreement with us on the part of the native, but psychological plausibility according to our intuitive folk-psychology». And «the folk-psychology involved is very much a matter of empathy». (Quine 1990: 158). We should be ready to attribute animism, in Quine's example, to the jungle people even if that belief is not true or not rational according to our standards. Quine writes:

The translator will depend early and late on psychological conjectures as to what the native is likely to believe. This policy already governed his translations of observation sentences. It will continue to operate beyond the observational level, deterring him from translating a native assertion into too glaring a falsehood. He will favor translations that ascribe belief to the native that stand to reason or are consonant with the native's observed way of life. (Quine 1992: 46)

The linguist will rely also on observation of the local folkways. The child does too, but the linguist is a more seasoned observer. Unlike the child, the linguist will not accept everything the native says as true. He will indeed assume sincerity, barring evidence to the contrary, but he will try as an amateur psychologist to fit his interpretations of the native sentences to the native's likely belief rather than to the facts of circumambient nature. Usually the outcome will be the same, since people are so much alike; but his observation of the folkways is his faltering guide to the divergences. (Quine 1995: 80)

The passages above are pretty clear about the policy that the interpreter should follow. He should ascribe belief that "stand to reason" for the native, are "consonant" with the native way of life. His interpretation must respond to what is likely for the native to believe in consideration of the native's folkways. Trying to fit the interpretation of native's belief to his behavior in his natural environment, in his *ecological niche* in Dennett's phrase (Dennett 1987: 49), can give good results since we assume that people are alike. But when divergences emerge the interpreter's guide is, again, the observation of the native and his people way of life.

It seems that Quine is pointing out a tension between the method of truth (or whatever normative notion can play such a role as, for example, rational agreement) and the method of psychological plausibility. The former seems to appeal to normative standards predetermined by the interpreter. The latter does not seem to appeal to predetermined normative standards: we can say, in a first approximation, that the standards are negotiated with the contribute of empathetic projection. The crucial consequences that seem to me right to take from this is that Quinean interpreter does not deny that some normative standards play a role in belief's attribution. He thinks that the legitimate question is not just *what* are the standards we employ or should employ in interpretation but we may legitimately ask for the standards *of who* we employ. This is strictly connected to more

general questions. What we are interested in when we try to understand people, when we exercise mindreading? How much logicity or illogicity, consistence or inconsistency, coherence or incoherence, are we disposed to attribute to the interlocutor?

In every day cases we are often really good at recognizing a certain degree of coherence even in wrong reasoning, or in false ones. If what matters is understanding, what we need is not just reveal and impute an error to our interlocutor. What we need is to be 'able' to follow 'her' flux of reasoning or 'her' chain of actions. Like as in Quine's example of animist culture, what matters is not that something may appear to us as totally irrational for it could be perfectly consequent for our interlocutor. And we do not need to speculate a lot about jungle people case because adult people who try to make sense of children's behavior and beliefs are often facing the same kind of situation. If I want to have any chance to understand my son or my daughter when they play or ask me something, I have to follow his or her chain of reasoning and acting even if they are guided by standards quite different from mine adult ones.

From this point of view, the Quinean inspired hybrid theory based on empathy does not exclude the use of normative notions and the appeal to rationality. It asks to shift the centre of the evaluations from *me* to *you*, from *us* to *them*. How much we are inclined to do this in everyday dealings can be the subject for further valuable psychological research and philosophical analysis. A further point I can envisage is that the shift can have a noteworthy ethical value for it suggests the way for a full blown and wholehearted comprehension of the others, even if it asks for a downsizing of the ideal of reason and rationality as universal.

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Book reviews

Subjective Consciousness: A Self-Representational Theory,
by Uriah Kriegel. Oxford University Press Inc., 2009, 335 pp.

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In the last decades interest in consciousness from within philosophy of mind has stepped up enormously and with it the number of competing theories. Uriah Kriegel offers in his book 'Subjective Consciousness: A Self-Representational Theory' a provoking new naturalistic theory that combines the idea, made popular by Brentano, that conscious states are about the world but also about themselves with the tools of contemporary analytical philosophy.

Conscious experiences have a subjective dimension, undergoing them feels some way; *it is like something for the subject* to undergo them. When I look at the red apple close to my computer, there is *something it is like for me* to have this experience. This is the phenomenal character of the experience. Kriegel divides the problem of providing a comprehensive theory of consciousness into two different ones by identifying a conceptual distinction among two components of phenomenal character: the qualitative character and the subjective character.

A theory of qualitative character accounts for *what it is like* for the subject to undergo the experience, the concrete way it feels to undergo it. In this sense the qualitative character is what distinguishes the experience I have while looking at my red apple from the one I have while, say, looking at a golf course. On the other hand, a theory of subjective character explains what it is like *for the subject* to undergo the experience. It abstracts from the particular way having different experiences feel and concentrates on the problem of what makes it the case that having a conscious experience feels at all. The qualitative character is what makes it the phenomenally conscious state it is and the subjective character what makes it a phenomenally conscious state at all.

Kriegel regards subjective character as the core of the problem of consciousness, because the subjective character provides the existence condition of phenomenally conscious mental states; it is what distin-

guishes phenomenally conscious states from other kinds of states. Although he offers in chapter 3 an interesting and controversial response-dependent representationalist account of qualitative character, his main concern is the subjective character of experience and I will, therefore, focus on it in this review.

According to Kriegel's Self Representational theory (henceforth SR), a state is phenomenally conscious if it represents itself in the right way. Kriegel's master argument for self-representationalism comes in three steps:

The first one goes from subjective character to *awareness*. Phenomenally conscious experiences do not merely happen *in me*, like the beating of my heart, but are also *for me*. Kriegel maintains that a mental state can exhibit this *for-me-ness* only if I am in some respect *aware* of it. He further suggests that it is plausible that the right kind of awareness be also sufficient for a state to be *for-me* in the relevant sense. In such a case, given that a mental state is phenomenally conscious if it has for-menness or subjective character, a state would be phenomenally conscious if it is a state I am aware of in the right way.

The second step goes from *awareness* to *representation*. This step is supported by the following two principles that Kriegel assumes: i) being aware of something is a matter of representing it and ii) representing something is a matter of being in a mental state that represents it. Accepting them and the result of the previous step one can derive the conclusion that a state is phenomenally conscious if it is adequately represented by some mental state. This line of reasoning is very similar to the one that advocates of Higher-Order Representational (HOR) theories appeal to. What distinguishes SR from HOR theories is the claim that, in the case of phenomenally conscious states, the meta-representation is not performed by a numerically distinct state. So, in a third step Kriegel offers a collection of arguments against HOR theories of consciousness to conclude that a phenomenally conscious state is one that represents itself in the right way.

The idea of self-representation might appear contradictory at first glance and in chapter 6 Kriegel makes a laudable effort to make sense of it and to make it compatible with naturalistic theories of mental content. To this aim, Kriegel introduces the notion of indirect content and makes use of the mereological distinction between complexes and sums. Roughly, the difference between mereological sums and complexes is that the way parts are interconnected is not essential for

the former but it is for the latter. Kriegel concludes that a phenomenally conscious state, M , is a complex state that has two states, M^* and M^\diamond , as proper parts, such that M^* represents M^\diamond directly and M indirectly in virtue of representing one of its proper parts. M is not a mere mereological sum of M^\diamond and M^* , but a mereological complex. The difference between HOR and SR rests mainly on this metaphysical distinction and as one can see in chapter 7 where Kriegel carefully explores and presents some interesting evidence from neurosciences, he makes similar neurological hypothesis with regard to the brain structures that implement phenomenally conscious states as some HOR theories do.

I will now briefly present three worries that, I think, SR left unsolved.

In the first place, it doesn't seem plausible that indirect content enters the phenomenology and Kriegel fails to make the case in favor of it. The problem is that self-representation which determines the subjective character depends on the notion of indirect content and this is hardly compatible with the claim that subjective character is phenomenologically manifest. Kriegel considers this objection and tries to resist it:

My inclination is to contest the claim that the indirect content of a representation does not show up in the phenomenology [...] one might be tempted to hold that a normal perceptual experience [...] of freshly brewed coffee represents the coffee by representing its odor, [...] it seems that both are manifest in the phenomenology. However, by the light of the principle that only direct content enters the phenomenology, the coffee would have to be non-phenomenal. (p.230)

I disagree. It might well be that the coffee is part of the content of the experience, but not part of the content that determines the phenomenal character: the coffee itself is not phenomenologically manifest. Different substances with the same aroma would give rise to the very same kind of experience and even if one concedes that these two experiences would differ in content, they do not differ in the content that determines the phenomenal character of experience, because, taking representationalism for granted, both experiences have the same phenomenal character. If I smell the aroma of a substance X I have never smelled, seen, nor heard about before, I do not understand how X enters into the experience in the sense of being phenom-

enally manifest. Just consider another substance Y that has the same aroma. The experience one has while smelling X and while smelling Y is exactly the same. Therefore, neither X nor Y are phenomenologically manifest despite both being indirectly represented.

My second worry with Kriegel's proposal is that self-representationalism seems to be in tension with the phenomenological observation that motivates the distinction between qualitative and subjective character. I agree that all my experiences seem to exhibit a quality of *for-meness*: they are somehow marked as *my* experiences. That seems to suggest, that the experience I have while looking at the red apple is about the apple, but also somehow about myself. There is often an ambiguity in the use of 'self-representational' (present also in Brentano's writings). The expression 'M is self-representational' can mean either i) that M represents itself or ii) that M represents the self. It seems to me that the only sense in which M being self-representational can be said to be phenomenologically manifest is the second one: the experience is about both the world and the experiencing subject. If this is true, then SR fails to offer an account of the subjective character. Kriegel concedes that the phenomenological observation reveals these facts (p.177), but denies that they are constitutive of phenomenal consciousness: what is constitutive of a phenomenally conscious mental state is having a content like 'this mental state is occurring' and not one like 'I am in this mental state'. Kriegel suggests that, whereas the experience is self-involving in normal human adults, infants' or animals' experiences might fail to be so. Unfortunately, he leaves this claim unsupported.

Finally I want to cast doubts on the idea that self-representation, as Kriegel unpacks it, can guarantee sufficient conditions for being a conscious mental state; in other words, it is not clear that this condition cannot be satisfied by non-phenomenally conscious mental states. We have mental states that are represented by other mental states without thereby giving rise to any phenomenally conscious mental state. Consider a state M_H that represents M_L . Call M_{NC} the aggregate of M_H and M_L and suppose that M_{NC} is a non-phenomenally conscious mental state. Why is not M_{NC} a phenomenally conscious mental state? The only reply available seems to be that M_{NC} , contrary to M , is not a complex and therefore M_{NC} does not represent itself. If we had to appeal to M being phenomenally conscious in order to explain the fact that M is a complex, then SR would not be illuminating at all. So, either there is something in the way that M^* and M^\diamond interact that is

different from the way M_H and M_I interact or SR cannot characterize for-menness. According to SR, a mental state is conscious if it is a complex that satisfies some further condition (one proper part represents the other) but unless we are given reasons why a phenomenal conscious state like M is a complex and M_{NC} is not, SR cannot be considered an account of subjective character, for it fails to explain in virtue of what a mental state is a phenomenally conscious mental state. In chapter 7, Kriegel hypothesizes that M^* and M^\diamond are connected via synchronization of their firing rates. Unfortunately for SR connection via synchronization of their firing rates seems not to be exclusive of phenomenally conscious states. There is empirical evidence suggesting, for instance, that synchronous neurological oscillations are a plausible mechanism of medial prefrontal cortex driven cognitive control independent of consciousness. If M_H and M_I are connected via synchronization of their firing rates, then M_H and M_I are connected the same way that M^* and M^\diamond and it still has to be explained why M but not M_{NC} is a complex.

Kriegel's book is engaging and clear despite the elusiveness of some of the notions involved. It offers conceptual tools and arguments worthy of serious consideration for further research and, although the theory has some important elements that require further elaboration, it presents a compelling alternative in the current debate among theories of consciousness. I strongly recommend this book to anyone interested in the philosophy of mind and in consciousness in particular.

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Physicalism, by Daniel Stoljar. New York: Routledge, 2010, 264 pp.

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This book belongs to the series "New problems of philosophy", edited by José Luis Bermúdez. According to the editor, the aim of the series is to provide a clear starting point for the study of a topic of huge but

recent philosophical interest in such a way that it becomes both accessible to undergraduate students and of interest to professionals. And there is little doubt that Stoljar has written this work having clearly in mind this overall aim.

First, there are many formal aspects of the book that make it reader-friendly: chapters are not long (seventeen pages is the average length); the book is written in short paragraphs; there are no footnotes in the text. Second, contents also follow this same concern: no single philosophical position or notion is presupposed; there is a glossary of key technical terms at the end of the book; each chapter ends with a short summary and a selection of bibliographical recommendations; the argument proceeds slowly, especially in the opening chapters; there are useful recapitulations along the way; key philosophical arguments are presented in such a way that both premises and conclusion are explicitly displayed.

Leaving aside the introduction and a first preliminary chapter, the book can be divided into three clear-cut parts:

- (1) Chapters 2-5 discuss the first problem of formulation of physicalism.
- (2) Chapters 6-8 discuss the second problem of formulation of physicalism.
- (3) Chapters 9-11 discuss the status of physicalism in light of the (skeptical) results of the previous two parts.

For those ignoring the terminology here, let me make clear what is meant by the first and second problems of formulation of physicalism. Nowadays physicalism is understood as a metaphysical thesis (it is noteworthy that it was not so understood during the times of the Vienna Circle, when the term 'physicalism' was introduced in the philosophical lore – incidentally, Stoljar makes some concise but useful historical remarks about the thesis of physicalism, mainly in the first chapter) roughly stating that every empirical entity is, or is determined by, a physical entity. This is of course very rough and vague until one makes clear what are the fundamental physical entities in question and in what consists the relation of metaphysical determination. The first task is what I'm referring to as first problem of formulation; the second, of course, is the second problem of formulation. As it happens, these two allegedly preliminary questions have proved hard to deal with. In fact, most of the recent philosophical

literature arguing for physicalism actually deals with one or both problems of formulation.

It bears mention that Stoljar, our Virgil leading us through the depths and complexities of this new and important philosophical problem of physicalism, happens to be an outright skeptic about the view. His conclusion after discussing the first problem of formulation could not be more appalling for the physicalist, I quote: “there is no version of physicalism that is both true and deserving of the name” (p. 90). And the conclusion of the third part is that physicalism does not play any crucial role in the main philosophical problems and arguments where it is usually thought to be involved, mistakenly so, of course, according to the author. So this is indeed good news for the philosopher who tries to make sense of all these recent metaphysical discussions but has serious doubts about the cogency of the physicalist thesis. But it could not be worse for the physicalist-minded philosopher: in a nutshell, the general conclusion of the book is that physicalism is a red herring in many contemporary discussions in philosophy of mind, metaphysics and philosophy of science.

This is however a clever book. In spite of his overt skepticism, Stoljar manages to write the book in such a way that, as he unfolds his critical argument, he reviews most of the relevant philosophical literature on physicalism. Moreover, he proves able to do so without dwelling on too many details or ramifications of the discussions, which would no doubt make the book much longer and hard to read for undergraduates, and therefore unfit for the overall aim of the series.

However skilled and praiseworthy is the presentation, it also has, inevitably, its drawbacks. For instance, after concluding, at the end of chapter 4, that no version of physicalism is both true and deserving of the name, one wonders, from a purely dialectical standpoint, why should we indulge in another row of four chapters dealing with the problem of formulating physicalism, once we are told this is a hopeless enterprise.

As a philosopher sympathetic with physicalism, there is a wealth of points of discussion and criticisms I’d like to raise about Stoljar’s skeptical argument, but space limitations recommend austerity here. I shall therefore be content if I raise some points concerning the three main parts of the book. In doing so, I hope I will give the reader at least an inkling of the bulk of Stoljar’s case against physicalism, as well as, hopefully so, point to some possible philosophical leaks in the

argument which the physicalist may well use to escape Stoljar's severe verdict.

Let us first concentrate on Stoljar's discussion of the first problem of formulation. In fact, this is the crucial part of his argument. Stoljar's method here does not depart from that of physicalist philosophers. The aim is to find a characterization of the basic physical entities which renders a thesis which is plausible and can be properly called physicalist. And the way of doing so is to consider possible worlds or scenarios in which intuitively physicalism is correct and others in which it looks false. The alleged formulation of physicalism should then accord well with these intuitions: namely, should come out true for the first type of possible scenarios and false for the second.

As an illustration of this way of proceeding, consider an elucidation of 'physical' according to which by that term we understand the sort of entities which feature in commonsense physics, like body, mass, and so on. The reason to discard this as part of a good elucidation of physicalism is that the resulting thesis comes out false in a possible world in which all empirical entities are, or are determined by, the entities introduced by current theories in physics, things like spin or charge. This is so because these are definitely not entities belonging to commonsense physics. Yet, this is a possible world in which physicalism seems to be true. If in the actual world things were as described in this scenario, we would then think that physicalism is a true thesis. The fact that many physicalists take this possible world as being pretty close to the actual one only makes things worse.

A natural move for the physicalist is precisely that of elucidating the basic physical entities as those introduced by current physics (see Melnyk 2003) or a suitable improvement of it (Lewis 1994). Stoljar uses then the same method to discard also this other possible elucidation: he devises a possible world for which the elucidation comes out false while intuitively, he claims, it looks like a physicalist scenario. This is what he calls the "Twin Physics World" (p. 77). This is supposed to be a world in which "every property is necessitated by twin-mass, twin-charge and twin-spin", where these basic properties are "assumed to be of a quite different character to mass, spin and charge" and furthermore they are not "spiritual nor mental nor conform to any paradigm we have of a non-physical property".

Now, it is clear that the formulation of physicalism favored by Melnyk or Lewis comes out false for this Twin Physics world. The crucial question for this formulation is therefore whether physicalism should be judged as being true in the Twin Physics World or not. And Stoljar answers unhesitatingly in the affirmative. He seems quite confident with this verdict (which, as I read his overall case against physicalism, becomes absolutely crucial for his argument), so much so that he only offers a consideration, not a full-fledged argument for it. This consideration is that “physicalism is supposed to be an abstract account of the world, not tied to details of any particular physical theory” (p. 78). Yet I’m afraid that a philosopher like Melnyk would certainly object to it.

To see why, let us follow Stoljar’s argument one step further. Given that a formulation like Melnyk’s (or Lewis’) is to be abandoned since it allegedly renders the wrong result with respect to the Twin Physics World, it follows according to Stoljar that we need a “more abstract” formulation, one which is compatible with such a world. Stoljar’s suggestion in this regard is what he calls “the possibilist version of the Theory View”, which holds that “F is a physical property if and only if F is expressed by a physical theory that is true at some possible world or other” (p. 75). So the formulation of physicalism proposed will state that every property is or is determined by physical properties in the sense of the possibilist version of the Theory View (for reasons I cannot dwell into, Stoljar favors formulations of physicalism in terms only of properties and not of entities of other ontological categories).

Once we concede to Stoljar this move to the “possibilist version of the Theory View” formulation of physicalism, we are only one step away from his skeptical conclusion, since this formulation will certainly prove to be too liberal as it is compatible with overtly antiphysicalist scenarios, such as those in which everything is determined by entities introduced by some future physics which includes consciousness as one of the basic entities.

Yet, as announced, I think there is good reason to resist Stoljar’s move to “possibilist physicalism” (let us call it so by way of abbreviation). Stoljar is well aware that physicalism is intended to be a contingent thesis, and this thought will certainly loom large in his discussion of the second problem of formulation, along chapters 6 to 8 (incidentally, chapter 7 contains a very clear and useful discussion of the intricate issue of the modal status of physicalism). But, during his

discussion of the first problem of formulation, he seems however to overlook that physicalism is also intended to be an empirical thesis, in the sense that it is intended to be a thesis for which we have not merely possible but actual empirical evidence. In other words, the physicalist's reasons, her reasons to defend physicalism, are empirical reasons in the first place.

Now one wonders what kind of empirical evidence we have for a thesis formulated as possibilist physicalism. What sort of actual empirical evidence might we have concerning entities for which we do not have the slightest notion? This is why I'm afraid that a philosopher like Melnyk would, pace Stoljar, refrain from accepting a formulation such as possibilist physicalism and would instead happily accept a formulation of physicalism which is incompatible with the Twin Physics World.

Still, one can find this move also undesirable, and think that something like Twin Physics World should be a physicalist world after all. One could also endorse Stoljar's view that physicalism should not be conceived as a thesis inextricably tied to a particular physical theory. But we should bear in mind that whatever formulation of physicalism we propose which honors these considerations should be a thesis for which certain actual empirical evidence can be provided. This is certainly not a simple task, and seems to trap us in the two horns of the notorious Hempel's Dilemma. So it is Stoljar's inference from the truth of physicalism in the Twin Physics World to possibilist physicalism which looks problematic to me.

In chapter 5 of his book, Stoljar argues that his skeptical argument is actually stronger and better than Hempel's Dilemma. For the reasons just unveiled, I think that precisely the opposite is actually the case. The problem in a nutshell is to find a formulation of physicalism abstract enough, to use Stoljar's words, but also one for which actual empirical evidence can be provided. I myself have suggested a formulation of physicalism which partly rests on a mereological principle according to which properties of wholes are determined by properties of their constituents. The key here is that we seem to have a wealth of empirical evidence for such a general principle (for details, see Pineda 2006).

Let me now say something about Stoljar's discussion of the second problem of formulation. The treatment given in the book to the recent work on the notion of realization is simply too cursory. Stoljar is simply not interested in this. Maybe a good indication of this is the

persistent mistake in the description of Shoemaker's analysis of realization. This occurs twice in the book (on pages 125 and 155) and in both cases it is said that, according to the analysis, the causal powers of the realizer are included in the causal powers of the realized, when in fact it is the other way around. Also the discussion of the functional analysis of realization, defended by Melnyk among others, on pages 123-4, is too quick to say the least.

This dismissal of what many physicalist-minded philosophers take to be the most novel and interesting work on the matter in recent years is symptomatic, I think, of an aspect of Stoljar's conception of the thesis of physicalism which is open to critique. Let me explain.

Stoljar defends that the notion of physicalist determination should simply be elucidated as that of metaphysical necessitation. So according to this what the physicalist has in mind is roughly that for every property *F* instantiated at the actual world, there is some physical property *G* instantiated at the actual world such that, for all possible worlds *w*, if *G* is instantiated at *w*, then *F* is instantiated at *w* (p. 112). This of course has the problem of not ruling out necessitation dualism, the view according to which psychological and physical properties are metaphysically distinct yet necessarily connected. Stoljar's reaction to that is the suggestion that necessitation dualism might be incoherent, if metaphysical distinctness entails only contingent, but not necessary, connections. Yet he acknowledges that this response may not be very convincing. And he acknowledges also that the defender of a realization formulation of physicalism is in a better position here, since she definitely can rule out necessitation dualism.

Now I disagree with Stoljar's views here. Moreover I think that there is something fundamentally wrong in his whole treatment of the notion of physicalist determination. To begin with, realization physicalism is attractive not merely because it rules out cases such as necessitation dualism, but because it tries to elucidate why the non-physical occurs in virtue of the physical, according to the physicalist. Physicalism is not merely a theory about necessary connections among empirical entities; it is rather an explanatory theory. The claim is that everything empirical in the actual world occurs in virtue of the physical. An analysis of realization just wants to elucidate this admittedly unclear 'in virtue of'.

Stoljar discusses, again very quickly, this view of physicalism as entailing an explanatory relation between the empirical in general and the physical. But he dismisses it altogether on the grounds that "it is

quite unclear that the physicalist must, of necessity, require that there is an explanation of why the necessitation relation obtains" (p. 156). But of course she must, otherwise there would be a basic metaphysical fact—the necessary connection between physical and non-physical entities—which would not be entirely physical. Yet surely physicalism entails that all basic metaphysical facts are entirely physical.

To conclude, let me just say a word about the third part of the book. After having dismissed physicalism, in the third part of the book Stoljar tries to argue that this is no serious loss. According to him, physicalism is a sort of philosophical *Weltanschauung*. Stoljar seems to conceive of physicalism as something akin to what Thomas Kuhn dubbed a 'paradigm', only that physicalism is supposed to be a paradigm in philosophy, not in science. As a Kuhnian paradigm, the main attraction of physicalism is, according to Stoljar, that it sets out 'normal problems' (Stoljar speaks of 'placement problems', he never mentions Kuhn, though it looks as if that is what he has in mind) for the philosophical community to resolve, namely, how to account for certain philosophically intriguing notions, like consciousness or intentionality, in terms compatible with the general physicalist thesis.

Consequently, the bulk of the third part of the book is to argue, going case by case (although only the consciousness case is discussed with some depth), that physicalism is not essentially involved in any of these problems and philosophical discussions. So then the outright dismissal of physicalism, Stoljar finally concludes, does not involve any real loss of significance for current philosophical discussions.

Admitting that all this is very suggestive and interesting, I think that it is again misconceived in an important sense. For once again the empirical and explanatory character of physicalism is overlooked. Stoljar's view on physicalism seems so to speak very philosophically endogamic. The focus is on the role of physicalism in certain philosophical arguments and positions. But in fact there seems to be empirical evidence for physicalism which is quite independent from the role of physicalism in philosophical arguments. It is rather evidence revealed by the actual proceedings of current science. If you ask to the layman about the nature of a mental state (I've actually done that) the most common answer is that it is a brain state. Although this view may not be entirely correct, it is in any case a view which relies on empirical evidence. The psychiatrist deals with a depression by prescribing certain drugs affecting the workings of certain neurotransmit-

ters; the neurologist deals with the Parkinson disease by prescribing drugs which try to restore the correct distribution of dopamine in the brain, and so on. As Jerry Fodor once made clear, typically when a special science law faces an exception the scientist descends one level down to look into the laws governing the realizers in order to account for the exception and deal properly with it. This is the sort of empirical evidence which points to something like physicalism. So the dismissal of physicalism, irrespective of what turns out to be the case with current philosophical discussions, should be accompanied with the formulation of an alternative theory that can equally account for all this considerable amount of empirical data. This is what I'm afraid is entirely missing in Stoljar's otherwise excellent discussion of physicalism.

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