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Mixtures and Mass Terms

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In this article, I show that the semantics one adopts for mass terms constrains the metaphysical claims one can make about mixtures. I first expose why mixtures challenge a singularist approach based on mereological sums. After discussing an alternative, non-singularist approach, I take chemistry into account and explain how it changes our perspective on these issues.

Let me prepare my favorite drink. I pour lemon juice, water, and sugar in a glass, and mix them with a spoon. I soon obtain some refreshing lemonade. (Better but more complicated recipes are easy to find.) Have I thereby created something new? Before answering the question, notice that I have used mass terms such as *water* and *lemonade* in order to describe what happened. A central aim of this article is to show that the semantics one adopts for mass terms constrains the metaphysical claims one can make about mixtures like lemonade.

So, in section 1, I present the two main accounts that have been proposed of their semantics. The singularist approach treats them as singular terms referring to mereological sums (e.g. Link 1983). The non-singularist approach is based on the idea that, together with plurals, mass terms have the ability to refer to one or several entities at once (e.g. Nicolas 2008).

In section 2, I present Barnett's (2004) case for arguing that mereological sums are inadequate to capture our intuitions concerning the identity of mixtures over time. His conclusion is that a mixture is indeed something new, a "rigid embodiment". In section 3, I discuss how the non-singularist approach can deal with mixtures. I show that it must treat nouns of mixtures as collective, temporary predicates. On this approach, a mixture is not something new; it is just the plurality of its constituents when they stand in the appropriate relation.

Then, in section 4, I turn to chemistry, the science of matter and its transformations. My guide is the work of Needham (2010). Once his perspective

on chemistry is adopted, the metaphysical issues raised by Barnett concerning mixtures appear in a very different light. This leads me to compare, in section 5, two approaches according to which all portions of matter are nuclei and electrons, or sums thereof.

1 Two approaches to the semantics of mass terms

Two types of account have been proposed for the semantics of mass terms, such as *water*, *gold*, *lemonade*, and *succotash*: the singularist and the non-singularist approaches (see Nicolas 2018 for an overview).

1.1 *The singularist approach*

The singularist approach is very popular in linguistics and philosophy (e.g. Link 1983; Zimmerman 1995). The key idea is that a mass term is a singular term: whenever it is used to refer, it refers to a single entity, since it is used in the singular. This entity is usually identified with a mereological sum (see Steen 2016 for discussion of alternatives).

The notion of sum belongs to mereology, the study of relations between parts and wholes (Cotnoir and Varzi 2021). It can be characterized in different ways, for instance as a least upper bound with respect to the relation of part:

> SUM. *s* is the sum of some entities : ≡ *s* is part of anything that has these entities as parts.

A well-known set of axioms yields *classical mereology*, with in particular:

> UNRESTRICTED SUMS. Any entities have a sum.[1]

Now, let *M* be a mass term and *P* a predicate. If there is some M that P, then the definite description *the M that P* designates something, namely the sum of the M that P. Thus, *the water in the two bottles* refers to an entity, the sum of everything which is water in the two bottles. And *the gold in the safe* designates the sum of the gold in the safe—for instance, the sum of three gold nuggets.

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1 Classical mereology is often formulated in first-order logic, using axiom schemas. Here, for ease of exposition, plural logic is implicitly used. Compare sections 2.1 and 6.1.2 from Cotnoir and Varzi (2021).

1.2 *The non-singularist approach*

The non-singularist approach is put forward by Nicolas (2008), drawing inspiration from Laycock (2006). Mass terms do not admit the grammatical contrast between singular and plural, so one may argue that their use in the singular has no semantic significance. Mass terms are not singular terms, but non-singular terms: just like plurals, they may refer to one or several entities at once.[2]

Let *M* be a mass term and *P* a predicate. The definite description *the M that P* refers collectively to the entities that are some M that P. Thus, *the water in the two bottles* refers collectively to two entities, the water in the first bottle and the water in the second one. And *the gold in the safe* refers collectively to three gold nuggets if this is what the safe contains.

Nicolas proposes a semantics of mass terms based on this idea. This semantics is developed in non-singular or plural logic. In usual logics, such as predicate logic, terms are singular in the following sense. Under any interpretation, a constant is interpreted as one entity, and under any assignment, a variable is interpreted as one entity. By contrast, plural logic has both singular and non-singular terms (Florio and Linnebo 2021). Under any interpretation and assignment, a non-singular term (a constant or a variable) can be interpreted as one or several entities in the domain of interpretation. In particular, a formula consisting of a predicate whose argument is a non-singular term is true if and only if the term is interpreted as one or more entities which collectively satisfy the predicate.

Two things should be stressed. First, the claim is not that mass terms are plurals. It is that mass terms and plurals share a common property, namely the ability to refer non-singularly. Second, in this approach, one does not need to postulate that any entities have a sum, since one can directly refer to these entities themselves.

2 Mixtures and the singularist approach

A mixture is obtained by mixing portions of different types of matter, without creating a new chemical bond between elements. Thus, one obtains lemonade by mixing lemon juice, water, and sugar.

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2 By contrast, according to Laycock (2006) and McKay (2015), mass terms come with their own primitive form of non-singular reference.

As we shall now see, Barnett (2004) argues that the identity of mixtures over time presents a problem for a singularist approach based solely on mereological sums. Indeed, according to him, a portion of a mixture cannot be a sum.

Barnett adopts the following definitions:

> PORTION. p is a portion of a type of matter M $:\equiv p$ is some M.

> SUBPORTION. q is a subportion of p of type M $:\equiv p$ is some M, q is some M, and q is part of p.

> LEAST PORTION. p is a least portion of M $:\equiv p$ is some M and p has no proper part which is also some M.

Thus, the water in a bottle is some water (a portion of water), and the water in the lower half of the bottle is a subportion of water. Like many authors, Barnett supposes that a least portion of water is a molecule.

Barnett holds that sums exist unrestrictedly, and moreover, that they are mereologically constant:

> MEREOLOGICAL CONSTANCY. A sum of entities exists when, and only when, these entities exist.[3] Thus, the sum of all the molecules exists when, and only when, these molecules exist. If one molecule ceases to exist, so does the sum.

Now, according to Zimmerman (1995, sec.8), a portion of a type of matter M is the sum of its subportions, so it should satisfy a more specific property:

> SUBPORTION CONSTANCY. A portion of a type of matter M exists when, and only when, its subportions exist.

Barnett disagrees, distinguishing two types of matter, discrete and non-discrete:

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3 While MEREOLOGICAL CONSTANCY is a popular thesis, it remains controversial. For instance, Inwagen (2006) argues vigorously that sums can change their parts. Consequently, one might prefer to adopt a weak mereology which says little about what happens to sums over time (Donnelly and Bittner 2009).

DISCRETE MATTER. Least portions have no part in common. Discrete matter satisfies SUBPORTION CONSTANCY. Thus, take a portion of water. It is the sum of a great many least portions (molecules), which have no part in common. This portion of water exists when, and only when, these molecules exist. This portion has always the same subportions, each subportion being the sum of certain molecules.

NON-DISCRETE MATTER. Least portions can share parts. Non-discrete matter does not satisfy SUBPORTION CONSTANCY. Thus, take a portion of lemonade in a glass. Its subportions contain lemon juice, water, and sugar, and two least portions can share, for instance, some lemon. When one stirs the lemonade in a glass, at least one of its subportions will disappear because its own constituents (certain portions of lemon juice, water, and sugar) are separated and do not form lemonade together anymore.

For non-discrete matter, Barnett proposes to use the notion of rigid embodiment from Fine (1999, sec.3):

RIGID EMBODIMENT. An entity *o* is a rigid embodiment of a relation R in some constituents *p*, *q*, … :≡ *o* exists when, and only when, *p*, *q*, … stand in the relation R. Thus, a portion of lemonade exists when, and only when, its constituents (lemon juice, water, and sugar) stand in the relation Appropriately Mixed.

Overall, according to Barnett, the ontology of matter involves entities of two different kinds: mereological sums *and* rigid embodiments. This may be worrisome if one wants to minimize ontological commitments or if one is suspicious of rigid embodiments.

   At this point, let's reflect about the relation between metaphysics and language in this discussion. Barnett's hypotheses about mixtures and matter are metaphysical claims which are expressed using mass terms. In particular, the notions of portion and subportion of M are defined using the mass expression *some M*. Moreover, these hypotheses are motivated by a particular understanding of general features of our use of mass terms. As Zimmerman (1995, 55) puts it: "Attention to the presuppositions of our ordinary use of mass terms reveals a 'proto-theory' of masses", involving "central proto-theoretical

assumptions about the referents of mass expressions of the form" *the M* and *some M*. The "proto-theory" in question is an instance of the singularist approach; it presupposes in particular that, for many nouns of matter, a definite description of the form *the M* denotes a mereological sum. In the next section, I turn to the non-singularist approach, which makes different presuppositions. As we shall see, it offers a different view on mixtures, one that incurs simpler ontological commitments.

3 Mixtures and the non-singularist approach

Two broad conceptions concerning the relation between mixtures and their constituents can be distinguished:

> Novelty.   A mixture is something *new* compared to its constituents—e.g. a rigid embodiment for Barnett.

> Mere relatedness. A mixture is *just* the sum or plurality of its constituents *when* they stand in the appropriate relation—cf. Burge (1977, 109–12), for whom a mixture is a temporal phase of an "aggregate".

Thus, when one mixes lemon juice, water, and sugar appropriately:

- According to Novelty, one creates something new (some lemonade), which did not exist before.
- According to Mere relatedness, one does not create anything new; one merely puts certain constituents in an appropriate relation with one another.

As explained below, the non-singularist approach, as developed by Nicolas (2008), is incompatible with Novelty, given the following, extremely plausible assumption about pluralities (Florio and Linnebo 2021, chap. 10):

> Plural constancy. A plurality of entities exists when, and only when, these entities exist. Relatedly, two pluralities are identical if and only if they have the same members. Thus, the chairs that are in the office are the same as the chairs that were in the living-room if and only if these two pluralities of chairs have the same members.

Following Sharvy (1979), let's consider the case of succotash (idealized below), an American dish made of Lima beans and kernels of green corn cooked and served together. Imagine the following scenario:

- At t_0, beans b_1 and b_2 and kernels of corn k_1 and k_2 are cooked together.
- At t_1, b_1 and k_1 are served in one cup, b_2 and k_2 in another. So, each cup contains succotash.
- At t_2, b_1 and k_2 are served in a bowl, b_2 and k_1 in another. So, each bowl contains succotash.

Given this, the following statement of identity over time seems true:

> *The succotash (which was in the cups) at t_1 is identical to the succotash (which was in the bowls) at t_2.*

Can we explain this intuition if we combine the non-singularist approach with the first or the second conception above?

If we combine the non-singularist approach with NOVELTY, we get this:

- The term *the succotash at t_1* denotes the succotash s_1 (made of b_1 and k_1) and the succotash s_2 (made of b_2 and k_2).
- The term *the succotash at t_2* denotes the succotash s_3 (made of b_1 and k_2) and the succotash s_4 (made of b_2 and k_1).
- s_3 is distinct both from s_1 and s_2, and s_4 is distinct both from s_1 and s_2.

If we combine the non-singularist approach with MERE RELATEDNESS, we get that:

- The term *the succotash at t_1* directly denotes b_1, k_1, b_2 and k_2; there are no new entities s_1 and s_2.
- The term *the succotash at t_2* directly denotes b_1, k_1, b_2 and k_2; there are no new entities s_3 and s_4.

Given PLURAL CONSTANCY, the non-singularist approach is actually incompatible with NOVELTY. Indeed, the identity of succotash over time would correspond to the fact that s_1 and s_2 are identical to s_3 and s_4, and so that s_1 is identical to s_3 or s_4 (and likewise for s_2), contrary to the scenario. The non-singularist approach must therefore adopt MERE RELATEDNESS.

For the non-singularist approach, mass terms designating mixtures turn out to be temporary, collective predicates: they hold collectively of certain

entities when, and only when, certain conditions are satisfied. (Similarly, the temporary predicate *child* holds of a person when, and only when, certain conditions of age are satisfied.)

Here, it seems fair to recognize that, according to common sense, when one mixes lemon juice, water, and sugar, one *does make* something new, some lemonade which did not exist before, and which one can now drink, give, or sell. Being at odds with common sense may appear as a disadvantage for the non-singularist approach.

At the same time, as we saw, Barnett is led to distinguish two kinds of matter, mereological sums and rigid embodiments. The ontological commitments of the singularist approach, on Barnett's analysis, are thus more costly than those of the non-singularist approach, which is only committed to pluralities.

In order to make progress, in the next section, I present the conception of chemistry put forward by Needham (2010). As I explain, this conception has important consequences for the issues just discussed.

4 The perspective from chemistry

4.1 *Constancy of matter*

In a chemical reaction, the mass of the reactants before reaction is identical to the mass of the products after reaction. Chemists think that constancy of mass is due to something deeper, namely constancy of matter. Thus, according to 19$^{\text{th}}$ century chemistry:

- Elements (like oxygen and hydrogen) are permanent.
- When put together, they form compounds (like water) and solutions (like lemonade), in which they are actually present. Compounds and solutions are impermanent. The constancy of elements in chemical reactions explains the constancy of mass.

And according to 20$^{\text{th}}$ century chemistry:

- In a chemical reaction, electrons are gained, lost, or shared by elements and compounds (cf. ions, metals, etc.).
- So, what remains constant is nuclei (not atoms) and the overall number of electrons.

This leads Needham (2010, sec.2) to defend the idea that all nouns of matter are temporary predicates, which apply to portions of matter when, and only when, they have certain properties. For instance, let's consider the combustion of hydrogen in oxygen, which gives water:

$$2H_2 + O_2 \rightarrow 2H_2O$$
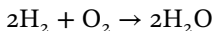
Before reaction, at time t_1, there are two portions of matter, p and q, and their sum, $p + q$. The temporary predicates *hydrogen* and *oxygen* apply to p and q, respectively: hydrogen$(p,t_1) \wedge$ oxygen(q,t_1). After reaction, at time t_2, we still have the same portions of matter.[4] The temporary predicate *water* now applies to their sum: water$(p + q,t_2)$. The common-sense preconception spelled out as NOVELTY in the previous section is thus rejected, not only for mixtures, but for matter of any kind.

4.2 *Liquid water in constant reaction*

A liquid portion of water undergoes constant chemical reactions, and this explains important properties of water.

Thus, there is a continual association of molecules into larger polymeric species (due to hydrogen bonding), and a continual dissociation (Needham 2010, sec.6):

$$H_2O + H_2O \leftrightarrow (H_2O)_2$$

$$H_2O + (H_2O)_n \leftrightarrow (H_2O)_{n+1}$$
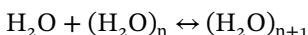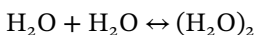
And there is also a continual dissociation of molecules into hydrogen and hydroxide ions, and a continual recombination, together with the hydrogen-bonded clusters:

$$H_2O \leftrightarrow H^+ + OH^-$$

$$(2n+1)H_2O \leftrightarrow (H_2O)_nH^+ + (H_2O)_nOH^-$$
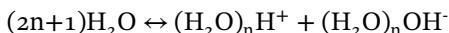
The conductivity of water is due to this: a hydrogen ion attaches at one point of a polymeric cluster, this induces a transfer of charge across the cluster, and

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4 For Needham, a given portion of matter is the constant sum of certain nuclei and electrons.

ultimately a hydrogen ion is released. Other properties of water (boiling at a given temperature, for instance) are also due to such reactions and hydrogen bonding.

So, the microstructure of water cannot be simply characterized as a collection of molecules. And the subportions of a liquid portion of water constantly change, similarly to what happens in the case of a mixture like lemonade.

5 Conclusion

I have examined three approaches about mixtures and mass terms. For two of them, mass terms are singular terms; for one, they are non-singular terms which may refer to one or several entities at once. As explained:

- Barnett distinguishes between discrete matter (a portion of water for instance), which is the sum of its subportions; and non-discrete matter (a portion of lemonade for instance), which is not a sum but something new, a rigid embodiment.
- According to the non-singularist approach, a mixture is not something new; it is just the plurality of its constituents when they stand in the appropriate relation. Nouns of mixtures are temporary predicates.
- From the perspective of chemistry, according to Needham, any portion of matter is the sum of some nuclei and electrons. All nouns of matter are temporary predicates, not just nouns of mixtures.

The non-singularist approach can readily accommodate Needham's perspective. It suffices to take a portion of matter to be the plurality of some nuclei and electrons. The two views then end up being similar. However, their ideologies differ, as do the formal apparatus they use: classical mereology for the former, plural logic for the latter. Are there reasons to prefer one approach over the other? Five come to mind, but none seems decisive.

First, plural logic is a form of higher-order logic, which in its simplest form is similar to monadic second-order logic. So, the fact that mereology can remain first-order may be taken as an advantage: being incomplete, such a theory would be less demanding than plural logic. But this is disputable, since such a theory lacks the resources to say everything a mereologist would like to say. For instance, it cannot say that any entities have a sum. Consequently, several philosophers prefer to characterize mereology using second-order or

plural logic (Cotnoir and Varzi 2021, sec.6.1). Indeed, this is what was done in section 1.1 for ease of exposition.

Second, the non-singularist approach requires one to identify which entities are non-singularly quantified over. Given what we know about chemistry, it is natural to identify these entities with nuclei and electrons. The mereological approach is consistent with this kind of identification, but at the same time, it does not necessarily force one to make an identification. In this respect, the mereological approach may appear as ontologically less restricting. However, Needham's argumentation, summarized earlier, *does* rely crucially on an identification of this kind, namely, to sums of nuclei and electrons. So, both approaches are on a par with respect to the assumptions they make about chemistry.

Third, and relatedly, what about the possibility of "gunk", i.e. indefinitely divisible matter? It is in fact easily accommodated by both approaches (putting aside assumptions about chemistry for the purpose of discussion). Say that a predicate M is "gunky" if, whenever it is true of something, it is also true of a proper part of it. Using sets, there is no difficulty in specifying its denotation: it is the set of entities the predicate M is true of. Mutatis mutandis, the same is true in plural logic. The denotation of the predicate M is just those entities it is true of. This can then be combined with an independently motivated relation of part without assuming UNRESTRICTED SUMS (Nicolas 2008, sec.5).

Fourth, one may wonder whether questions concerning the determinacy of electrons are particularly pressing for the non-singularist approach. If, as argued by S. French and Krause (2006), electrons are not subject to the law of identity, how could one refer plurally to some electrons? However, this would also be a difficulty for the mereological approach as articulated by Needham. According to him, a portion of matter is the sum of some nuclei and some electrons. In classical mereology, a sum of entities requires these entities to be determinate. So, the indeterminacy of electrons seems inconsistent with both approaches.

Fifth, while classical mereology postulates that any entities have a sum, plural logic is, by itself, silent about this. A thirst for ontological simplicity may then lead one to the non-singularist approach. There is no need to postulate that any entities have a sum when it is possible to refer directly to these entities

themselves. But of course, this is unlikely to convince a friend of classical mereology.[5]

Overall, it remains hard to adjudicate between the two approaches. A related way to consider this debate is the following. One-sorted plural logic and atomistic, classical mereology are mutually interpretable (Florio and Linnebo 2021, sec.5.3): each theory can be interpreted in terms of the other. How, then, should one understand their ideological differences?

What is the relationship between these metaphysical issues and language? The scientific knowledge of chemistry and the theoretical considerations that have been invoked are largely foreign to ordinary speakers. They are the concern of metaphysicians. Still, metaphysicians routinely *use mass terms* when making theoretical claims about mixtures and matter. According to semanticists, mass terms are either singular terms that refer to mereological sums; or they are non-singular terms that can refer to one or several entities at once. As we have seen, notably in section 2 and section 3, adopting either of these approaches constrains the metaphysical claims one can make about mixtures.[*]

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5 See Cotnoir and Varzi (2021, sec.5.2.1) for a short survey of arguments about Unrestricted sums.

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An Analysis of Fink's Argument in Favour of Normative Process-Requirements

Leonhard Schneider

This paper analyses and (tentatively) rejects Julian Fink's argument for the existence of normative process-requirements. According to Fink, only process-requirements allow us to give appropriate normative credit to a subject *S* who violates certain state-requirements but is undergoing a process that will eventually lead to their satisfaction. I will show that Fink's argument applies, at best, only to a restricted set of cases—namely, when *S*'s undergoing a process has not resulted in the formation of new mental states. In these remaining cases, however, it is implausible to give *S* normative credit for undergoing the relevant process. Thus, we can assign the correct degree of the corresponding normative property solely in terms of state-requirements. To the extent that this holds, Fink's argument does not entail that there are normative process-requirements.

Normative requirements play an important role in our understanding of normativity (see e.g. Broome 2007). That there are different types of normative requirements (e.g. rational, moral, prudential) is commonly accepted. It remains an open question, however, whether there are such things as normative process-requirements.

This paper takes a closer look at and (tentatively) rejects Julian Fink's (2012) argument for the existence of normative process-requirements. In short, Fink (2012, 132) claims that process-requirements are necessary if we are to "assign fine-grained degrees of a normative property to a subject." To show this, Fink uses the case of two subjects, Jack and Jim, who both violate a certain state-requirement that requires them to have a certain intention. Jack is deliberately undergoing the process of forming this intention, whereas Jim is not undergoing any such process. Fink (2012, 134) argues that the only way

to give Jack normative credit for "moving in the right direction" is to refer to process-requirements.

The central claim of my essay is that if there is a "normative difference"[1] between Jack and Jim, this difference can be explained in terms of state-requirements. Hence, there is no need to assume the existence of process-requirements. Before I come to my claim, I will explain Fink's argument in more detail (sections 1, 2). Afterwards (section 3), I will show that Fink's argument applies, at best, only to a restricted set of cases—namely, when Jack's undergoing a process has not resulted in the formation of new mental states. In these remaining cases, however, it is implausible to give Jack any additional normative credit and, hence, there is no need to invoke normative process-requirements (section 4).

Whether normative process-requirements exist has important implications. Kolodny (2005), for example, argues that not all rational requirements are reason-giving. That is, he argues, the following is not necessarily the case: You have a normative reason to do X if rationality requires you to X. Kolodny's argument presupposes that there are (at least some) rational process-requirements.[2] If there are no normative process-requirements, his argumentation is therefore unsound.

1 Definitions

Fink's argument is supposed to prove the existence of normative process-requirements. But what are process-requirements?

First, Fink (2012, 118) defines the "general-requirement schema":

> GRS. The GRS obtains if and only if, "at t, a normative source N requires of a subject S that S 'Xs'."

On this basis, Fink (2012, 118) defines process-requirements in terms of their content:

> The GRS represents a process-requirement if and only if the proposition "S Xs" signifies a positive relation between S and a process.

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1 I will use the term "normative difference" as a synonym for "a difference between Jack and Jim regarding their 'deserved' normative credit."

2 Fink (2012) analyses Kolodny's (2005) two arguments in favour of (rational) process-requirements and rejects them before he develops his own argument.

Fink (2012, 118) takes "change [to be] [...] a necessary and sufficient aspect of processes." Additionally, the proposition "*S X*s" signifies a positive relation. This means that "*S X*s" is true only if *S* really undergoes a process at *t*. Therefore, process-requirements require subjects to change in certain ways. Conversely, a requirement *not* to undergo a certain process signifies a negative relation. Being required not to undergo a certain process entails, ceteris paribus, that a subject should remain as she is. Such a requirement is a state-requirement.

We can define state-requirements along the following lines:

> The GRS represents a state-requirement if and only if the proposition "*S X*s" signifies a relation between a subject *S* and a state.

A normative state-requirement therefore requires you to be or remain in a certain state.[3]

In his defence of process-requirements, Fink (2012, 130ff) focuses on a particular type of process-requirement: those that require you to undergo a process that "aims at ending in a particular attitudinal state." He refers to these as "teleological process-requirements." Further, he refines his account of teleological process-requirements by stating the satisfaction conditions for such requirements. According to his account, if *S* is under a certain teleological process-requirement *R* at a given time *t*, *S* satisfies *R* if and only if *S* is (successfully) undergoing the process of getting to the required attitudinal state.

Put generally, then, Fink wants to prove the existence of "in-the-process satisfaction process-requirements." I will now show how his argument for the existence of these process-requirements is meant to work.

2 Fink's argument for the existence of process-requirements

Fink (2012, 132) states that "in-the-process satisfaction process-requirements are necessary to assign fine-grained degrees of a normative property to a subject." To argue for his claim, Fink uses the example of Jack and Jim:

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3 The requirement to maintain a state is also a state-requirement, even if the subject "has to do something" to stay in that state. This is because change is a necessary condition of something's being a process, and remaining in a certain state is not a species of changing (Fink 2012, 118).

> Suppose, at t, a normative source N requires Jack and Jim to intend to help their neighbours. However, both violate this requirement, as, at t, Jack and Jim have no intention of helping their neighbours. Suppose further that, at t, Jack and Jim are identical in every aspect save one: at t, Jack is deliberately undergoing a process of (successfully) forming an intention to help their neighbours, whereas Jim is not. (2012, 132f)

Jack and Jim are almost identical. They both fail to be as they are normatively required to be. Indeed, they violate the same state-requirement: Both are morally required to have an intention to help their neighbours. Let us call this intention I_{final}. There is only one descriptive difference between them: Jack is deliberately undergoing a process that will (eventually) lead him to be as he is required (by the state-requirement) to be—let us call this process "F." What does this descriptive difference imply? According to Fink, there is a normative source N that assigns a higher degree of its corresponding property to Jack. The normative source is morality. Hence, Jack seems to be "more moral" than Jim (2012, 133).

Fink assumes that Jack and Jim are subject to the same normative requirements. He therefore argues that we can give Jack more "normative credit" only if he satisfies at least one normative requirement more than Jim. This is because Fink (2012, 133) assumes that there is "a [strictly monotonically increasing] function from requirement satisfaction/violation to the degree of a normative property." The only descriptive difference between Jack and Jim is that Jack is deliberately undergoing a *process* which Jim does not undergo. Hence, Fink concludes that the resulting normative difference must be explained in terms of the satisfaction/violation of *process*-requirements. The *only* possible way to account for the difference regarding their normative credit is to refer to process-requirements.[4]

Therefore, according to Fink, we must assume that at least one process-requirement applies to Jack and Jim if we want to assign different degrees of the relevant moral property to them. Fink (2012, 134) proposes the following

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4 It is crucial to understand that Fink regards the use of process-requirements as the *only* way to express the normative difference between Jack and Jim. Referring to a process-requirement is necessary only if the fact that Jack is deliberately undergoing process F does not lead to the satisfaction of any state-requirement. Thus, Fink must assume that Jack does not satisfy any state-requirements that Jim fails to satisfy. Otherwise, there would be no need to assume process-requirements to capture the normative difference in the first place. (See section 3 for further explications.)

process-requirement: "[A]t *t*, morality requires of both Jack and Jim that each deliberately forms an intention to help his neighbours." Jack satisfies the proposed process-requirement because he is deliberately undergoing the required process of forming the intention while Jim does not (and, hence, violates the process-requirement). Hence, Jack is "more moral."

Fink concludes that it is necessary to assume the existence of process-requirements as they have a unique, essential function—that is, "to assign the correct degree of a normative property to those subjects who violate a set of normative state-requirements, yet who are undergoing a process to redeem this failure" (2012, 135).

3 Restricting the scope of Fink's argument

In this section, I show how narrow the scope of Fink's argument, if it succeeded, would be. This is because most of the cases we naturally think about when saying "Jack is undergoing process *F*" can be captured by state-requirements. Roughly, this is the case if Jack is undergoing a complex process in which "on his way towards I_{final}" he completes several "sub-tasks." That is, by getting towards I_{final}, Jack forms several mental states on the way.

More generally, I assume that we can divide all such complex processes of arriving at an intention into basic steps. Of course, such basic steps can be decomposed further—but not into "mental subprocesses" that are referred to at the personal, folk-psychological level of explanation. Instead, further decomposition to a sub-personal level of explanation yields sub-personal processes. These sub-personal processes are not constituted by mental states anymore, but only by sub-mental states of a subject's cognitive system. Hence, the "input" and "output" of sub-personal processes are not mental states, such as beliefs or intentions.[5]

Now, if Jack has already undergone at least one of the basic steps that he needs to perform to complete the whole complex process *F*, he has formed at least one mental state that Jim has not formed. Thus, they have different sets

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5 This account draws on Wedgwood's (2006) account of reasoning (as a causal step-by-step process) and his notion of "basic step of reasoning." Further, an analogy can be drawn with complex actions (say, making pizza) achieved by means of performing more basic actions (rolling out the dough, cutting tomatoes, etc.).

of mental states.[6] In this case, you can assign a higher degree of the relevant normative property to Jack by referring to state-requirements.

It will be helpful to provide an example of a complex process that Jack could be undergoing to (eventually) reach I_{final}. Using "i" for "Jack intends that" and "b" for "Jack believes that," the basic steps that Jack, in the current example, would have to undergo to reach I_{final} can be described as follows:

First basic step (BS_1):

- (i) (Jack will promote happiness)
- (b) (helping people who are in danger promotes happiness)
- (i) (Jack will help those in danger)

Second basic step:

- (i) (Jack will help those in danger)
- (b) (Jack's neighbours are in danger)
- (i) (Jack will help his neighbours)

We can imagine that Jack has already completed the first basic step of F (and has therefore formed the intention to help those in danger) but not the second step. Thus, Jack is undergoing the process of forming I_{final} but has not completed this process. To capture the normative difference between Jack and Jim, we can now simply postulate the state-requirement to have the intention to help those in danger. Jim does not satisfy this state-requirement. Due to Jack's satisfaction of this further normative *state*-requirement we can give him more normative credit than Jim.[7]

This rough sketch is sufficient to indicate the narrow scope where Fink's argument applies. Jack's undergoing process F is most likely to be constituted or achieved by undergoing more basic processes. If undergoing these processes already led to the formation of mental states, state-requirements suffice to

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6 Fink assumes, and must assume, that Jack and Jim are in the same mental states *before* Jack starts to undergo process F. If this were not so, there would be no need to assume process-requirements in the first place. This is because the normative difference between Jack and Jim could easily be captured in terms of state-requirements (see footnote 4).

7 In the current example, we are interested in Jack's *moral* credit. Hence, the postulated state-requirement that Jack (but not Jim) satisfies is a *moral* state-requirement. There might be, given certain background assumptions, other requirements that apply to the reasoning process in the first basic step, such as some (wide-scope) rational state-requirement. However, we can still stipulate that there is a moral state-requirement to form the intention to help those in danger. (I would like to thank an anonymous referee for pressing this issue.)

give Jack normative credit. Hence, in assessing Fink's argument we only have to look at the restricted set of cases where Jack and Jim still have—even though Jack is undergoing process F—the same set of mental states. That is, we need to consider cases where Jack is currently undergoing the first or only basic step that (partly) constitutes his undergoing process F. In this case, however, I will argue in the next section, it is implausible that Jack deserves more normative credit than Jim.

4 Against process-requirements

Basic steps of undergoing F cannot be decomposed into further mental subprocesses—they can only be broken down into sub-personal processes (see section 3). I assume that such sub-personal processes are *subconscious*, i.e. (as understood here) we do not and cannot realise that we are currently undergoing them.[8] The rationale behind this assumption is that sub-personal processes (as defined in section 3) are not constituted by mental states anymore—only by sub-mental states. And, we do not have, I assume, conscious access to these sub-mental states.[9]

Given my assumptions, it would be highly problematic to give normative credit for Jack's being in subconscious processes and states—the only differ-ence between Jack and Jim.[10] First, it would involve an appeal to a problematic kind of (moral) luck. Given Fink's own assumption, Jack and Jim are identical

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8 An elaborate machine monitoring your brain could perhaps tell you that (and hence make you realise that) you are undergoing such processes. But you are not able to realise that you are undergoing a subconscious process via introspection or in the absence of any other extraordinary assumptions.

9 This assumption can be supported by the following consideration. If we had conscious access to sub-mental states (and, thereby, to the corresponding sub-personal processes), I think it is quite reasonable to assume that state-requirements could apply to these sub-mental states. In this case, the argument laid out in section 3 applies. We could (if it seems appropriate) give Jack extra normative credit for undergoing the process F solely in terms of state-requirements. These state-requirements would require Jack (and Jim) to be in some consciously accessible sub-mental state that is reached while undergoing the (first) basic step of F. Hence, if (contrary to my assumption) we have conscious access to sub-mental states and the corresponding sub-personal processes, no process-requirements are needed. In the following, I argue that if (in line with my assumption) sub-personal processes and sub-mental states are subconscious, there is no need for process-requirement as well.

10 One might argue that this set-up conflicts with Fink's description "Jack is deliberately undergoing a process" (see section 2). That is, one might argue that Jack is not undergoing this process *deliberately*. If this holds true, Fink's example would have to be reformulated (without the notion of "deliberately") to include the current case of Jack's undergoing process F.

save that Jack is undergoing process *F*. Hence, they have the same capacities, dispositions, etc. Further, Jack and Jim are in the same mental states. Hence, whether or not they undergo process *F* is "beyond their control." It is not due to their agency that they are descriptively different. It is therefore difficult to see how we can hold Jack (morally) responsible for being different than Jim and give him additional normative credit. Of course, Jack could be held (morally) responsible for something. For example, he can be responsible for his mental states, capacities, dispositions, etc. that lead to his undergoing *F*. But the point is that Jim has the same set of mental states and the same capacities, dispositions, etc. Thus, Jack is not (morally) responsible *for being different from Jim*. If you still want to hold Jack "more moral," then the normative difference between Jack and Jim must be the result of a problematic kind of luck.[11]

Moreover, Jack cannot decide to stop undergoing *F*, because he is not aware of the sub-personal processes that constitute the fact that he is undergoing *F*. He does not know that he is currently really undergoing *F*. Hence, he cannot even deliberately try to reverse the facts (processes) that are supposed to make him "more moral" than Jim.

Because of these considerations, it seems very implausible that there is a normative difference between Jack and Jim in the current case and, hence, a need to assume normative process-requirements. Where this leaves us will be indicated in the conclusion.

5 Conclusion

According to Fink, only process-requirements allow us to explain the normative difference between Jack and Jim, where both violate a state-requirement but Jack is undergoing a process that will (eventually) lead to its satisfaction and Jim is not. Since it is plausible to give Jack normative credit for undergoing this process, process-requirements seem to have a unique, distinctive function.

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11 Moral luck occurs if "a significant aspect of what someone does depends on factors beyond his control, yet we continue to treat him in that aspect as an object of moral judgment" (Nagel 1979, 26). Jack and Jim's case would be a case of resultant moral luck, i.e. "luck in the way things [e.g. (mental) actions] turn out" (Nelkin 2019, 5). According to Nagel (1979, 25), moral luck poses a problem because it conflicts with the idea that agents are morally assessable only for what depends on factors under their control. To what extent there can be (certain types of) moral luck is a matter of much debate and cannot be discussed here. See Nelkin (2019) for a helpful overview.

I have argued against this notion. My argument appealed to considerations related to (moral) responsibility and (moral) luck. Given the scope of this essay, I cannot address these issues here in detail. I believe, however, that the burden is on Fink to provide further details in order to defend his argument. In any case, I have shown that the set of cases where we would need normative process-requirements, *if* Fink's argument succeeded, is much smaller than one might at first think. We would need them only when one's undergoing a process has not resulted in the formation of new (mental) states to which state-requirements could be applied. To the extent that my argument holds, however, the following claim is true: If there is a difference regarding the normative credit due to Jack and Jim, then this normative difference can be explained in terms of state-requirements. It follows, there is no need to assume process-requirements, at least not on the basis of Fink's argument.*

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Perceptual Learning, Categorical Perception, and Cognitive Permeation

Daniel C. Burnston

Proponents of cognitive penetration often argue for the thesis on the basis of combined intuitions about *categorical perception* and *perceptual learning*. The claim is that beliefs penetrate perceptions in the course of learning to perceive categories. I argue that this "diachronic" penetration thesis is false. In order to substantiate a robust notion of penetration, the beliefs that enable learning must describe the particular ability that subjects learn. However, they cannot do so, since in order to help with learning they must instruct learners to employ previously existing abilities. I argue that a better approach recognizes that we can have sophisticated *causal precursors* to perceptual learning, but that the learning process itself must operate outside of cognitive influence.

> Mentioning that a human has two legs is useful for differentiating a person from a goat or a toaster, but it is hard to think up further specification that does not degenerate into a long disjunction of special cases. Even if such an expansion were successful, the resulting tome would no longer serve the purposes of efficient communication or [...] instruction. (Brooks and Hannah 2006)

If cognitive penetration occurs, then perceptual experience is affected by the content of cognitive states. This entails that perceptual processes are modified by interaction with cognition.[1] Recently, a number of theorists have argued in favor of the *diachronic cognitive penetration thesis* (dCPT), the view that perception is permeated by cognition in the course of perceptual learning

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1 There are a variety of ways of describing this relationship: perceptual representations must bear a "logical relationship" to knowledge (Pylyshyn 1999); perception uses cognition as an "informational resource" (Wu 2013); there is an "inferential" relationship between cognitive states and the outputs of perception (Brogaard and Chomanski 2015). These characterizations are highly ambiguous (Burnston 2017a), but I will take them as read here.

([Cecchi 2014](); [Siegel 2013](); [Stokes 2021](); [Stokes and Bergeron 2015]()). The argument for the dCPT is abductive, and is based on an *enabling* claim. The idea is that there are certain kinds of contents, particularly those corresponding to kinds of objects, that perception on its own lacks the capacity to represent, but that interaction with cognition eventually enables them to do so.

It has recently been pointed out that the notion of "penetration" has gendered connotations ([Ransom 2020]()). In what follows, I will use "permeation" instead, but I mean to reference the same thesis philosophers have investigated (so, the thesis under consideration is the diachronic *permeation* thesis, with the same acronym). My aims in this paper are to articulate the commitments of the dCPT, to raise problems for the view, and to propose an alternative for the possible role of beliefs in perceptual learning. In particular I argue that, if the dCPT is true, then category-specific beliefs must be held *prior* to perceptual learning and must *specify* the learned perceptual content. If these conditions are not met, then another thesis is more plausible, namely that cognitive states serve as *causal precursors* to a purely internal process of perceptual learning. I will argue for the second position, enlisting current perspectives from the psychology of perceptual learning.

In section 1, I'll flesh out the dCPT in more detail, and articulate the priority and specificity conditions. In section 2 I'll discuss perceptual learning, and argue for the minimal claim that category-relevant perceptual learning *can* occur without cognitive permeation. Section 3 then raises objections against the dCPT, the key move being to question whether the enabling role posited for beliefs posited by the dCPT is in fact incompatible with their fulfilling the specificity condition. In order to guide learning, I argue, beliefs must describe contents that subjects can *already perceive*. But if that is so, then they cannot describe the novel contents learned. Section 4 considers and rejects ways of weakening the dCPT to avoid this argument. Section 5 concludes.

1 The dCPT

1.1 *The dCPT*

Cognitive permeation is an *explanatory* thesis. The idea is that there are perceptual differences amongst perceivers, or within a perceiver over time, and the best explanation for those differences is that the contents of cognitive states have modified perceptual processing. While the thesis itself is internal to the philosophy of psychology, its ramifications are potentially widespread.

One reason for these widespread ramifications is the thought that cognitive permeation might be one way in which perception can be *enriched*. If our percepts come to reflect our beliefs or theoretical assumptions, then they represent more about the world than just what they can glean from sensory input. In turn, cognitive permeation has been proposed as one potential explanation for how perception comes to represent higher-level properties, i.e. categories beyond simple perceptual features like shape and color (Siegel 2013). It has been used as one way of explaining the kinds of dispositions developed by skilled perfomers (Fridland 2015), to account for moral perception (Cowan 2014) and, more recently, for a wider range of expertise effects (Ransom 2020; Stokes 2021).[2] Theorists have gone on to consider the epistemic upshot of enriched perception, arguing both for its potential benefits and its potential detriments for perceptual justification (Siegel 2012, 2017; Stokes 2021).

So, the issue of whether cognitive permeation occurs is important for a range of philosophical enterprises. Unfortunately, the extensive debate about cognitive permeation has failed to produce even an agreed-upon definition of the thesis. Theorists disagree on, amongst other things: whether cognitive influence on perception must be direct (Macpherson 2012; Raftopoulos 2015); whether causal interactions between them are sufficient, or whether stronger semantic and computational relationships are required (Burnston 2017a; Stokes 2013; Wu 2017); whether cognitive permeation results in representation of higher-level contents or in changes to lower-level properties (Briscoe 2015; Siegel 2013; Stokes and Bergeron 2015); and, importantly, whether attentional effects count as instances of permeation (Gross 2017; Marchi 2017; Mole 2015; Stokes 2018). Some have even suggested that cognitive permeation should be characterized purely according to its *consequences* for relevant philosophical debates (Stokes 2015).

While there are different characterizations of cognitive permeation, one thing should not be up for debate, namely that the truth of the cognitive permeation thesis would be a *surprising* and *transformative* result for our understanding of the mind. The idea is that certain empirical and theoretical considerations force us to give up the intuitive view that changing our beliefs does not change what we perceive (Firestone and Scholl 2016). So, when considering the cognitive permeation thesis, we should ask whether the kind

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2 The papers by Ransom (2020) and Stokes (2021) were published while this paper was in submission. There is considerable commonality between my conclusion and Ransom's. That said, I focus on slightly different phenomena in perceptual learning than Ransom does, and provide distinct (if compatible) arguments against the dCPT.

of relations discovered between cognition and perception prompt this sort of foundational change to our understanding, or whether more mundane notions can capture the evidence at hand. It is in this spirit that the present paper attempts to assess the issue.

One thread of the argument that has been present since early discussion of cognitive permeation is whether it occurs through perceptual *learning*. Churchland (1988) classically argued that, to the extent that perceptual systems were plastic, they were likely to be infiltrated by knowledge, and hence that perception is likely theory-laden. Recent interest in learning has picked back up, as it is one potential explanation for the existence of higher-level content and for perceptual expertise. And there is strong reason to focus on perceptual learning as a test case. For one thing, learning often involves changes in one's beliefs, and therefore is one possible case in which a change in belief could eventuate a change in perception. Moreover, expertise often involves *training*, wherein one intentionally focuses on certain features of examples in order to develop one's abilities. If perception is changed during this process, then it seems a likely case for cognitive permeation.

Here as well, however, we find a diversity of views. Perceptual learning itself is defined in different ways, sometimes in terms of generated perceptual *abilities*—e.g. of discrimination or generalization—and sometimes in terms of changes in perceptual *contents* (Connolly 2014, 2019; Prettyman 2019). Some have advanced the position that, for certain instances of perceptual learning, learning effects are evidence that cognitive permeation occurs. Stokes and Bergeron (2015), for example, cite cases of *categorical perception*, on which learned categories modify perception, as proof that cognitive permeation occurs during perceptual learning, while Firestone and Scholl (2016; cf. Valenti and Firestone 2019) challenge this view. Others, as mentioned, only take cognitive permeation as one *possible* explanation for perceptual learning (Siegel 2013), or consider that some instances of learning may be instances of permeation and others not (Stokes 2021). And some, indeed, propose that perceptual learning is an *alternative* to cognitive permeation (Arstila 2016; Connolly 2014), i.e. that learning within the perceptual system is an alternative explanation to the permeation thesis.

This is a tangled, almost bewildering set of considerations, and I want to remain neutral to as many of them as possible. I assume that genuine perceptual learning occurs, which modifies perceptual representations and results in novel perceptual abilities. I will further discuss evidence that such changes occur at several "levels" of perception, although I will remain neutral

on whether the higher levels constitute higher-level contents. (For an extended discussion of the relationship between these representations and the debate on higher-level contents, see Burnston 2022). The question then is whether, in any of these cases, cognitive permeation is the right explanation of effects in perceptual learning. I will use the language of perceptual representation and perceptual content, but I do not commit in this paper to any particular way of typing contents. Instead, I will try to describe the representations at work as directly as possible.

I will assume a broadly semantic conception of cognitive permeation at the outset, and I will consider later whether one can abandon this conception. According to the semantic conception, a specific change occurs within perception and is *explained* by the content of the permeating state. This entails that perception has access to or processes the content of cognitive states (Wu 2017; Ransom 2020). Further, it entails that the contents of cognition can explain the changes in the contents of perception. That is, perception operates differently after learning, and the reason for *that specific change* is that it has taken the contents of cognitive states into account in modifying its processing. I further presume that attentional mediation is one good candidate for a mechanism that might bring that change about. That is, cognitive instruction to attend to a stimulus in such-and-such a way is one plausible way in which cognitive permeation could occur. The question I will consider is whether, given the empirical data on perceptual learning, cognitive permeation of this sort is a good explanation for that learning.

This focus on explanation fits well with the abductive nature of arguments that many proponents of cognitive permeation espouse. After looking at a range of effects, these theorists argue, the best account of changes to perceptual experience is permeation (Stokes 2021; Stokes and Bergeron 2015). In particular, I am interested in a variety of *enabling* claim. The idea here is that cognitive permeation—i.e. the resources provided by cognitive contents—allows perception to work in a way that it could not on its own. So, for instance, Stokes and Bergeron argue that, while perception may have evolved a capacity to represent faces, "there is no account to be given about the evolution or plasticity of perception for the Pink Panther or the Coca-Cola icon" (2015, 16; cf. 2015, 325). If perception *itself* lacks the resources to discriminate these categories, then perhaps processing cognitive contents is how perception comes to do so. Similarly, Cecchi suggests that, when perceptual learning occurs during intentional practice at a task, it is because "cognitively induced

architectural modulations enable [...] the visual system to perform the [...] task" (2014, 91).

So, finally, my construal of the dCPT is this. Perception develops novel abilities during the course of perceptual learning, and the explanation for how it does so is that it processes cognitive contents. In the next section I explore the commitments of this kind of view, and articulate an alternative, namely that the role of cognition in perceptual learning is merely to serve as a more-or-less sophisticated *causal precursor* to a purely internal process of perceptual learning. An effect that can be explained as a causal precursor to a change in perception is not sufficient to compel the transformative consequences that cognitive permeation is supposed to have. The position I will argue for is that, although causal precursors can be quite important and specific, the learning that perception does is based solely on interaction with a stimulus-set, not on processing cognitive contents (cf. Ransom 2020).

1.2 *Candidates and conditions*

Given the enabling role in perceptual learning that is posited for beliefs by the dCPT, the first condition that any potential permeator should meet is what I'll call the *priority* condition. Since it is the presence of beliefs that is supposed to enable perceptual learning, those beliefs must be ones that the subject plausibly possesses before the content is learned. Meeting the priority condition, though, is insufficient, since there are many kinds of beliefs that could meet the condition but fail to be good candidate permeators. Here are three kinds of beliefs that are poor candidates for implementing diachronic permeation.

The first is *essentialist* beliefs. Suppose that you know something about the respective chemical structures of jadeite and nephrite, or the facts about phylogenetic history that distinguish whales from fish. While the propositions that are the contents of these beliefs are (at least if you're an essentialist) definitive of the categories to which they apply, the contents of the beliefs themselves have no upshot for how the categories should be *perceived*. Knowledge of chemical structure doesn't help you perceptually discriminate jadeite from nephrite. Similarly, knowledge about cladistics doesn't suggest modifying your percepts of whales or fish in any particular way. This is true even if you hold the beliefs prior to learning to perceive the kind.

A second poor set of candidates is *demonstrative* beliefs. Suppose I hand you an object of a type you've never seen before, and say "this is a glunk."

You might reasonably form the belief that the object you are now holding is a glunk. It is true that the demonstrative "this" *refers* to a particular glunk, but the simple content of the term doesn't contain the resources to help you learn what's *perceptually* characteristic of glunks. Indeed, the belief would play the same role *no matter what* glunks in fact look like. Hence, the demonstrative belief doesn't have the right kind of content to inform perceptual learning. (I'll discuss this example further in the next section.)

A third kind of beliefs, which we might call *denotational* beliefs, have more content than bare demonstratives, but their primary role is still to *pick out the category to be learned*. So, suppose you're about to walk into a room full of objects, and I tell you, "The glunks are on the far left." This belief might help you figure out which are the glunks, by providing a behavioral instruction to look at some objects rather than others. As in the demonstrative case, however, the content of the belief has no resources to inform the actual perceptual category you might learn. Again, the belief will play the same role no matter what perceptual characteristics actually individuate glunks, and hence cannot inform perception how glunks should be represented.

These considerations suggest that another condition is needed, which I will call the *specificity* condition: a candidate permeator must have sufficiently specific content to inform the *particular perceptual content* that is learned. A belief that meets the priority condition but not the specificity condition, I suggest, is best construed as a *causal precursor* to an instance of perceptual learning. A belief or other cognitive state's being a causal precursor to a percept, nearly everyone acknowledges, is not sufficient to make that belief a permeator of the percept. Suppose you know that a particular bird nests only on sheltered alcoves atop very high mountain ranges. This knowledge, along with some sophisticated knowledge about how to climb mountains, might eventuate in your learning to perceive baby birds of that type. But your knowledge of the location of the birds and how to navigate to a place where you can see them does not tell perception anything much about what it should do to recognize that type of baby bird specifically. This is true even if the knowledge is a *necessary* precursor—i.e. if climbing were the only way you could ever gain access to the birds.

Importantly, we now have an alternative interpretation of the "enabling" effect of cognitive states on some perceptual process. On this view, enabling beliefs are only causal precursors—they might point you in the direction of the objects-to-be-perceived, but do not permeate the eventual learned perception. Only beliefs that meet the specificity condition in addition to the priority

condition would force us to read enabling effects in terms of permeation. In section 4, I will consider whether a proponent of the dCPT can reasonably give up on or try to weaken the priority and specificity conditions while still offering an interesting thesis. For now, I will assume that both the priority and specificity conditions must be met by any successful candidate permeator.

Given these considerations, the *prima facie* best candidate for a type of belief that might permeate perceptual learning—and the one that I think most defenders of the dCPT have in mind—is *descriptivist beliefs*. These beliefs have as their content the properties, including the perceptible properties, that members of a kind have. Dachshunds, for instance, are long, brown, and short-legged. Maybe the belief that glunks are (say) *large* and *green* has the right kind of content to permeate perceptual learning, even if demonstrative or denotational beliefs do not. This view has some backing: Leslie (2008) has argued that "generic" beliefs about kinds are fundamental to cognition and learning, and generics often have descriptivist content—e.g. "Tigers have stripes." Reliance on descriptivist beliefs is perhaps the way to interpret Siegel's claim that we learn to recognize pine trees by coming to believe that they have "certain kinds of leaves and structure" (2013, 715), or Stokes' claim that we learn to recognize Mondrian's paintings in virtue of forming beliefs about their "organizational features" (2014, 17). The question is, can these kinds of beliefs meet the priority and specificity conditions?

In the next section I will outline the relevant psychological results on perceptual learning. I'll argue that in certain instances, perceptual learning of categorical content occurs without cognitive permeation. This will then provide the framework for asking whether descriptivist beliefs in general have the right kind of content to implement the dCPT.

2 Perceptual Learning

In this section, I will describe a current perspective on perceptual category learning.[3] The core idea behind the framework is what is called a "morphspace." Perceptual learning, the story goes, forms categories by differentiating and accentuating *dimensions of perceptual difference* between kinds of objects. According to the morphspace view, each perceptual category corresponds to a "space" defined along relevant dimensions. Dimensions

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3 See, e.g. Folstein, Gauthier, and Palmeri (2010); Folstein, Gauthier, and Palmeri (2012); Gauthier and Tarr (2002); Goldstone (1994); Goldstone and Hendrickson (2010); Goldstone, Lippa, and Shiffrin (2001); Goldstone and Steyvers (2001); Gureckis and Goldstone (2008).

can either correspond to low-level perceptual features (size, luminance, etc.) or to higher-order relationships between these features. Important higher-order relationships involve configural (arrangement in space) and associational (correlation) relations between lower-order dimension values. Perceptual learning, on this perspective, can both learn novel dimensions and modify extant dimensions. As categories are learned, dimensions can be "morphed" so that intra-category members are seen as closer along the relevant dimensions.[4]

While I will not make any explicit claims about higher-level content, the distinction between lower- and higher-order dimensions does show that perceptual learning operates at several distinct "levels," which correspond to novel categories. I do suggest that this kind of learning underlies the kinds of recognitional dispositions that some take to be indicative of higher-level content. My purpose in this section is to argue that this kind of learning *can* operate to develop and modify morphspaces in category-specific ways without cognitive influence. This will allow us to then question whether the dCPT is the best explanation of perceptual learning in *some* cases. For the remainder of the paper, I will refer to novel or modified representations, at whatever level, that underlie category discriminations as "categorical contents," where this is meant (for now) to be neutral on the higher-level contents debate (but see Burnston 2022).

Studies in categorical perception rely on training with exemplars, either with or without feedback. Studies without feedback show that subjects can form higher-order dimensions through *mere exposure*. Folstein, Gauthier, and Palmeri (2010) showed subjects a range of cartoon creatures (see Figure 1), where in the training set there were correlations between different lower-level features—for instance, particular wing shapes and head shapes, as well as particular body and arm shapes, might be correlated with each other, where there was no such relation between (e.g.) wings and legs. Having multiple correlations present in the same stimuli set was done to prevent subjects from forming unprompted, specific beliefs about category-membership. Subjects also performed a distractor task (judging how centered the stimulus was on the screen), which was intended to prevent them from forming category beliefs.

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4 This can sometimes correspond to a *loss* of discriminatory capacity *within* a category. In general, perceptual category learning is a generalization and discrimination tradeoff.
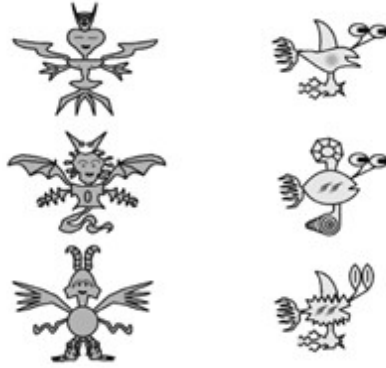
Figure 1: Stimuli from Folstein, Gauthier, and Palmeri (2010)

In the experiment, subjects were capable of picking up on the higher-order correlations in the stimuli. This was shown by a secondary task, in which subjects had to categorize novel examples. If categories in the second task matched the correlations in the exposure set, subjects learned them more quickly than if the categories did not match those correlations. If the controls worked, then subjects were capable of this kind of learning even if they formed no category-specific beliefs. Folstein, Gauthier, and Palmeri's (2012) interpretation is that it is possible for perception to form novel higher-order dimensions purely through "statistical perceptual learning," without influence from beliefs. Similar results have been shown for other kinds of stimuli (Fiser 2001; Burnston 2020).

Importantly for what follows, there are cases where category-specific feed-back is provided to perceivers, and this feedback plays a role in learning, but where, I will argue, the feedback does not meet the specificity condition. Figure 2 is an example from a wide range of studies in which subjects learn to differentiate objects along *arbitrary* dimensions (Folstein, Gauthier, and Palmeri 2012; Goldstone and Steyvers 2001; Gureckis and Goldstone 2008; Jones and Goldstone 2013). The experimenters created a morphspace of faces by taking four distinct faces, and creating exemplars (each square in Figure 2) that continuously blended each of their features. Subjects in these studies were shown exemplar pictures, and told whether each was an "A" face or a

"B" face, where As and Bs were defined according to the arbitrary vertical line in the center of the space.
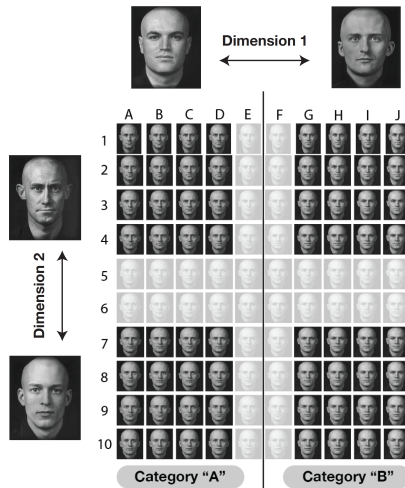
Figure 2: From Gureckis and Goldstone (2008).[5]

A variety of results from this kind of paradigm suggest that subjects learn to differentiate the novel dimensions of the space. For instance, after learning to make the discrimination, the dimensions transfer to new stimuli and categorizations (Goldstone and Steyvers 2001), such that subsequent discriminations along the previously learned dimension are easier than along other dimensions. Moreover, these learned representation affect *similarity judgments*. Across a range of types of similarity judgment, subjects tend to treat within-category members as more similar to each other after training than they did before training. In the example from Figure 2, this involves differentiating and then morphing the category-distinguishing horizontal "Dimension 1."

Hence, in this and other cases, perceptual learning both forms novel dimensions and stretches the morphspace along those dimensions to accentuate the difference between categories. But this kind of learning also modifies representations of lower-level features. Consider two cases. On one, subjects might

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5 Image courtesy of Rob Goldstone.

learn to accentuate discriminations made along already differentiable lower-level dimensions. On the other, subjects might learn to differentiate *between* lower-level dimensions that they could not previously tell apart. Goldstone (1994) investigated both types of changes.
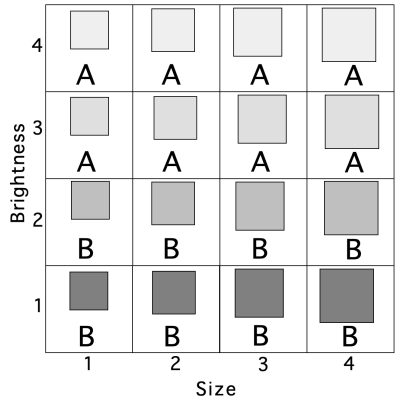
Figure 3: A morphspace for lower-level properties. From Goldstone (1994). [6]

As shown in Figure 3, Goldstone (1994) constructed a simple morphspace of squares comprising two lower-level dimensions, brightness and size. He then tested a variety of different categories defined in the space. For instance, size might be relevant and brightness irrelevant (i.e. drawing the categorical line between 2 and 3 on the x-axis) or vice versa (i.e. drawing the categorical line between 2 and 3 on the y-axis). Take just the brightness-relevant, size-irrelevant case. In this case, subjects learned, through feedback of the type discussed above, which squares belonged to which category. After training, their discriminations along the brightness dimension were *heightened* while their discriminations along the size dimension were *diminished*. That is, they became more sensitive to differences between levels of brightness and less sensitive to differences between sizes.

Goldstone (1994) also created a morphspace of squares based on levels of brightness and *saturation*, rather than size. What is interesting about this case is that, while brightness and saturation can be independently manipulated by an experimenter, subjects do not normally perceive them independently.

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6 Image published with permission from author.

That is, they cannot selectively attend to one rather than another, or make discriminations along one independently of variation in the other. Goldstone trained subjects in a similar way on these stimuli, separating them into categories based on either brightness or saturation, and training subjects on exemplars with feedback. Somewhat amazingly, subjects *do* in fact begin to differentiate the dimensions, showing similar (although smaller magnitude) learning effects as in the brightness and size case.

So, learning can both differentiate new dimensions, and modify existing dimensions to notice and accentuate category differences. There are now two questions to pursue. The first is whether the subjects' new representations can be considered genuinely perceptual, and then the second is whether they are cognitively permeated.

I suggest that there are two sets of interlocking reasons to consider the learned abilities here as due to changes in perceptual representation. First, notice that it is possible to form these representations *absent* category-specific beliefs. In the mere exposure case of Folstein, Gauthier, and Palmeri (2010), subjects have no prior beliefs about what will constitute the categories, and the control task and multi-associational structure are set up specifically to prevent subjects from forming those beliefs during learning. If these manipulations work, then perception can learn categorical content even with no relevant beliefs about the category.

Moreover, even when subjects are asked to reflect on their category judgments and describe them, their explanations are often coarse grained and map poorly to the representations that guided their judgments. For example, Goldstone and Steyvers report that subjects sometimes use abstract language to describe the categories, including such statements as "Faces in [category A] were happier" (2001, 135). But this is clearly not specific enough to have informed their judgments. Subjects presumably can already discriminate happy from unhappy looking faces, but this is not, prior to training, detailed enough for them to discriminate *these* categories of faces from each other. Moreover, Goldstone (personal communication) notes that different subjects sometimes use similar descriptive language even if they have learned different categorizations, thus proving that their beliefs do not discriminate the categories, or at least that they are not required to do so in order for subjects to learn the categories.

The second set of reasons stresses the *structure* of these learned representations, i.e. their dimensional structure. In these studies, there is a continuous pattern of variation amongst the conjunction of features that comprise the

examples. What subjects do is learn to represent this pattern of variation, suggesting that the representations learned have a kind of *metric* structure (Burnston 2017a, 2017b). While it is true that, according to the morphspace framework, the dimensions can be morphed to accentuate category difference, this morphing is done within the metric structure—that is, what is modified is the distance metric between examples along the relevant dimensions, and this is what underlies the changes in similarity judgments.

Arguably, representations of this type do not meet some standard conditions on concept possession, such as the generality constraint (Beck 2014). If one thinks that a way of distinguishing the conceptual from the non-conceptual is in terms of the structure of representations, and thinks further that the way to distinguish the perceptual from the cognitive is in terms of the conceptual/non-conceptual divide, then one will be strongly motivated to view these representations as perceptual.[7]

Moreover, this way of thinking corresponds with some traditional motivations in the non-conceptual content literature, which has historically focused on perceptual content. Notice that subjects develop the ability to apply new categorical concepts demonstratively—e.g. "that's an A face." But, this ability requires a *previously existing* representation which serves as the ground for that demonstrative reference (see further discussion below). This kind of point has been used by defenders of non-conceptual content to combat the idea that all perceptual content requires demonstrative concepts (Roskies 2010).

I consider these reasons to be mutually supportive rather than decisive. But recall the dialectic here—proponents of the dCPT posit genuinely *perceptual* contents which are the result of cognitive permeation. Hence, there should be strong motivation for the dCPT proponent to accept these learned representations as perceptual. The question, then, is whether these cases are instances of cognitive permeation. I suggest that whatever beliefs subjects have are inadequate to meet the specificity and priority conditions.

Consider the mere exposure cases first. If the manipulations and controls worked, then in this case subjects simply had no category-relevant beliefs prior to learning, and the resulting representations could not be due to permeation.

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7 Beck (2014) argues that representations of this type can be *cognitive* but *non-conceptual*. He bases this argument on analogue magnitude representations which, he argues, can be abstracted from any particular instance of magnitude judgment. The kinds of representations I am discussing here, conversely, cannot be so dissociated from their instantiations. I discuss this at length in Burnston (2022).

Next, consider the face case, in which subjects do in fact form beliefs during the learning process. In particular, based on the feedback, they are in a position to form a series of demonstrative beliefs, such as that a particular example was an "A" face. In the last section, I suggested that demonstrative beliefs of this sort do not have the right kind of content to meet the specificity condition. Let's consider this a little further.

There are two ways of individuating belief contents, narrow and wide. Speaking *very* loosely: narrow contents correspond to what the subject is prepared to *do* in virtue of a belief—i.e. the effect that the belief has on other psychological processes and behavior. Wide contents correspond to the extension of the belief. Neither way of individuating demonstrative belief contents supports a reading in terms of cognitive permeation.

The narrow content that the demonstrative belief has is very sparse. It can only convey something like "treat this as an A face." Even a *series* of demonstrative beliefs of this sort, corresponding to a set of A faces, can only result in something like "treat these all as A faces." But this content has no upshot for the *perceptual* recognition of A faces. Knowing that a set of objects should be treated as belonging to the same category doesn't say anything about the perceptual space that they share. Moreover, simply forming a series of demonstrative beliefs about already-seen exemplars does not, on its own, say anything about how the category should be morphed or extended to novel exemplars, but this is precisely what the learned perceptual representation does.[8]

So, the narrow individuation for demonstrative beliefs won't secure a result of permeation. Wide individuation fares no better. On the wide individuation, we could construe the demonstrative belief as conveying the content that the particular face is a member of the set of all A faces, where perhaps one defers to the experimenter to determine the extension of the belief. That's nice enough, but again simply referring to the set does nothing to inform the ability that subjects actually learn, which is to *recognize* that a novel example is a member of the set. So, the demonstrative beliefs that subjects might form on the basis

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8 Now, I am not trying to deny the importance of labels in general—it has been shown in many instances that labels can provide powerful perceptual/attentional cues (although they do not have to be semantically specific to the object to do so, cf. Lupyan and Spivey 2010). Simply labeling something, however, does nothing to say how that object should be perceived, hence the purely demonstrative function of these beliefs. *After* one can perceive the category, a label can provide a powerful attentional cue, but the label itself does not instruct perception to modify its processes in any particular way (Burnston 2017a).

of these instructions cannot meet the specificity condition. As discussed in the previous section, the best way to describe the role of the beliefs is as causal precursors. The demonstrative beliefs provide a behavioral instruction to look for commonalities among a set of objects. Perceptual learning then does the work of actually forming the discriminating representations.

One objection to this view would argue that subjects rely on *tacit* knowledge. One might suggest that, in mere exposure cases, the controls were insufficient to rule out the forming of tacit beliefs about what individuates the stimuli. And, in the feedback case, perhaps subjects develop tacit knowledge about how to apply their new concept of an "A face," that they fail to articulate when asked, but which shapes perceptual learning nonetheless. On this objection, subjects' tacit beliefs might permeate perceptual learning in these cases.

I will consider this objection further in section 4. For now, there are two points to be made about it. First, it is non-trivial to articulate the tacit knowledge objection in a way that is not question begging. Recall that proponents of the dCPT *must* admit that there are perceptual learning processes that result in novel perceptual content. I have suggested here that, at least in important cases, these processes can happen without cognitive permeation. Insisting that the process *must* be due to tacit beliefs in spite of the arguments above sounds suspiciously like a definitional claim that learning must be due to permeation. A definitional claim is out-of-keeping with the kinds of empirical causal arguments put forward by the proponents of the dCPT.

Second, invocation of tacit beliefs is often motivated by dispositionalist concerns, for instance the fact that people are inclined to assent to many more propositions than those for which they are likely to have explicit, stored propositional representations [e.g. "Neither cats nor dogs are numbers"; see Schwitzgebel (2015)]. But dispositionalists are *not* committed to a particular underlying psychological nature of the mechanism that produces the disposition, and thus are not inimical to the possibility that the dispositions are underlain by perceptual states. Given that neo-empiricist accounts of knowledge are at least on the table in cognitive science, it is illicit to simply *assert* that the presence of tacit knowledge means there is an influence of a propositional state on a perceptual process.

Neither of these points is decisive. All I hope to have suggested here is that an appeal to tacit knowledge can't just be a trump card in this debate. It has to be accompanied by specific claims about the contents of those beliefs and how they affect perceptual learning. I will give reasons in the following sections

to suggest that the dCPT proponent has no way of formulating this kind of proposal that will meet the priority and specificity conditions.

Even if you grant me all this, I have only established that cognitive permeation doesn't occur *in these cases*. Proponents of cognitive permeation, however, generally don't insist that perception is *always* permeated, only that it is *permeable*. That is, they suggest that in *some* cases perception is cognitively permeated. Perhaps, even if I am right about these cases, cognition permeates perceptual learning in other cases. Indeed, proponents of the dCPT often invoke particular kinds or particular types of learning/expertise for which permeation is a likely explanation of learned categorical content. The perceptual abilities of arborists or art experts might fall into this category, Siegel (2012, 2013) and Stokes (2014) propose. And Stokes and Bergeron (2015), as discussed above, suggest that perception cannot, on its own, learn to represent particularly novel kinds such as cultural icons.

In the remainder of the paper I will question whether, even in cases of expertise and highly novel kinds, the dCPT is the correct account of how perception comes to represent categorical content.

3 Against the dCPT

3.1 *Setup*

The dCPT posits that certain instances of perceptual modification cannot be explained by citing purely internal processes of perceptual learning. This is what underlies the abductive inference that, in certain instances, cases of perceptual learning must be due to cognitive permeation. As mentioned, one of the motivations for this view is that learning processes are often highly mediated—they involve intentional, knowledge-based learning that requires explicit belief formation and practice. In the previous sections, I suggested that there *is* empirical evidence that perception can learn to distinguish categories of objects, and that this involves changes to perceptual representations at multiple "levels," but questioned whether this process *must* be a result of cognitive permeation. I also offered an alternative, on which beliefs are merely causal precursors for an independent process of perceptual learning.

In this section, I consider whether the dCPT is likely to be the best explanation of perceptual learning, even in cases of novel, socially mediated kinds, or kinds requiring expert training. Importantly, the morphspace framework developed above has been extended to artefactual kinds such as cars

(Folstein, Gauthier, and Palmeri 2012) as well as to kinds involving developed expertise, such as subspecies of birds (Tanaka, Curran, and Sheinberg 2005). We can thus ask whether, for these kinds of cases, the processes discussed above require cognitive permeation. In section 3.2, I seek to loosen the supposedly close connection between category knowledge and categorical perception, by suggesting that our descriptivist beliefs regarding kinds are very frequently *equivocal* with regards to perceptual categories. In section 3.3, I make a stronger argument that the enabling role posited for descriptivist beliefs by the dCPT is in fact *incompatible* with their meeting the specificity condition. If I am right, then the causal precursor view is the better interpretation of how prior beliefs interact with perceptual learning.

3.2   *Equivocal Descriptions*

The dCPT suggests that perceptually learned representations are due to permeation of perception by descriptivist beliefs. Let's start to assess this claim by considering the category "Smurfs," a cultural artefact if ever there was one, and one that Stokes and Bergeron list as a good case to be explained by the dCPT. What kind of beliefs might one have about Smurfs *prior* to learning how to perceive the category? Here's one candidate list: "Smurfs are small cartoon people"; "Smurfs wear red hats"; "Some Smurfs have beards". (Perhaps one forms these beliefs by talking to a neighbor about their kids' favorite cartoons, or something.) Alas, this descriptivist content won't discriminate between Smurfs and Gnomes of the sort pictured below.

Figure 4: Small cartoon people in red hats.

Anyone with a modicum of experience with these two different cartoons will be able to discriminate Smurfs from Gnomes. But, patently, the list above does not make a discrimination between these two kinds, since it applies equally

to either case. Hence, this set of descriptivist beliefs, even if it met priority, would fail specificity—the descriptivist belief that the learner has is equivocal between the two perceptual categories, but the perceiver precisely learns to distinguish between those categories.

The natural response here is to posit that learners have more detailed beliefs about the categories. One might need the belief that Smurfs are *blue* (although one would then need the further clarification that it is their *skin*, and not their shirt, that is blue) to pick out the Smurfs rather than the Gnomes. Here is the problem with this. The dCPT suggests that perception on its own *cannot* come to discriminate the relevant kinds. As such, the view is committed to the idea that subjects who do learn the perceptual discrimination have prior beliefs that *do* distinguish the categories, since these are what enable the subsequently developed perceptual ability. As cases become more fine-grained, this requires that the grain of subjects' prior beliefs becomes comparatively more fine-grained. The idea that all learners have prior beliefs at the requisite level of grain before learning to recognize categories is, empirically speaking, just unlikely to be correct. Consider a more-fine grained discrimination between "Gnomes" and "Littl' Bits":

Figure 5: Gnomes (left) and Littl' Bits (right).

Despite the significant similarity in terms of their features (they both wear pointy hats, the girls wear red dresses, the boys blue shirtsleeves, they both have small noses and big cheeks, etc.), anyone who has watched a lot of both "David the Gnome" and "The Littl' Bits" can easily make the perceptual discrimination between one and the other. The proponent of the dCPT is forced into a pretty awkward stance regarding these categories. They must insist that anyone who learns to make this discrimination, *prior to learning to do so*, has sufficiently fine-grained beliefs to inform the perceptual categories. I submit that, in my own case, this is not what happened. At least, it certainly wasn't the case that, when I was four years old, anyone sat me down and gave me a thorough list of things to look for before I learned how to recognize

these categories. The proponent of the dCPT is forced into trying to articulate a process by which novice perceivers come by very fine-grained perceptual beliefs prior to learning, or they must admit that perceptual learning forms the discriminating representations on its own, at least in many cases.

If perception *can* learn to discriminate these categories, at least in many cases, absent permeation, the inference to the best explanation posited by the dCPT is strongly questioned. On the other hand, the view that beliefs are important causal precursors to perceptual learning, I suggest, is fully compatible with the datum that our descriptive beliefs are often equivocal between kinds that we can easily discriminate perceptually, at least after some learning. Consider the beliefs one is actually likely to have prior to category learning in everyday contexts. Sure, this set may include some beliefs describing general perceptual features. But it is also likely to include beliefs about when and where to find the objects. One might know, for instance, that *The Smurfs* is on Nickelodeon at 4, whereas *David the Gnome* is on at 5. This could help you discriminate the objects without requiring fine-grained descriptivist beliefs that meet the specificity condition.

What I am ultimately suggesting is that descriptivist beliefs are just *another variety* of denotational beliefs. Just like I might say, "Glunks are the objects on the left," I might say, "Smurfs are the little cartoon people in red hats that are on Nickelodeon at 4." What each set of beliefs does is help you *locate the set of objects to be learned*, so that these can be treated as exemplars for the category. But to play this role, all descriptivist beliefs have to do is enable you to sort the Xs from the non-Xs. And so long as the beliefs are descriptive *enough* to sort the exemplars appropriately, they will do the job. That is, they can do the job without specifically describing how Xs should be represented perceptually. As with the birds-on-top-of-mountains example discussed in section 1, this prior knowledge can play an important role in learning, but doesn't need to do so via describing to perception the content it should learn. A similar point goes for guided attention. All that one has to do attentively is focus on the right objects so that perceptual learning can go to work, and if I am right, then that's all that descriptivist beliefs do. (This is the point of the quote in the epigraph from Brooks and Hannah 2006).

This is true even when descriptivist beliefs are very specific. Return again to Figure 5. You may or may not have noticed that Littl' Bits, but not Gnomes, have little red dots on their cheeks. My telling you this might indeed help you look at them and say "Ah, ok, these are the Gnomes and these are the Littl' Bits." But notice how far short the content of the dotted-cheeks belief comes

of describing the *perceptual* category learned, at least if the morphspace view is correct. If the morphspace view is correct, then what is definitive of the *perceptual* category of Littl' Bits is not just their red cheeks—it's a complex set of correlations and configural relationships between lower-order properties (the shape and spacing of facial and bodily features, etc.). *This* content, however, is not described by your knowledge of red cheeks. Again, it has served as a (albeit important) causal precursor to perceptual learning.

All I have established so far, however, is that in many quotidian cases we can expect descriptive beliefs to fail to be fine-grained enough to describe learned perceptual content, and hence to deny the view that perception on its own cannot learn to discriminate categories. In many ways, expertise-through-training is the best case for the dCPT theorist. In these kinds of examples, learners are often specifically encouraged to look for certain features of the objects that fall within categories. In the next subsection, I offer an argument that even this apparently obvious case is misleading. Indeed, I will suggest that the enabling role posited by the dCPT is actually *incompatible* with descriptivist beliefs meeting the specificity condition.

3.3 *An Incompatibility Argument*

The kind of argument I take to most strongly speak against the dCPT suggests that, precisely *because* of the enabling role posited for descriptivist beliefs by the dCPT, they cannot meet the specificity condition. Put informally, the concern is this. Learning requires leveraging *extant* abilities in service of developing new ones. Training and expertise indeed involve describing the objects to be recognized, but in order to help the trainee, these descriptions must tell subjects what to *do*. That is, they must invoke them to focus on certain objects or properties they can *already* perceive, on pain of being unhelpful for learning. But if descriptions name already-perceivable content, and what subjects learn is *novel* perceptual content, then the specificity condition cannot be met. (This is, basically, a variety of Meno's paradox for the cognition-perception interaction.) Here is the argument in more formal gloss:

(1) If the dCPT is true, then prior descriptivist beliefs that meet the specificity condition enable perceptual learning.
(2) In order to enable learning, descriptivist beliefs must have as their content perceptual features that subjects can already perceive.
(3) Perceptual learning results in novel perceptual content.

(4) Novel perceptual content is distinct from content that subjects can already perceive.

Therefore,

(5) The content of the beliefs that enable learning is distinct from the perceptual content that is learned. (From 2 and 4)

Therefore,

(6) It is not the case that prior descriptivist beliefs that meet the specificity condition enable learning particular perceptual categories. (From 5)

Therefore,

(7) It is not the case that the dCPT is true. (From 1 and 6 via Modus Tollens)

The argument hangs on premise (2), and the move from (5) to (6). Premise (3) is granted by all parties, and premise (4) is trivial. Premise (1) is true so long as the dCPT theorist accepts the priority and specificity conditions. Step (5) follows from the lack of identity from learned to novel content, (6) from that claim plus a strong version of specificity, and then (7) is a simple deduction. This section will focus on premise (2). I will then consider in the next section whether a dCPT theorist might attempt to challenge (1), or the step from (5) to (6), by abandoning or weakening the conditions.

Premise (2) is intended to drive a wedge between the enabling thesis and specificity, and show that the two cannot be maintained together. The idea is simply that any instruction- or belief-based learning must leverage our extant abilities in the service of generating new abilities. So, if contents of beliefs are to enable learning, then they must name and enlist already extant perceptual abilities—my telling you that Smurfs have red hats, big shoes, beards, etc., will avail you not at all if you can't already perceptually recognize those features. But if prior beliefs must name features that a subject can already perceive, and the content they learn is distinct from that content, then the content of the beliefs cannot be specific to the content that is learned. Let's take novel dimensions first, and then modifications to already represented dimensions.

Consider the category of A faces from Gureckis and Goldstone's study. There are a number of features of these faces that subjects can already perceive—noses, ears, eyes, etc. But naming any of these features, or even a conjunction of them, is not the same as naming the dimensions that subjects actually learn,

because these dimensions are higher-order ones that capture the configural and correlational structure of the space. One cannot describe these dimensions in terms of simple feature descriptions, of the type that subjects are likely to already be able to perceive. Indeed, it is hard to describe them in simple terms at all, as evident by the poor job that subjects do in describing the dimensions they've learned. Thus, descriptivist beliefs that might actually help learning aren't going to do so by describing the novel contents that perceivers learn.

Similarly, there is evidence that learned perceptual categories *outstrip* descriptive beliefs. Brooks and Hannah (2006) had subjects learn to recognize a set of cartoon creatures on the basis of a description. They then had them perform a transfer task on objects that *equally met the description*, but varied in their overall similarity with the training set. Subjects performed better when the similarity was high, which showed that their learning outstripped the descriptions they had been given.

This dynamic is seen even more clearly in the Goldstone case which distinguished brightness from saturation. Given that subjects cannot perceive these dimensions independently, prior to training, simply telling them, for instance, that category "A" squares are distinguished by their saturation, cannot help them learn how to discriminate the squares. What should they look for to see the difference in saturation? Given that saturation, for them, is bound up perceptually with the orthogonally varying brightness, the instruction doesn't help. However, with the demonstrative feedback and training over exemplars, they can learn to differentiate this dimension. The process doesn't *require*, and indeed would not be *helped by*, descriptions of the category-relevant features.

This leaves us with the best-case scenario for the dCPT theorist, on which descriptions name already-perceivable features, *and* these features are definitive of the category. So, in the brightness versus size case from Goldstone (1994), subjects could easily be told that category As are bigger and Bs smaller, or that As are bright while Bs dark, etc. But, as I suggested at the time, this seeming content specificity is misleading. The behaviors that are *novel* are the ability for increased discrimination along these dimensions. But what the descriptivist beliefs describe—i.e. to sort by size or brightness, is an ability that the subjects *already had* before that learning. So, while what is learned is semantically consistent with the instruction, the instruction doesn't tell perception *how* to represent the stimulus—the novel ability arises due to the repeated interaction between perception and the stimulus (cf. Ransom 2020). Hence, the content of the descriptivist belief in fact "runs out" before

perceptual learning takes over. (I mean this semantically, not temporally. It is likely that we continue to use our beliefs to sort during learning.)

Again, there are empirical cases in which this exact dynamic plays out. Sowden, Davies, and Roling (2000) had inexperienced subjects study radiograph images, with the instruction that abnormalities in these images show up as dots. It is well-established that expert radiographers have more fine-grained perceptual sensitivities than novices in these kinds of stimuli. After training with the images, subjects in fact showed increased sensitivity—they could perceive dots at lower levels of contrast than they had before. However, *failure of transfer* shows that this ability clearly outstripped the descriptive belief about dots. Subjects who were trained on positive contrast (brighter than background) dots did not improve on discriminating negative contrast (darker than background) dots, and vice-versa. But this is just to say that the ability they learn is not specified by the beliefs they had, since the same belief resulted in *distinct* abilities (positive versus negative contrast sensitivity) depending on the stimulus. Again, the content of the descriptivist belief named an already extant perceptual ability (recognizing dots), and it was perceptual engagement with the training set that actually produced the learning effect. These kinds of effects have been posited to be relevant to expertise in general (Brooks and Hannah 2006).

A last, and famous example, is that of chicken sexing. Biederman and Shiffrar (1987) showed that one could short-circuit the extensive exemplar training usually required by chicken sexers by (i) showing subjects *where* to look for the "genital bulb," which is the distinguishing feature of males and females, and (ii) telling them that male genital bulbs were *convex* and female ones *concave*. The fact that performance improves almost immediately has been taken as a way of arguing that no perceptual learning *at all* occurs in this case (Pylyshyn 2003). Indeed, as Biederman and Shiffrar note, it is the fact that the visual system is *already* well-attuned to convexity and concavity that allows this immediate improvement. What is generally glossed over in discussion of this case, however, is that performance improved, but not fully to the level of experts. This is because there are range of specific concave or convex shapes that experts can discriminate. Rather than suggesting finer-grained descriptions, however, Biederman and Shiffrar suggest that the instructions would have to be *combined* with extensive training on exemplars. That is, the content of the description, and its aid in learning, is exhausted by describing features subjects can already perceive.

So, I suggest that the enabling role posited for descriptivist beliefs by the dCPT is in fact incompatible with their meeting the specificity condition. And if so, then the dCPT misdescribes the learning process—the better account is one that restricts beliefs to causal precursors. I wish to emphasize that the arguments in the last two sections have been about the possible roles of descriptivist belief contents, and therefore don't rely on whether the beliefs are explicit or tacit. In the next section, I will consider several objections. First, I will consider whether a more sophisticated view of tacit knowledge could save the dCPT here. Then I will consider whether there is a substantive version of the dCPT that could weaken or abandon the priority or specificity conditions.

4 Objections

4.1 *Sophisticated Tacit Belief*

One might complain that I have oversimplified the contents of the beliefs at play here, by taking them as analogous to simple linguistic descriptions. There is an informal and a formal way to cash out this objection. The informal way involves noting that, at some level, there *is* a match in content between the prior belief and the resulting perceptual state—both represent the category "gnomes." One might suggest that simply by suggesting that the set of objects can be grouped together, the specificity condition is met. Or, one might say that there is *more* content to the belief than simply to label a set of objects as "gnomes." Perhaps the "gnome" content carries with it a range of deeper connotations that perception can use in learning the category.

These informal responses fail because, along the lines given in section 1, they fail to explain how the belief's content could instruct perception how to represent the category, and it is just this kind of informational relation that is posited in the enabling claim. The first version gives no account of how the grouping label informs the specific content that constitutes the perceptual category—that is, the morphspace. The second version, which posits a richer content to the "gnome" belief than the ones I've listed, is obscure. One would have to theorize about what these richer connotations might be, and, if what I have said so far is correct, they cannot consist in any of essentialist, demonstrative, or descriptivist beliefs.

The more formal way of pushing the objection would appeal to Bayesian and other hierarchical generative approaches to perception to push against my

rejection of tacit knowledge in section 2. While proponents of such views don't agree on their upshot for cognitive permeation (Brössel 2017; Hohwy 2013; Vance and Stokes 2017), they do suggest that both perception and perceptual learning are kinds of abductive inferences that take top-down information into account. Hence, someone might be tempted to claim that, rather than the lexicalized beliefs I've been discussing, diachronic permeation comes about due to the role of top-down knowledge in model-based learning.

Still, however, appealing to tacit knowledge of Bayesian priors in support of the dCPT runs into problems specifying the content of the tacit beliefs that are supposed to permeate perception. Consider two possibilities. First, the beliefs involved in perceptual learning might be descriptive beliefs with probabilities attached to them [although these kinds of contents have also been attributed to perception itself; see Morrison (2020)]. So, one might believe that if an object is a glunk then it is green and round with probability P. This kind of belief pretty clearly will not solve the problem, since the attached probability does not add any *perceptual* content to the belief. If "green and round" is not sufficient to describe novel perceptual contents without the probabilistic modifier, then having the probability attached does not change anything.

Second, the priors might be couched in a representation that *directly describes the feature space*. In Tenenbaum and colleagues' model of face recognition, for instance, the priors are encoded in a feature-space of lower-level features such as face shape, pose, and lighting conditions, and conditional probabilities defined over those parameters constitute a probabilistic representation in the face space (Yildirim et al. 2015). But the problem is now apparent: the feature spaces that define categories in the examples above are not complete until learning has occurred. So, we can't represent the relevant category via prior knowledge of the categorically relevant dimensions of the feature space because, prior to learning, the feature space does not distinguish the relevant categories. And given the result that perceptual learning *does* generate novel features and dimensions, the prior knowledge will not describe those dimensions.

It is thus telling that, when Bayesians model perceptual learning, they often combine generative Bayesian models with more bottom-up deep learning ones (Salakhutdinov, Tenenbaum, and Torralba 2013; Yildirim et al. 2015; cf. Buckner 2018). According to Tenenbaum and colleagues, this allows for "a bottom-up latent variable recognition pipeline for our generative model" (Yildirim et al. 2015, 2). And this is for good reason—Bayesian models are limited by their need to enlist "a priori" (read, already known) variables to

describe the domain, whereas bottom-up networks are not. And "committing to the a-priori defined feature representations, instead of learning them from data, can be detrimental" for novel stimuli or tasks (Salakhutdinov, Tenenbaum, and Torralba 2013, 1).

4.2 *Weakening Priority?*

We can generalize the discussion of Bayesian models above to assess whether a proponent of the dCPT could attempt to abandon or weaken the priority condition. I don't believe that abandonment is an option, since if X enables Y, then it seems obvious that X must precede Y. One might attempt to weaken the condition by positing that beliefs and percepts are developed *in tandem*, for instance by generating new descriptivist beliefs and checking them against the data in an iterative hypothesis-and-test method.

This can't work as a way of defending the dCPT, because the same situation described in the previous sections would arise in terms of the *generation* and *confirmation* of the hypotheses. Suppose that values along some dimension X are definitive of a perceptual category. If one can already perceive X, then one is in a position to both generate and confirm a hypothesis about category membership. But in this case, both the generation and confirmation of the hypothesis are being based on already existent perceptual capabilities, and not on the generation of novel representations.

On the other hand, suppose you can't already perceive X. If you cannot perceive it, according to the morphspace framework, that means you can't differentiate it from other dimensions. So it would not be available to you as a distinct hypothesis from your experience. Now, you might know independently that there is a dimension X, or someone might tell you about it, or you might guess that there is one. In this case, you could form the hypothesis "I wonder if it is X that distinguishes these exemplars," but since you cannot discriminate X, you are in no position to tell if it is really X that determines between examples. The only way you could *perceptually* confirm this hypothesis is by coming to discriminate the dimension. And as suggested above, this learning is not informed by the descriptivist belief.

4.3 *Weakening Specificity?*

One might be tempted to argue that I've foisted too strong a specificity condition on the dCPT. In particular, the step from (5) to (6) in the incompatibility

argument seems to imply a *very* strong notion of specificity. One might contend that the dCPT theorist can reject the argument by abandoning specificity (thus rejecting premise (1)) or weakening it (thus denying the move from (5) to (6)).

Proponents of cognitive permeation are often non-committal about how close a semantic relationship, in addition to a causal relationship, is required for an interaction between cognition and perception to count as permeation. Siegel (2013) is satisfied with the idea that cognitive and perceptual states might have "close" contents. Stokes (2015) has offered a definition of cognitive permeation that doesn't define the notion in terms of content relationships at all, but instead in terms of whether a causal relationship between a cognitive state and a perceptual process is "internal" and "mental." Recently, he has argued that these kinds of internal connections can be mediated by attention (2018).

So, the question on the table is whether the specificity condition can be weakened or simply abandoned. The main worry about this move is that it risks trivializing the dCPT. We often must have *some* relevant beliefs about a category prior to learning to perceive it—consider the birds-on-a-mountaintop case again. Without something like a specificity condition, all of the essentialist, demonstrative, and denotational beliefs discussed in section 1 will count. If one really wants to posit that my knowledge that "glunks are on the left" permeates my eventual learned perceptual category for glunks, it's hard to legislate against it—it's certainly a logically possible move. However, this kind of influence on perception is neither surprising nor particularly informative about cognitive architecture.

The situation looks worse when we think about more general knowledge of categories. Consider my belief that ostriches are flightless. If I have this belief before I can perceptually discriminate ostriches, it certainly will provide some general constraints on how I come to learn to perceive them. I will only bother, for instance, looking at objects on the ground. (And things can get worse than that; consider "ostriches are objects.") If *these* instances count as permeation, then no one would have disagreed with the dCPT in the first place.

This suggests that some degree of semantic relevance or coherence is required for the dCPT to hold. If one wanted to weaken the specificity condition without abandoning it, one would have to posit some degree of semantic coherence more strict than the cases above, but more permissive than the specificity condition. One could, for instance, suggest that the prior beliefs

must describe particular perceptible properties. However, it is highly unlikely that there is a principled way of drawing this distinction. All of the beliefs I've mentioned have *some* upshot for perceiving categories—for instance, the belief that baby birds of a certain type live on mountain tops means something about the kind of perceptual surroundings they're likely to be found in.

One might go further with this, perhaps by saying that descriptivist beliefs can only name lower-level perceptual properties that adhere to the bodies of category members. But the only possible justification for restricting the candidates this way would be an intuition that these contents are *more specific* to the perceptual content that's learned. And, given that weakening specificity is what the dCPT proponent is purportedly trying to do here, that is an odd move at best.

Lastly, consider purely causal construals, and in particular causal construals that posit attentional mediation between cognition and perception. I suggest that the trivialization worry holds in these cases as well. In particular, the worry about semantic specificity is replaced by a worry about causal specificity. Suppose that you are going to sit stock-still while I present you a lineup of objects, and I tell you that the glunks are on the left. You will likely covertly attend to the objects on the left. And this cognitively-mediated process will contribute to your ability, with enough practice, to recognize glunks. But if this is not a sufficiently close relationship in the semantic case, it's hard to see why it is on a purely causal story either. Similarly, many loosely connected beliefs might be causally prior to perceptual learning. The causal version of the dCPT would, similar to the semantic version, have to posit a way of constricting candidate permeators, or risk trivializing the thesis.

On the contrary, the position that I've defended, on which cognitive states are causal precursors to purely perceptual learning, need make no arbitrary distinctions of this sort. There is nothing wrong with causal precursors being more or less specific, and thus focusing us on more or less particular characteristics and more or less constricted sets of objects. Indeed, our doing so might play an important role in perceptual learning. It just needn't be done via permeation.

5 Conclusion

A correct view of perceptual learning should recognize that we do in fact sometimes have descriptive, demonstrative, and denotational beliefs about objects prior to learning to perceive them. I'm not entirely sure, as I sit here,

what broccoli rabe looks like. But I am pretty sure that it's green, that it's the kind of thing I can find at the grocery store, and moreover that there will be a label there to help me fix the demonstrative belief that a particular object is an exemplar of that vegetable. However, rather than implementing cognitive permeation, employing these beliefs *puts us in a position* to learn certain perceptual abilities, by getting us to focus on the right objects, and thus provides causal precursors to perceptual learning. Recognizing the capabilities of perceptual learning, independent of cognitive influence, shows us that we don't need anything more than these precursors to explain the role of cognition in in generating new perceptual representations.*

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The Dis-Unity of Humean Space

Ruth Weintraub

My aim in this paper is to explore some metaphysical and psychological implications of the (contentious) idealist interpretation of the belief in external objects ("bodies") Hume ascribes to us in the *Treatise*. More specifically, I will argue that the interpretation commits Hume to the claim that space is spatially fragmented, both synchronically and (even more so) diachronically, and renders Hume incapable of allowing for all the spatial thoughts we think we can have. But (perhaps surprisingly) it does not impugn Hume's view of causation.

The idealist interpretation of Hume is defended, *inter alia*, by Laird (1932, 150), Cook (1968), Bennett (1971, 321, 349), Penelhum (1975, 64), Stroud (1977, 111), Fogelin (1985, 67), Dicker (1998, chap. 5), Noonan (1999, 164), Wilson (2008) and Weintraub (2011). Here, I only explain it.

The term "idealism" is usually used to denote an *ontological* thesis, denying the existence of anything but perceptions (and minds, according to some idealists, Berkeley, for instance). The thesis I impute to Hume, by contrast, is *semantic*: it concerns the *reference* of the words "tree," "table," etc. So to avoid confusion, I will use the term "semantic idealism" to denote the view I am imputing to Hume, and whose implications I will discuss. The two theses (the ontological and the semantic) are logically independent. We might be capable of thinking about (even believing in the existence of) material bodies even if there aren't any. This is an "error theory" about body-terms.[1] Conversely, even if material objects exist, there may be no words in our language denoting them. Of course, it is awkward to state this, because if there are no relevant words, what is it we are supposing to exist? But the difficulty is only apparent. Even if there are no terms in our language denoting (putative) material objects, it is possible to say in it that not everything is a perception.

The distinction between the two versions of idealism is important in the present context. Only the semantic version can plausibly be thought to im-

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[1] I borrow the term from Mackie (1977), who claims that our moral terms ("good," "bad," etc.) purport to denote moral properties that are too "queer" to exist, although we *think* they do.

pinge on the kinds of thoughts we can have. And Hume is a semantic idealist (according to the interpretation I endorse). But he does *not* advocate the onto-logical thesis. Indeed, he enjoins us to remain agnostic about things, if any, that are not perceptions:

> It is a question of fact, whether the perceptions of the senses be produced by external objects, resembling them: How shall this question be determined? By experience surely; as all other questions of a like nature. But here experience is, and must be entirely silent. The mind has never any thing present to it but the perceptions, and cannot possibly reach any experience of their connexion with objects. The supposition of such a connexion is, therefore, without any foundation in reasoning. (E 12.12; SBN 153)[2]

Here is the second clarificatory remark. Although according to the semantic idealist interpretation, both vulgar and "philosophers" identify objects with *impressions*, we need, for the purpose of the present paper, to distinguish be-tween the beliefs Hume ascribes to them. The vulgar believe—*de dicto*—that objects *are* perceptions: an apple, for instance, is a complex impression with impressions of colour, taste and smell as (simple) constituents. Indeed, that is what they *mean* by the term "apple." The philosophers believe, at least in their reflective moments, in "a double existence internal and external, represent-ing and represented" (T 1.4.2.36; SBN 205).[3] They "distinguish [...] betwixt perceptions and objects, of which the former are suppos'd to be interrupted, and perishing, and different at every different return; the latter to be uninter-rupted, and to preserve a continu'd existence and identity" (T 1.4.2.46; SBN 211). According to the semantic idealist interpretation, the independently existing "objects" behind our impressions are also impressions (that we do not perceive).

Note, next, that the wording in the title is somewhat inauspicious, because Hume thinks we cannot talk about spatial points as distinct from bodies. "[T]he idea of extension is nothing but a copy of these colour'd points, and of the manner of their appearance" (T 1.2.3.4, SBN 34). So talk of the unity of *space*, which I will sometimes adopt for ease of exposition, needs to be understood as the claim that all the bodies (that ever exist) are *spatially related*.

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2 References to the *Enquiry* are to Hume (1999) and to Hume (1975), hereafter SBN.
3 References to the *Treatise* are to Hume (2000) and to Hume (1978), hereafter SBN.

For instance, Rome, London and Paris form a triangle, and ancient Rome is located (roughly) between nineteenth century London and modern Cairo.[4] I rely on an intuitive understanding of spatial relatedness, because the notion is clear, and I cannot think of a non-circular way of making it more precise.[5]

Here is the fourth clarificatory point. The suggestion that idealist objects are not *public*, i.e. that no object can be perceived (at any one time) by two different people, has been made in discussions of Berkeley's immaterialist philosophy. The claim in which I am interested pertains, instead, to the *unity* of space. These questions are logically independent. A space in which intersubjective ("public") objects are located may (logically) be fragmented. And, conversely, subjective "bodies," perceivable by one person only, may be located in a unified space, be spatially related to each other. What I will say about the unity of semantic idealist space will have no bearing on the more familiar question— the inter-subjectivity of the objects that occupy it.

The structure of the paper is as follows. After considering both kinds of spatial unity, synchronic (section 1) and diachronic (section 2), I will conclude that in both cases, Hume cannot allow for all the spatial thoughts we seem to have, and that semantic idealist space (itself) is fragmented, much more markedly in the diachronic case, there being no diachronic spatial relations *at all*. This, I will note (section 3), does not entail that there are no causal relations, and in particular, does not impugn Hume's causal claims, initial appearances notwithstanding.

1 Synchronic spatial unity

We all believe that there is a single space in which all extant bodies are located: the book that I am now reading, the moon (that I do not perceive now), and

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4 It might be thought that diachronic spatial relations are reducible to synchronic ones. Ancient Rome being to the north of modern Cairo concerns the two spatial *sites*, which are co-existent. But, first, talk about spatial sites goes against Hume's relationism about space, his claim that we cannot think of spatial points as distinct from bodies. And, second, the proposed reduction doesn't eliminate cross-temporal spatial relations. To say that ancient Rome was in some presently existing site, S_1, is spatially to relate two non-contemporaneous sites: that which ancient Rome occupied, S_2, and S_1.

5 The suggestion that comes to mind is that in a unified space, there is a *path* between any two points. But what is a "path"? It is not enough that it be possible to reach from any point in it to any other, because that only requires that it be possible to be at the two points at two different times. And this is not the notion I have in mind. To require that two points in a path must be spatially related is to render the definition circular.

the apple I am munching, to name but a few. In this section, I will consider the spatial relatedness of this (largely unperceived) totality of coexisting objects from a Semantically Idealist perspective (section 1.1), and the spatial thoughts Hume's semantic idealist can allow us to have (section 1.2).

1.1 *Metaphysics*

The intuitive view is that space is synchronically unified. This view can be upheld if, as Jackson (1977, 81–87) thinks, perceptions are spatially located in *physical* space, alongside physical objects. They then derive their spatial relations (and spatial unity) from it: physical objects are all spatially related to one another. So if, for instance, perceptions are located in retinas, which are physical objects, they are spatially related to one another through the spatial relations between their respective retinas.

As Anderson (1976) notes, Hume sometimes talks in this vein. In explaining how we acquire the idea of extension, he suggests that ideas are located in the brain: "the mind is endow'd with a power of exciting any idea it pleases; whenever it dispatches the spirits into that region of the brain, in which the idea is plac'd; these spirits always excite the idea, when they run precisely into the proper traces" (T 1.2.5.20; SBN 60–61).

In a similar vein, when he explains how malice and envy are aroused (T 2.2.8.3; SBN 372), Hume suggests that "the image and idea of the object are [...] equally extended in the retina, and in the brain or organ of perception."

But the intuitive view cannot be taken for granted from the semantic idealist point of view. Here, the retina isn't *physical*: it is an *impression* of a retina. And if an image of a house (say) is located in a retina-image, this simply means that an image of a house is a part of an image of a retina. And this does not give the semantic idealist a way of relating perceptions spatially, their being located in retinas notwithstanding. For *that*, we need to be told how the house-image is spatially related to a tree in a *different* perception, which the fact that they are both located in retina-images doesn't determine.

So there is a serious question pertaining to the synchronic spatial unity of semantic idealist space. In response, I argue for two claims. First, the objects any one person perceives by touch or sight at any one time are spatially related to one another. The restriction to two sense modalities is required because Hume thinks only these two kinds of perceptions are spatially located. He says "an object may exist, and yet be no where" (1.4.5.10; SBN 235). Tastes,

smells and sounds are not spatially located. Second, not all extant objects that *are* spatially located are spatially related; space is synchronically fragmented.

In arguing for the first claim, I need to rebut the suggestion, made by several commentators, that Hume is committed to thinking that *no* two coexisting bodies are spatially related. Thus, Huemer, who, like Hume's semantic idealist, rejects the suggestion that perceptions are spatially located in physical space, concludes that we must invoke material objects in order to account for the spatial nature of our experience. "In perception," he reasonably claims, "[we are] aware of things with spatial properties (things with shapes, sizes, and spatial relations to each other)" (2001, 150). And, Huemer continues, since these are not perceptions located in physical space, they must be material objects. Since the semantic idealist denies their existence, he cannot account for the spatial nature of our experience.

Huemer's reasoning is specious. The semantic idealist may invoke spatial relations *within* perceptions by way of spatially relating perceived "bodies." Indeed, this is Hume's strategy. He thinks many minimally visible points are simultaneously and adjacently coexistent in our perceptual field: "my senses convey to me [...] the impressions of colour'd points, dispos'd in a certain manner" (T 1.2.3.4; SBN 34). Again:

> The perception [of the table] consists of parts. These parts are so situated, as to afford us the notion of distance and contiguity; of length, breadth, and thickness [...] the very idea of extension is copy'd from [...] an impression, and consequently must perfectly agree to it. To say the idea of extension agrees to any thing, is to say it is extended. (T 1.4.5.15; SBN 239–240)

Costa (1998, 79) elaborates: "For an idea to represent space it must resemble space, and to do that it must itself be an instance of the spatial relation, i.e. it must consist of ideas that are spatially related. Thus, an idea of space is literally spatially extended."

The semantic idealist is not yet home and dry. The claim that there are spatial relations within Humean impressions is contested. Green (1874, 205) and Annand (1930, 589) impute to Hume the claim that *no* two impressions are co-existent. If that were so, we would never have a compound impression with simpler constituents spatially related to one another. But the ascription is based on a misinterpretation of Hume's claim that "time [...] consists of different parts [...] [which] are not co-existent" (T 1.2.3.8; SBN 35–36). The idea

of time depends on there being *some* non-simultaneous impressions: it "must be deriv'd from *a* succession of changeable objects" (T 1.2.3.8; SBN 36, italics mine). It doesn't require—and Hume nowhere suggests that it does—that *no* two impressions in the temporal order be simultaneous.

Still, even if Hume doesn't *say* that there are no spatial relations within impressions, perhaps he is *committed* to this claim. I find in Kemp Smith (1941, chap. 14) an argument in support of the claim that for Hume, spatial relations aren't part of the content of impressions. My interpretation of Kemp Smith isn't the standard one. Garrett (1997, 52–54) interprets these passages in Kemp Smith as claiming that our ideas of space and time do not have corresponding impressions. And in response, he suggests, echoing Hume himself (T 1.2.3.5; SBN 34), that the ideas of space and time are *abstract*: "although there is no *separate* impression of space, every spatially complex impression is an impression of space [...] *every* idea of space [...] is an idea that has been copied from previous impressions" (1997, 52–54, original italics).[6] But this is not to the point as *I* see it. Kemp Smith's objection, I think, is that there are no *individual* ideas which can represent the abstract idea of space (and time), since in no impression—and correlatively, in no idea—is the spatial arrangement *present*.

I think my reading better fits Kemp Smith's text. Spatial arrangements of simple perceptions "are, Hume is virtually saying, contemplated or intuited—'viewed,' 'taken notice of' are his favourite expressions—but *are not sensed*. They are non-impressional" (Kemp Smith 1941, 274, italics mine). And he asks,

> [H]ow is it that [Hume] has not taken what would seem to be for him the easier and more obvious course, at least as regards space—the course usually taken by those who hold a sensational-ist theory of knowledge—that extensity is a feature of certain of our sensations (those given through the senses of touch and of sight), and in consequence sensibly imaged? (1941, 277)

Whatever Kemp Smith's intention, the argument is one we should consider. If sound, it would show that semantic idealist "bodies" (those to which our body-terms refer) are *never* spatially related, because on the interpretation of spatiality of "bodies" we are considering, spatial relations obtain within perceptions (and not between material objects).

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6 Falkenstein (1997), too, interprets Hume in this way.

The argument I am imputing to Kemp Smith for the claim that spatial arrangements aren't given in perceptions is that Hume himself thinks the idea of space (and time) is not "given in the content of any one perception, and also does not consist in any mere summation of them. The arrangement is over and above the perceptions" (Kemp Smith 1941, 274). Now, the premise does not entail that the spatial arrangement of impressions isn't part of the content of the complex impression *in addition* to the coloured minima. To bridge the logical gap, another assumption is required, which Kemp Smith imputes to Hume: "it is in simples, to the exclusion of any supplementary factors, *relational* or other, that compounds consist" (1941, 279, italics mine). Since (uncontentiously) Hume also thinks that simple perceptions have no extension (spatial or temporal), he is committed, according to Kemp Smith (1941, 288–89), to the supposition that extension is not given in experience (and, by implication, Kemp Smith 1941, 548 thinks, must be *a priori*).

Kemp Smith adduces indirect evidence in support of the attribution to Hume of the "composition theory": it was "so little questioned in his day," so much so that Hume holds to it "tenaciously and dogmatically, without argument and in the face of contrary evidence" (1941, 279). But in fact, the imputation is implausible. First, Gibson, on whom Kemp Smith relies for the attribution, ascribes the view to "thinkers of the *seventeenth* century" (1917, 47, italics mine), and Hume is an *eighteenth* century philosopher. Second, the view Gibson ascribes to them does not, *pace* Smith, preclude relations as constituents of wholes; it only requires a whole to be the *aggregate* of its constituents: two wholes can differ only if they differ with respect to some part. So when Locke, who does belong in the relevant period, says (II.xxv.8, italics mine)[7] that relations are "not contained in the real existence of Things, but something *extraneous* and *superinduced*," he doesn't mean to suggest that a relation isn't *objective*; that it is imposed by us. He means, far less pregnantly, that a relation isn't an *intrinsic* property of the relata. And when he says (II.xii.7) that a relation "consists in the consideration and comparing one Idea with another," he doesn't mean to suggest that the comparison is of *our* making. For instance, and closer to home, he says (II.xiii.2) it "is so evident that Men perceive, by their Sight, a distance between Bodies [...] as that they see Colours themselves." This is in keeping with compositionality if, for instance, *a* being to the left of *b* is composed of *a*, *b* and "to the left of."[8]

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7 All quotations from Locke are from (1975).

8 Inukai (2010, 22) notes that compositionality doesn't allow for a distinction between asymmetric relations: $a\mathrm{R}b$ and $b\mathrm{R}a$ have the same constituents, yet one might hold without the other. But

This does not yet show that Hume's semantic idealist can allow for spatial relations within perceptions. If he subscribes to this (less radical) version of compositionality, according to which a relation *may* be part of an idea, but has to be an idea *itself*, he is still in trouble vis-à-vis spatiality. For, unlike Locke, he cannot allow the relational impression aRb ("*a* is to the left of *b*," for instance) to include R as a constituent impression, in addition to *a* and *b*. This is because Hume subscribes to the principle of separability: "Everything that is different [...] may be separated" (T 1.2.2.10; SBN 36). In fact, Hume invokes several, logically independent, versions of the separability principle (Weintraub 2007). According to the version that is relevant here, a perception can constitute the entire content of the mind (at a given time); it is a *complete* image.[9] Of course, it *may* appear as part of a more complex image, but it *needn't*.

Hume invokes this version of separability, call it SP_1, on several occasions; for instance, in the argument he adduces (T 1.2.3.9; SBN 36) to show that time is not a "distinct idea," but rather, a "manner" in which perceptions "appear to the mind." Since time cannot "be conceiv'd without our conceiving any succession of objects," Hume argues, "it can[not] alone form a distinct idea in the imagination [...]. The idea of time is not deriv'd from a particular impression mix'd up with others, and plainly distinguishable from them." Note that a weaker separability principle, call it SP_2, according to which any two perceptions are separable *from one another*, which Hume invokes elsewhere, does not suffice for Hume's purpose here. "Five notes play'd on a flute give us the impression and idea of time." But five violin notes would have done equally well. *Some* sequence of objects is required, but no particular one. So we see that Hume is here invoking the stronger separability principle.

The strong separability principle, SP_1, prevents relations, e.g. "to the left of" from being ideas, and, consequently, aRb from having three constituents, a, b, R. The weaker principle, SP_2, doesn't here pose a problem for Hume. The relation "to the left of" can be separated from any two *specific* relata (the

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this seems to me a subtle problem of which Locke was unaware. So it doesn't constitute evidence against the imputation to him of the compositional view.

9   Hume uses the term "image" broadly, as applying to anything which can be given in, or copied from, experience, not just the *visual*. The term "impression" applies to "*all* our sensations, passions and emotions, as they make their first appearance in the soul [...] [and ideas are] the faint images of these" (T 1.1.1.1; SBN 1, italics mine).

table and the chair) when it relates two *other* relata, a pen and a pencil, for instance.[10]

Locke, by contrast, rejects separability. (Indeed, he rejects both of its versions.) "Many ideas require others as necessary to their Existence or Conception, which yet are distinct Ideas. Motion can neither be, nor be conceived without Space [...] and they are very distinct ideas" (II.xiii.11). So although "to the left of" and "roundness" aren't complete images, they can count as ideas for him.[11]

So Hume is in trouble with respect to the spatial character of our experience *if* he subscribes to compositionality. But there is no clear-cut textual evidence that he does, and much that tells against its attribution to him. There are some passages that might be taken to favour the ascription, but they are compatible, I will argue, with the (weaker) ascription according to which each perception has simple perceptions as parts, which does not imply that each perception is the *aggregate* of simple perceptions. The first passage is Hume's explanation of the distinction between simple and complex perceptions, where he says that "complex perceptions [...] may be *distinguished into* parts" (T 1.1.1.2; SBN 2, italics mine). This *might* be taken to suggest that a complex perception is the *aggregate* of its simple parts. But it can be taken to mean, instead, that a complex perception, unlike a simple one, *has* parts, which does not entail that it is identical to their aggregate. The second such passage is Hume's discussion of the association of ideas (T 1.1.4.1; SBN 10), where he attempts to account for the way simple ideas are combined. The rules constrain the way simple ideas unite, so that, for instance, similar ideas tend to be associated. But the

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10 Falkenstein (2006, 68) suggests the problem engendered by the separability principle is that it is not clear how a red point can be separated from a blue point if their "manner of disposition" isn't a distinct impression. By way of a solution, he suggests that there are here three different impressions: the red point, the blue point and the (complex) impression of the red point to the left of the blue one. But the problem engendered by the separability principle pertains to the *relation* and not to the relata. The former cannot exist on its own, whereas the latter two can.

11 Inukai cites another Humean reason against the existence of an impression "to the left of" (2010, 203). She points out that Hume explicitly says that there is no additional impression to the impressions of notes from which our idea of time is derived. Rather, the idea of time "arises [...] from the manner, in which impressions appear to the mind" (T 1.2.2.10; SBN 36). There being no relevant difference between time and space, she argues, Hume would say that there isn't, in addition to "the impressions of colour'd points disposed in a certain manner" (T 1.2.2.4; SBN 34), another impression from which our idea of space is derived. Although x can be to the left of y, there is no impression "to the left of." I think this is not an additional Humean *reason* against an impression "to the left of," but rather, *textual evidence* for the imputation of the claim to Hume.

rules do not imply that the association of simple ideas is the *only* way of forming complex ideas.

The final relevant passage is more troublesome for my claim that Hume does not endorse compositionality. In his attempt to account for our possession of some seemingly problematic ideas, Hume says that "we do not annex distinct and compleat ideas to every term we make use of, and that in talking of *government*, *church*, *negotiation*, *conquest*, we seldom spread out in our minds all the simple ideas, of which these complex ones are compos'd" (T 1.1.7.14; SBN 23, original italics). But what matters for Hume here is his claim that we can bring to mind the constituent ideas of these terms should the occasion require. And this does not require compositionality.

Here, now, is evidence for the claim that Hume *denies* compositionality. As is apparent from his discussion of distinction of reason, Hume (sensibly) thinks whiteness is *somehow* part of the impression of a white globe. He thinks an impression of a white globe is different from an impression of a black one: "when a globe of white m*a*R*b*le is *presented*, we are [not] able to separate and distinguish the colour from the form" (T 1.1.7.17; SBN 25, italics mine). We have an impression of a white globe, but neither the whiteness of the globe nor its roundness is a constituent impression, because neither is capable of appearing on its own in the mind.[12] So *contra* Kemp Smith, Hume *breaks* with the compositional tradition.

The break with compositionality is also manifest in Hume's view of relational impressions: "space [...] consists of a number of co-existent parts dispos'd in a certain order, and capable of being *at once present to the sight* or feeling" (T 2.3.7.5; SBN 429, italics mine). But for any relation, R, the fact *a*R*b* includes components (*a* and *b*) without being their aggregate; if it was, there would be no difference between different relations ("to the left of," "is taller than," etc.). Neither, we have seen, does it have R as a component-perception. So *a*R*b* isn't a *combination* of perceptions. It *includes* simple components, but isn't their aggregate. It, thus, has a *non-compositional* structure.[13]

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12 The white globe is a complex, extended, image (composed of white *minima*), and can appear in the mind on its own. So can a *single* coloured perception (although it has no shape).

13 Hume's treatment of the apple example (T 1.1.1.2; SBN 2), designed to illustrate the distinction between simple and complex perceptions, is perfunctory. He says "a particular colour, taste, and smell [...] [are] all united together in this apple." But he is here contravening his own claim, made subsequently, that the colour of an object *isn't* an impression, because it cannot appear on its own without some shape. Hume is more circumspect in his attitude to the *shape* of the apple, which he *omits* from the list of its constituents.

I conclude that Hume rejects (albeit implicitly) the "composition theory,"[14] and this rebuts Kemp Smith's argument for the claim that he is not entitled to suppose that spatial relations are *given* in perceptions. But the semantic idealist needs to contend with another threat to his claim that spatial relations are given in perceptions. If "to the left of" is not a perception, how can it be *present* in a perception? The natural thought here (Garrett 1997, 70) is that it is a non-separable *aspect* of the perception. It makes a difference to the perception without itself being a perception. But Hoffman (2011, 1139) argues that Hume takes the separability principle to hold *universally*, and to apply, in particular, to *aspects*: "*Whatever* is distinct, is distinguishable; and whatever is distinguishable, is separable by the thought or imagination" (T App. 19; SBN 634, italics mine). Since aspects cannot exist on their own, Hoffman argues, they are not separable. Consequently, they are not distinguishable "by the thought," and we cannot, *pace* Garrett, think of R as a (non-separable) aspect of *a*R*b*.

I needn't here adjudicate between Garrett and Hoffman. For my purpose, it suffices to show that even on Hoffman's (ontologically more austere) interpretation, Hume countenances relational thoughts like "*a* is to the right of *b*." The first step in showing this is to note that if he cannot, no more can he countenance monadic thoughts like "*a* is brown." If R is not a (non-separable) aspect of *a*R*b*, neither is the brownness a non-separable aspect of the perception of a brown *a*.

That this is a very unreasonable view doesn't show that it is not Hume's, but it puts pressure on us to show that he is *not* committed to it (and Kemp Smith isn't vindicated after all). To this end, suppose, following Hoffman's Hume, not only that there are no colours, over and above objects, but also, that there are no "aspects" of objects. Now, consider the statement "Fido is brown." We can represent the statement with a perception of a brown Fido, and distinguish between *it* and statements ascribing other colours to Fido. Despite his (radical) nominalism, Hume (sensibly) recognises that these are all phenomenologically different perceptions. The dispute over the existence of aspects, whether separable or not, pertains to the *underlying ontology*; to

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14 My (interpretative) claim pertaining to compositionality is restricted to the *Treatise*. In the first *Enquiry*, Hume endorses compositionality. "Complex ideas may, perhaps, be well known by definition, which is nothing but an enumeration of those parts or simple ideas, that compose them" (EHU 7.4; SBN 62). But his endorsement here makes sense, since compositionality is simple and elegant, and is precluded in the *Treatise* by Hume's allegiance to separability, which he relinquishes in the *Enquiry*.

the sorts of facts in virtue of which statements have the truth-values that they do.

Similarly, there is a phenomenal difference between a's being on the left of b and a's being on the right of b, even if (as the principle of separability dictates) neither perception has a non-separable aspect, "to the left of" or "to the right of." There are here two distinct perceptions, representing in thought two different states of affairs.[15]

At the end of this (somewhat tortuous) analysis, I conclude that there *are* spatial relations within Humean perceptions, so (this is the first claim I set out to defend) the (visual and tactile) "bodies" each person perceives at any one time are spatially related to one another. This is fortunate for Hume the semantic idealist, since a commitment to the denial of this claim would constitute a strong argument against, a *reductio* of, his science of man. Evidently, our visual experience has a spatial character.

I move to argue for my second claim, that not *all* visual or tactile coexisting objects are spatially related. The supposition that they are all spatially related means, from the semantic idealist perspective, that they are all included within *one* (visual) impression. This is an adaptation of a suggestion from Berkeley's ontological idealism, at least as it is sometimes construed. According to Foster's interpretation of Berkeley,

> God has an all-embracing perception of a vast spatiotemporal arrangement of sensible qualities [...] As a result, the arrangement, though just an idea in God's mind [...] qualifies as our physical world. It is something which has, in relation to us, the publicity and externality which our concept of the physical requires. (1982, 30)

Of course, Hume will omit God and invoke only the "vast spatiotemporal arrangement of sensible qualities."

In determining whether Hume's semantic idealist can allow for this all-inclusive impression, we should consider separately the vulgar and the philo-

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15 Inukai claims that because Hume accepts the separability principle, he is committed to denying the phenomenal reality of relations. Relations only exist, she suggests on Hume's behalf, "at the level of ideas *in* the imagination" (2010, 206, original italics) . But her claim is based on the mistaken supposition that Hume accepts compositionality. If relations were a real part of our perceptions, she argues, they would be inseparable perceptions, in violation of the separability principle. But without compositionality, the inference is fallacious. Relations can be experientially real without being perceptions.

sophical beliefs. I start with the former. He clearly *cannot* if all the "bodies" that we think exist actually do. The all-encompassing impression must include *all* the details of all the extant (semantic idealist) "bodies." For instance, it must include the (visual) impressions of all extant cities. So it is too rich and detailed to be had by any human.

Perhaps we shouldn't assume that all these "bodies" exist. After all, Hume thinks that although unperceived "bodies" (that is, impressions) *may* (logically) exist, our impressions do not, as a matter of fact, continue to exist when they are not perceived: "all our perceptions are dependent on our organs, and the disposition of our nerves and animal spirits" (T 1.4.2.45; SBN 211). Hume adduces several "experiments" in support of this claim, and their cogency may be questioned (Bennett 1971; Wright 1983, 44). Fortunately, we can bypass this issue. Even if Hume is right in thinking that only perceived bodies exist, the threat to the supposition that there is no impression "housing" all extant bodies remains, and the space of extant vulgar "bodies" is not unified. The experiments at most show that every extant object is perceived by *someone*. And since there are many perceivers, there are too many perceived (visual) objects to be included in a single human impression. So although the (visual and tactile) objects *each person* perceives are spatially related, vulgar space, which includes (visual and tactile) bodies perceived by *some person or another*, is not spatially unified.

There is another reason for thinking there is no all-inclusive impression. Suppose I am sitting at my desk, seeing it and the things on it. The "bodies" I perceive are constituents of a single (complex) impression, and stand in spatial relations: the pencil is on the left of the pen, for instance. Suppose, further, someone else is perceiving the pen and a bookcase behind me, which I am not perceiving. Since the pen, the pencil and the bookcase are all perceived by someone, they all exist. So we are looking for an impression that will include them as constituents. Remember, the vulgar perceive "bodies" *directly*. "The very image, which is present to the senses, is with us the real body" (T 1.4.2.36; SBN 205). For instance, when a vulgar person has a table-impression, he is directly perceiving what the word "table" refers to when used by the vulgar.

Here, the impediment is not that the requisite impression is too detailed, but rather, that no impression in vulgar space can play the envisaged role. An impression that includes the bookcase and the pen is (intuitively speaking) an image of them as seen from a perspective different from my current one (facing one, but not the other). And looking at these two objects from somewhere

else (the door, for instance), the image of the pen will be different from the image I have of it now, and will, therefore, constitute a *different* image.

What about the philosophers' space? Hume thinks that the philosophers' belief in the existence of unperceived "bodies" is *unjustified*, rather than false:

> The only conclusion we can draw from the existence of one thing to that of another, is by means of the relation of cause and effect, which shews, that there is a connexion betwixt them, and that the existence of one is dependent on that of the other. The idea of this relation is deriv'd from past experience, by which we find, that two beings are constantly conjoin'd together, and are always present at once to the mind. But as no beings are ever present to the mind but perceptions; it follows that we may observe a conjunction or a relation of cause and effect between different perceptions, but can never observe it between perceptions and objects. 'Tis impossible, therefore, that from the existence or any of the qualities of the former, we can ever form any conclusion concerning the existence of the latter, or ever satisfy our reason in this particular. (T 1.4.2.47; SBN 212)

So we cannot tell for any body-impression had by someone whether the impression "behind" it, the "body," exists, and correlatively, whether philosophers' "bodies" can be "housed" in a single all-inclusive impression. This means that Hume is committed to the *undecidability* of the synchronic unity of philosophical space. But this is far less significant than his commitment to the spatial *disunity* of *vulgar* space. He thinks the vulgar belief is *predominant*: "almost all mankind, and even philosophers themselves, for the greatest part of their lives, take their perceptions to be their only objects, and suppose, that the very being, which is intimately present to the mind, is the real body or material existence" (T 1.4.2.38; SBN 206).

1.2 *Psychology*

I now move to discuss the psychological question, concerning our ability to *think* various spatial thoughts. I will focus on thoughts pertaining to the spatial relatedness of *particular* objects: {Paris, London}, {Rome, Moscow, Neptune}, {Jerusalem, the cup on my desk, the Eiffel tower, the moon}, etc., and will argue that Hume cannot account for those that involve too many objects.

It might be wondered why I do not focus, instead, on the *generalisation* that all contemporaneous objects are spatially related. The answer is that this seemingly more natural suggestion is problematic, because it is not clear that Hume's imagism can accommodate thoughts involving complex structures such as logical connectives and quantifiers. And the generalisation that all objects are spatially related engenders a special instance of this problem. Hume explains how error engendered by (nominalist) thinking with representatives is typically avoided. If we erroneously generalise from an equilateral triangle that all triangles are equilateral, the "other individuals of a scalenum and isoceles, which we overlooked, crowd in upon us, and make us perceive the falsehood of that proposition" (T 1.11.7.8; SBN 21). But he doesn't explain how the erroneous thought was possible to begin with, how we could *meaningfully* "assert, that the three angles of a triangle are equal to one another." The natural suggestion is that a generalisation, that all dogs are brown, for instance, is a huge conjunction of statements about individual dogs: that Fido is brown; that Spot is brown, etc. But this suggestion familiarly fails. The universal belief (about *all* dogs) cannot be reduced to (defined in terms of) them. One might believe the generalisation without thinking about any individual dog; indeed, without knowing even of a single one. I avoid the difficulty by focusing on spatial thoughts that are not vulnerable to this difficulty so as to highlight another.

I argued above that there is no (human) impression capable of "housing" *all* the bodies we think exist (at any one time). And similar reasoning will rule out impressions "housing" a sufficiently large number of them: all the world's capitals or denizens, for instance. And the same seems to hold for the corresponding *beliefs*. Like any Humean belief, they are (sufficiently lively) ideas, and they are seemingly too "crowded" to be human perceptions. So is the thought (for instance) that Rome is between Paris and Jerusalem *impossible*?

A positive reply would be worrying. Hume might not balk at the suggestion that all of our everyday spatial beliefs are *false*. It is not an adequacy condition on the science of man that it *vindicate* our beliefs; its aim is to *account* for them. Indeed, Hume might view the falsity of some of our spatial beliefs as an interesting discovery within his science, akin to his claim that our attributions of personal identity across time and our belief in the continuing existence of objects are false. Berkeley, too, attributes to us wholesale error when he suggests that our ordinary causal judgements are false, because causation requires agency, and the objects of sense are ideas, which are *inactive* (1710, sec.25). The true cause of all event, he thinks, is God. But our *having* these

spatial beliefs is a fact that Hume's science of man ought to countenance, and even explain (like our (false) belief in the continued existence of bodies).

The suggestion that naturally comes to mind by way of a Humean response is that there is a difference between semantic idealist space itself and our (ideational) thoughts about it, a difference that might make possible the seemingly problematic spatial beliefs. Consider the thought that Rome is between Paris and Jerusalem. Putting together my impressions of the three cities engenders an impression that is too rich and detailed to be had by any human. But Hume thinks an idea of an object needn't include all the details included in its impression. "I have seen Paris; but shall I affirm I can form such an idea of that city, as will perfectly represent all its streets and houses in their real and just proportion" (T 1.1.1.4; SBN 3)? By means of this rhetorical question, Hume makes the point that although he has forgotten many of the details present in his original impression of Paris, he nonetheless has an idea of it. And, similarly, we cannot have a "just" idea of a mite, because a "just" idea has to represent "every part" (T 1.2.1.5; SBN 28), which is impossible, because of their "vast number and multiplicity." Yet, we do have an idea of a mite (and those of "other objects vastly more minute"), which enables us to think about it. Similarly, the thought that Rome is between Paris and Jerusalem doesn't require an idea with all the details included in my impressions of the three cities, so there is no impediment to my having it.

But even if an idea of Paris may be quite "thin" in comparison with Paris itself (i.e. its impression), it seems that an idea cannot represent, even "thinly," *all* the (numerous) cities that I think are spatially related: it would (impossibly) have to include at least one detail for each city. So we cannot have spatial beliefs pertaining to (sufficiently) many objects.

There are two points worth noting. First, the impossibility is due to Hume's theory of ideas, not to his semantic idealism. This means that the problem will not arise for a non-imagist semantic idealist. Second, the difficulty is not restricted to spatial thoughts. Hume's semantic idealist cannot countenance *any* thought that involves a large number of objects. The spatial case is an interesting special case of the difficulty.

2 Diachronic spatial unity

2.1 *Metaphysics*

We are here concerned with spatial relations obtaining between *non-simultaneous* objects. Since we are aiming to discern the implications of Hume's semantic idealism, this means that our question pertains to cross-temporal spatial relations between *impressions*. If my impression includes the (visual) objects on my desk, they are straightforwardly spatially related to one another. But how can the lamp in my T_1-impression be spatially related to the computer in my *subsequent* T_2-impression?

Here is a suggestion for contending with the problem. Several contemporaneous perceptions may constitute a more complex *single* perception: "a particular colour, taste, and smell are qualities all united together in this apple" (T 1.1.1.2; SBN 2). So can't *successive* perceptions count as one, *temporally extended*, perception? And if they do, won't there be spatial relations between even their non-simultaneous constituents?

The answer to the first question is "Yes." Hume has a reason for thinking that some perceptions are temporally extended: "from the succession of ideas and impressions we form *the* idea of time" (T 1.2.3.7; SBN 35, italics mine). For instance, the (single) idea of a particular temporal sequence ("I ate and then I drank") is composed of two successive ideas, the first of which represents my eating; the second—my drinking. The same is true, *mutatis mutandis*, of *general* temporal ideas, "lasting 5 minutes," for instance. Each is represented by some particular idea of a succession (a song lasting 5 minutes, for instance), and associated with other (isomorphic) particular successions.[16]

The second question, as to whether there are spatial relations between non-simultaneous constituents of a temporally extended perception, is much thornier. We have granted that some perceptions are temporally extended. Let us even allow that each person's entire perceptional biography constitutes one (temporally extended) perception. Does this mean that there are spatial relations between its non-simultaneous constituents? The answer, I will now argue (at length), is "No."

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16 Like any plurality, a complex perception is taken by Hume to be ontologically inferior: it depends for its existence on that of its constituents. "But the unity, which can exist alone, and whose existence is necessary to that of all numbers, is of another kind, and must be perfectly indivisible" (T 1.2.2.3; SBN 31).

Suppose I have a perception of a ball and then of a pen. The two objects in the successive perceptions are not spatially related even if they are constituents of a single (temporally extended) perception. What *would* allow for cross-temporal spatial relations between the two perceptions is some third perception spatially related to both. If, for instance, both the ball and the pen were directly above a cup, the pen at T_2 would be where the ball was at T_1.
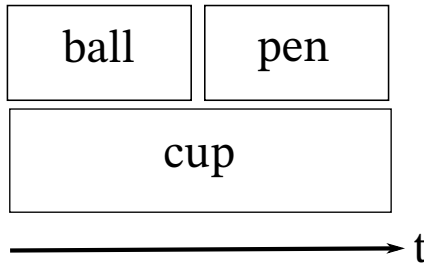
Figure 1: A

But the cup-perception coexists with a succession (the ball-perception and the pen-perception). And it is natural to suppose that it must, therefore, be a *succession* of (qualitatively identical) perceptions of equally brief durations (Price 1940, 46–47; Stroud 1977, 103; Waxman 1994, 200):
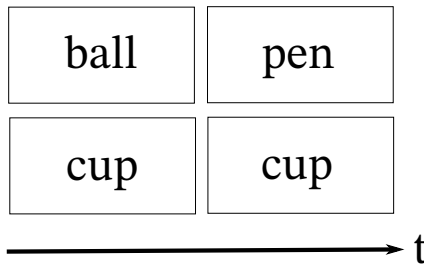
Figure 2: B

But then, it cannot make for cross-temporal spatial relations (between the ball-perception and the pen-perception). If we require some external "anchor"

so as to relate the (non-simultaneous) ball-perception and the pen-perception, the same is true of the (non-simultaneous) cup-perceptions. That they are qualitatively identical doesn't help.

The cup-perception which co-exists with the ball and the pen can make for cross-temporal spatial relations only if, as Baxter's (2007, chap. 3) Hume thinks, it has no temporal parts, doesn't itself *endure*. I am persuaded by Baxter that despite its strangeness, the view of time he imputes to Hume is consistent (Baxter provides a formal model), and doesn't contravene our concept of time. That something with no temporal parts might coexist with a temporal succession is no stranger than was the suggestion that a set can have the same size as a proper subset, which Cantor's set theory made respectable. Indeed, the (ingenious) proposal Baxter imputes to Hume does to time precisely what Cantor did to *size*. By choosing the possibility of mapping one set on to another as a criterion for sameness of size, Cantor allows size to violate the very intuitive assumption that a set is larger than any proper subset. So similarly, by choosing as a criterion for A and B coexisting neither having a temporal part that is earlier than all temporal parts of the other, Hume allows temporal coexistence to violate the intuitive assumption that coexisting objects must have the same number of temporal parts.

But the consistency of Hume's conception does not make for cross-temporal spatial relations. The question is whether the (strange) temporal structure *in fact* obtains. This is a question about the *actual* structure of time, or—in our (semantic idealist) context—impressions. *Are* there "steadfast" impressions? Suppose I see a ball and then a pen, both above a cup. There are here two possibilities as to the temporal structure of my impressions:
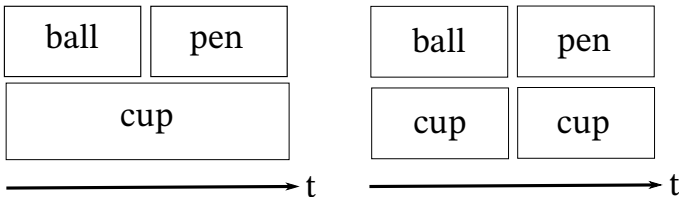

Figure 3: B (left) and A (right)

Which is the correct one? We cannot tell by introspection. True, Hume says that "all sensations are felt by the mind, such as they really are" (T 1.4.2.5;

SBN 189), and "[t]he perceptions of the mind are perfectly known" (T 2.2.6.2; SBN 366). But elsewhere, he denies that the temporal or spatial structure of perceptions is transparent. There is no perceptible dividing line between adjacent visual atoms. If there was, it would itself be a perception, contrary to the supposition that the two atoms are adjacent. We can tell where one visual atom ends and another begins only when they are differently coloured. That is why we cannot tell how many points (visual minima) there are in a line, and equality of the number of points is "useless" (T 1.2.4.19; SBN 45) as a standard for equality of length. It is "difficult for the imagination to break [a spot of ink] into its component parts, because of the uneasiness it finds in the conception of such a minute object as a single point" (T 1.2.4.2; SBN 42).

But surely, one will protest, if A and B differ *significantly* with respect to the number of their atoms, they feel different. In the spatial case, we *can* tell which of two lines is longer when one is markedly longer. "When the measure of a yard and that of a foot are presented, the mind [cannot] question, that the first is longer than the second" (T 1.2.4.22; SBN 47).

The answer is that the spatial and temporal cases differ. A temporal succession, no matter how long, is perceived to be a succession only if its members aren't qualitatively identical: "the idea of duration is always deriv'd from a succession of *changeable* objects" (T 1.2.3.11; SBN 37, italics mine).[17]

Since the question isn't decidable by introspection, I submit that we should opt for A, because it is much *simpler*. True, there are fewer entities in B. But even if (implausibly) Ockham's razor enjoins us to minimise the number of entities (rather than number of *types* of entities), it also bids us to give (methodological) weight to simplicity. And B is *structurally* much more complex. In A, (perfect) coexistence of impressions coincides with sameness of number of (non-overlapping) parts.

Not only is Ockam's razor plausible, Hume subscribes to it (without using the label). He says "we must endeavour to render all our principles as universal as possible, by tracing up our experiments to the utmost, and explaining all effects from the simplest and fewest causes" (T Introduction 8; SBN xii).

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17 It might be objected that a sufficiently long temporal succession, even of qualitatively identical perceptions, feels different from a steadfast perception. But this does not help us to decide between A and B. If the steadfast perception of the cup (in B) appeared *on its own*, it would feel different from the corresponding succession (in A). But we are trying to decide between A and B *in their entirety*. And here, because the "steadfast" part of B happens alongside a "changeable" temporal succession (the ball and the pen), the duration we experience may be entirely due to that of the variable succession.

This means that the temporal structure of my impressions isn't the one Baxter's Hume allows (perhaps even thinks) it to be. So even if Baxter's interpretation is correct, and "steadfast" impressions required for cross-temporal spatial relations *could* exist, as a matter of fact they don't. I conclude that on both interpretations of Hume's view of time, there are no spatial relations between non-simultaneous impressions. Humean space is maximally fragmented diachronically.

2.2 *Psychology*

The falsity of such diachronic spatial thoughts is no skin off Hume's nose: he doesn't aim to vindicate common sense. Indeed, it is another interesting discovery he makes, showing yet another of our beliefs to be false. But doesn't the argument I adduced on Hume's behalf to show that such diachronic thoughts are *false* also show that we cannot *have* them? This *would* be a problem for Hume, who aims to *account* for human beliefs.

I will argue that there *is* here a problem for Hume, although it is not obvious. In our context, ideas differ significantly from impressions. In the case of impressions, there is a *tie* between two ways the temporal structure of our impressions might be. And simplicity favours the standard structure: each temporal atom temporally coinciding with all others with which it overlaps. But in the case of ideas, explanatoriness tips the balance in favour of the non-standard structure, because only *it* can explain our seeming to have the diachronic thoughts.

So Hume can account for our thinking, for instance, that ancient Rome is located (roughly) between nineteenth century London and modern Cairo. But the diachronic structure of space renders this thought (and others of its ilk) *false*. This is not troubling for Hume the idealist. But he *should* be concerned by the fact that diachronic spatial thoughts involving *a large number of objects* are not possible within his semantic idealist system (because they require ideas that are too detailed).

3 Causation

The fact that there are no diachronic spatial relations seemingly poses a difficulty for Hume's invocation of causal claims. Cause and effect, he claims in his analysis of causation, must be spatially (and temporally) contiguous, or at least linked by an intermediate chain of "causes, which are contigu-

ous among themselves, and to the distant objects" (T 1.3.2.6; SBN 75). And, furthermore, causes precede their effects. This means that the requisite contiguity is between objects in *successive* perceptions. But this condition, we saw (section 2.2), is never satisfied. The only spatial relations obtain between perfectly simultaneous impressions. This is not a discovery about which Hume can be sanguine: it undermines his *own* causal claims, pertaining to the mind. For instance, "our impressions are the *causes* of our ideas" (T 1.1.1.8; SBN 5, italics mine); resemblance, contiguity in time or place, and cause and effect "*produce* an association among ideas" (T 1.1.4.2; SBN 11, italics mine); the "mind is *determin'd* by custom to pass from any cause to its effect" (T 1.3.11.11; SBN 128, italics mine). Like Hume's (interpretatively contentious) sceptical argument against induction, his semantic idealism threatens to undermine his science of man. Indeed, it seems as if the "standard" Hume cannot even *think* his causal thoughts about the human mind. Like philosophers' talk about *substance* and *occult powers*, significant parts of the *Treatise* might turn out to be *unintelligible*.

Hume wavers in his attitude to the requirement of spatial contiguity. It is one of the three conditions he discerns for causation in its analysis. "I find in the first place, that whatever objects are consider'd as causes or effects, are contiguous" (T.1.3.2.6; SBN 75). But in his discussion of psycho-physical causation, he drops the requirement of contiguity. Since being "constantly united is *all* the circumstances, that enter into the idea of cause and effect, when apply'd to the operations of *matter*, motion may be and actually is, the cause of thought and perception" (T 1.4.5.30; SBN 248, italics mine).

Hume seems to have forgotten that in his analysis of causation, he found the requirement of contiguity so essential and the idea of action at a distance so repugnant, that "when in any particular instance we cannot discover this connexion, we still presume it to exist" (T 1.3.2.6; SBN 75). But in fact, he adduces spatial contiguity as one of the requirements for causation *tentatively*. "We may therefore consider the relation of CONTIGUITY as essential to that of causation; at least may suppose it such, according to the general opinion, till we can find a more proper occasion to clear up this matter, by examining what objects are or are not susceptible of juxta-position and conjunction" (T 1.3.2.6; SBN 75).

And later, he argues that tastes, smells and sounds "exist no where" (T 1.4.5.10; SBN 235). And these, of course, are involved in causal relations. For instance, a foul smell may cause one to retch, and an unexpected loud sound—to jump with fright. So causation does *not* require spatial contiguity

(even if we think that, as a matter of fact, it often goes with it). Hume exploits this discovery to vindicate the possibility of psychophysical causation. And *we* can now conclude that semantic idealism does *not* pose a problem for Hume's causal claims.

4 Conclusion

The Humean position regarding spatiality that has emerged is the following. First, Hume's semantic idealist cannot account for some spatial thoughts we seem capable of having, both synchronic and diachronic. And this is a problem for Hume's science of man. Second, semantic idealist space (itself) is fragmented, much more markedly in the diachronic case, there being no diachronic spatial relations *at all*. Hume will view this (metaphysical) implication of semantic idealism as an interesting discovery. Finally, because Hume does not think causation requires contiguity, his semantic idealism does not imply that there are no causes and effects or that his science is replete with unintelligible or false causal claims. Neither does it engender a problem for our ability to have causal beliefs.*

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Boghossian, Bellarmine, and Galileo: Adjudication and epistemic relativism

Wim Vanrie & Maarten Van Dyck

Many prominent arguments for epistemic relativism take their departure from the observation that a certain kind of epistemic symmetry is present in particular empirical cases. In this paper, we seek to attain further clarity about the kind of symmetry at issue, and the sort of relativism to which such symmetry can reasonably be taken to give rise. The need for such an investigation is made apparent, we believe, by the fact that prominent anti-relativist arguments such as that advanced by Boghossian in his influential book *Fear of Knowledge* (2006) yield distorted pictures of the matter. Following Boghossian, we present our argument through a detailed consideration of the dispute between Bellarmine and Galileo concerning heliocentrism. Contrary to what Boghossian claims, the relevant sort of symmetry does not concern a difference in fundamental epistemic principles between Bellarmine and Galileo, but rather a much more localized difference in procedures for adjudication between shared principles in the novel epistemic circumstances generated by Galileo's telescopic observations. Bellarmine and Galileo advance fundamentally different procedures of adjudication that are nevertheless equally rational. The upshot is not so much the denial that there are absolute epistemic facts as such, as Boghossian thinks, but rather the denial that there is an absolute fact of the matter as to which was the most rational way to proceed: Bellarmine's or Galileo's. What this gives us, is the denial that there is a *certain kind* of absolute epistemic fact.

Paul Boghossian's influential book *Fear of Knowledge* (2006) has reinvigorated the philosophical debate on epistemic relativism.[1] In the introduction to his book, Boghossian characterizes relativism as the idea that "there are many radically different, yet 'equally valid' ways of knowing the world, with science being just one of them"—an idea that he claims has been adopted within

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[1] See Baghramian and Coliva (2020) and Kusch (2020) for representative overviews of recent philosophical literature on relativism.

"vast stretches of the humanities and social sciences" (2006, 2). Boghossian takes it to be the task of analytical philosophers to counteract this, by showing how a careful analysis of this thesis of Equal Validity reveals it to be mistaken or even incoherent.

There are, of course, many versions of epistemic relativism. Our interest lies in relativist positions that take their departure from the observation that a certain kind of epistemic symmetry is present in particular empirical cases, a kind of symmetry that is taken to support a relativistic conclusion. That Boghossian is concerned to address this sort of relativism is apparent both from his choice of targets,[2] and from his sustained analysis of one prominent such empirical case: the dispute between Bellarmine and Galileo about heliocentrism. Boghossian recognizes that this case has been taken to manifest a form of epistemic symmetry that supports a relativistic conclusion, and seeks to show why this is a mistake.

What is striking, however, is Boghossian's cavalier way of handling the historical details of the case, to the point of openly admitting that he merely offers "some potted astronomical history" (2006, 59). He relies on an outdated source (Santillana 1955), and feels free to disregard the historical facts even as reported there: contrary to what Boghossian (2006, 60) suggests, Bellarmine never refused to look through the telescope, but was careful enough to look for himself—as explicitly mentioned by Santillana (1955, 28)—and moreover to ask the opinion of the expert astronomers at the Jesuit Collegio Romano. It may be the case that there is an unwarranted "fear of knowledge" amongst scholars in the humanities and social sciences, but those scholars could well retort that analytic philosophers should be reminded that there is such a thing as historical knowledge as well—and that there is no need to be fearful of it either.[3]

Boghossian's sloppy treatment of the historical evidence has been pointed out before,[4] but we wish to provide a more sustained investigation of its philosophical significance than has been done so far.[5] We will argue that a careful

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2 These targets include such relativists as Shapin, Schaffer, Barnes, and Bloor, whose relativist positions cannot be disentangled from their study of empirical cases.

3 See e.g. Wootton (2007).

4 See e.g. MacFarlane (2008), Kusch (2009), Seidel (2014). Note that our focus on Boghossian's sloppiness is not meant to suggest that relativists are somehow immune to such sloppy treatment of historical, anthropological, or other evidence. Thanks to an anonymous referee for pressing us on the need to make this point explicit.

5 Kinzel and Kusch (2018) have similarly criticized epistemological debates on relativism as suffering from a lack of attention to empirical details. We take our account to be largely complementary

consideration of the Bellarmine/Galileo dispute reveals that Boghossian has misunderstood the sort of epistemic symmetry that is at issue, and thereby the relevant thesis of Equal Validity. Contrary to what Boghossian claims, the symmetry is not situated on the level of the fundamental principles of an epistemic system, but rather on the level of the procedures for adjudicating between such fundamental principles.[6] The relevant thesis of Equal Validity then becomes the thesis that—when faced with a novel epistemic situation such as the one generated by Galileo's telescopic observations—there may be available fundamentally different yet equally rational procedures for how to adjudicate between epistemic principles in this novel situation. The upshot is not so much the denial that there are absolutely correct epistemic principles as such, as Boghossian thinks, but rather the denial that there is an absolute fact of the matter about which was the most rational way to proceed: Bellarmine's or Galileo's. In terms of Boghossian's initial statement quoted above: the relevant sense in which there may be "different ways of knowing the world" that are equally valid, is quite different from what Boghossian makes it out to be.[7] As we will explain, this yields quite a different understanding of the sort of reconception of our epistemic rationality that the relativist is after.

We start by discussing Boghossian's own construal of the relativist argument. According to him, it revolves around the observation that—when we are confronted with an alternative epistemic system consisting of a different set of fundamental epistemic principles—we cannot offer a non-circular justification of the fundamental principles of our own epistemic system. From

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to theirs. Whereas we try to make our point by advancing a thoroughgoing internal critique of Boghossian's treatment of the Bellarmine/Galileo debate, they are more concerned with setting up a general theoretical framework for epistemological relativism in terms of what they call "situated judgment."

6 Boghossian chose to focus specifically on the Bellarmine/Galileo dispute because it was invoked by Richard Rorty to argue for a relativistic conclusion (Rorty 1979, 328–33). Because Rorty's use of the case is rudimentary, it is unclear at which level Rorty himself wishes to locate the epistemic symmetry.

7 It is natural to respond that, if the relativist claim as we construe it is not that there are no absolute epistemic facts as such, then it is not, in fact, a *relativist* claim. Our discussion aims to show that the better conclusion to draw is that it is the characterization of relativism as a blanket denial of the existence of absolute epistemic facts that needs to go. The core relativist commitment does not lie in such a blanket denial, but in the sort of thesis of Equal Validity that we will articulate. Note, in this regard, that MacFarlane already pointed out that Boghossian's thesis of Equal Validity need not rely on the claim that there are no absolute epistemic facts (2008, 398). This reveals that Boghossian's target is unstable: does he wish to show that there are absolute epistemic facts, or does he wish to argue against Equal Validity? These are not one and the same project. We will return to these issues in more detail below.

this, the conclusion is to follow that there are no absolutely correct epistemic principles. Bellarmine's epistemic system is meant to constitute an example of such an alternative system. Boghossian argues, however, that Bellarmine's system does not qualify because it involves principles of adjudication that are *ad hoc*.

In this way, Boghossian takes the relevant form of epistemic symmetry to be situated on the level of a confrontation between the fundamental principles of different epistemic systems. We argue that a careful analysis of the historical details of the Bellarmine/Galileo dispute reveals that this misconstrues the nature of the case: what is at issue in the debate between Bellarmine and Galileo, is not which fundamental principles to accept (or how to justify them), but rather the issue of how to *adjudicate* between those principles in the face of the novel epistemic situation generated by Galileo's telescopic observations. The epistemic symmetry lies in the fact that they develop fundamentally different procedures of adjudication that are equally rational and cannot be justified in a non-circular way.

Next, we discuss how this deepened understanding of the historical case problematizes several aspects of Boghossian's argument. Not only does it reveal that Boghossian's somewhat cavalier characterization of Bellarmine's procedures of adjudication as *ad hoc* is unfounded, it also reveals that— for Boghossian's absolutism to have any bearing on our actual epistemic practices—it must pertain not only to fundamental epistemic principles, but also to procedures of adjudication. The relativist thesis of Equal Validity— once properly understood—does not automatically issue in a blanket denial of the existence of absolute epistemic facts as such, but rather in a denial of the existence of a *certain kind* of absolute epistemic facts: facts that would objectively settle in advance, for any epistemic situation that may arise, what the uniquely correct procedures of adjudication are in that situation. It is this absolutist commitment, we claim, that is put under serious pressure by the historical evidence. Philosophical analyses that identify epistemic systems with sets of fundamental principles without taking into account the matter of adjudication, however, are blind to this issue, and thereby blind to the core relativist concern, as we understand it.

1 Boghossian on the relativistic argument

1.1 *The argument: circular justifications*

We start by giving a reconstruction of Boghossian's construal of the relativist argument. According to Boghossian, the relativist starts by noting that any argument that we could give for the superiority of our own epistemic system over alternative ones must rely on epistemic principles that we ourselves accept, and which therefore belong to the very system we are trying to justify. Thus, if we *are* confronted with an alternative system, there is a problem: "If we really do take our confrontation with an alien epistemic system to throw our system into doubt, and so to call for a genuine justification of that system, how could we possibly hope to advance that project by showing that our system is ruled correct by itself?" (Boghossian 2006, 79). Since a genuinely alternative system is one that rejects our set of epistemic principles, we are stuck in a vicious circle: we cannot justify our principles without presupposing their validity, thus begging the question against the proponents of the alternative system. In such a situation of confrontation, therefore, it is no longer possible to arrive at justified beliefs about which epistemic principles are correct. Strictly speaking, this argument does not establish epistemic relativism, since there might still be absolute epistemic facts, even if we cannot know what they are. As Boghossian acknowledges, however, there would be little interest in "an absolutism about epistemic truths which combined that absolutism with the affirmation that those truths are necessarily inaccessible to us" (2006, 76).

This argument depends on the presence of an alternative system, and it is here that Boghossian situates the relativist's invocation of historical cases such as the Galileo/Bellarmine dispute. As Boghossian sees it, Galileo uses an epistemic system that is constituted by a number of fundamental principles: (OBSERVATION), (Deduction), (Induction), and possibly (Inference to the Best Explanation). Let us give the explicit statement of (OBSERVATION) as an example:

> OBSERVATION. For any observational proposition p, if it visually seems to S that p and circumstantial conditions D obtain, then S is prima facie justified in believing p. (Boghossian 2006, 64)

That these principles make up Galileo's epistemic system means that he "relies upon them in forming beliefs, or in assessing the beliefs of others" (Boghossian 2006, 64). These principles are "implicit" in his epistemic practice, he operates "according to" them (2006, 65). That these principles are fundamental means that their "correctness cannot be derived from the correctness of other epistemic principles" (2006, 67). Using these principles, and presumably relying heavily on (Observation) to justify his use of telescopic observations, Galileo concludes that the available evidence makes it rational to believe that the Earth revolves around the Sun.

Bellarmine, on the other hand, is taken to use an epistemic system with an additional fundamental principle:

> Revelation. For certain propositions *p*, including propositions about the heavens, believing *p* is prima facie justified if *p* is the revealed word of God as claimed by the Bible. (Boghossian 2006, 69)

Using this principle, Bellarmine concludes that it is rational to believe that the Sun revolves around the Earth.

Both Galileo and Bellarmine claim that their belief is justified by the available evidence, but neither can appeal to any further fact of the matter to justify that claim in a way that is acceptable to the other. Boghossian's relativist now concludes—along the lines presented above—that there is no independent way to establish whether (Revelation) is a legitimate fundamental principle, so that we must accept that Galileo's and Bellarmine's ways of knowing the world are equally valid.

Boghossian's reply to this argument has two main components, which we take up in turn. First, he questions the cogency of the inference from the presence of fundamentally different epistemic systems to the non-existence of absolute epistemic facts (section 1.2). In a second step, he questions not this inference, but the premise that Bellarmine presents us with a fundamentally different epistemic system (section 1.3).

1.2 *Defusing the argument: blind entitlement*

To block the inference from the presence of fundamentally alternative epistemic systems to the non-existence of absolute epistemic facts, Boghossian invokes what he calls "blind entitlement," the idea that "each thinker is entitled to use the epistemic system he finds himself with, without first having to

supply an antecedent justification for the claim that it is the correct system" (2006, 99). This raises the bar for an alternative system to instill "legitimate doubt" about our own epistemic system. Such doubt is only legitimate "if we were to encounter an actual, coherent, fundamental, genuine alternative [...] whose track record was impressive enough to make us doubt the correctness of our own system" (2006, 101). In the absence of an alternative system satisfying these more demanding criteria, we need not have any scruples about a circular justification of our own epistemic system. Thus, the conclusion is no longer that we cannot know what absolute epistemic facts (if any) there are, but only that we cannot know this while under the spell of such legitimate doubt. And there is no compelling reason, Boghossian points out, to infer from this more limited obstacle to our knowing the absolute epistemic facts that there are none to begin with (2006, 103).[8]

Once this argument based on Boghossian's notion of blind entitlement is in place, it no longer matters what the precise nature of Bellarmine's epistemic system is: irrespective of whether it instills legitimate doubt or not, the relativistic conclusion that there are no absolute epistemic facts never follows. In this way, the historical details become irrelevant. The same, moreover, goes for Boghossian's original presentation of the argument: when blind entitlement is not on the radar as imposing constraints on what doubt is legitimate, *any* imagined alternative system satisfying some minimal conditions of coherence is supposed to render us powerless to justify our own. Once again, we need not bother to closely examine the historical details. Boghossian's characterization of the relativistic argument makes it proceed more or less independently from the nature of the actual cases studied by those scholars in the humanities who, according to Boghossian, accept Equal Validity. Given the emphasis that such scholars usually place on such cases and the epistemic symmetry which they take to be manifested in them, this raises the suspicion that Boghossian's way of framing the matter fails to take the full measure of their position, a suspicion that will be confirmed in our subsequent discussion.

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8 It is questionable whether the absolutist can be as happy to accept this weaker conclusion as Boghossian seems to suggest. Boghossian does not clarify if or how, once legitimate doubt about our epistemic system has arisen, we would be able to remove that doubt. If we cannot—if such legitimate doubt constitutes an epistemic black hole from which we cannot free ourselves— then the abstract possibility of our knowing what the absolute epistemic facts are antecedent to such doubt would offer little consolation. Boghossian himself admits, in a later paper, that such legitimate doubt would probably leave us in a "crippling *skepticism*" (2008b, 428). In an earlier paper he still took it to be plausible that absolute epistemic facts are *known*, not merely that they can be known (2001, 4).

We can sharpen this suspicion. Boghossian confidently states that "perhaps it is overdetermined that the relativist will agree" with the idea of blind entitlement (2006, 99). No doubt, all relativists will agree that everybody is *prima facie* entitled to use the epistemic system they find themselves with. But Boghossian requires more, as he goes on to assume that this entitlement puts one in a position to justify the *absolute* correctness of one's epistemic system, and this the relativist will deny.[9] In assuming this, Boghossian is begging the question against the relativists. As will be shown in what follows, relativists can resist this move in a principled way by appealing to the results of a more fine-grained analysis of the historical details of the Galileo/Bellarmine dispute. Rather than blind entitlement ruling out the relevance of historical evidence, it is the historical evidence that is taken to prompt a different understanding of the nature and scope of this entitlement. As we will see, the resulting understanding of our blind entitlement to our own epistemic system is not in fact incompatible with the thesis of Equal Validity that the relativist seeks to defend.

1.3 *Doubting the premise: fundamental difference?*

In the second step of his reply, Boghossian does not offer a blanket denial of the possibility of fundamentally alternative epistemic systems, but argues that Bellarmine's system does not qualify. Still, his analysis yields the general conclusion that "it is much harder than one may be inclined to assume at first blush to come up with an epistemic system that is a genuine fundamental alternative to the ordinary one" (2006, 103). The main question, as Boghossian sees it, is this: can we coherently ascribe to Bellarmine an epistemic system that has (REVELATION) as one of its fundamental principles, rather than as a merely derived principle? Boghossian argues that we cannot, so that Bellarmine is simply "someone using the very same epistemic norms we use to arrive at a surprising *theory* about the world" (2006, 104). Whatever the merits of that surprising theory, we would no longer be dealing with a fundamentally different epistemic system, so that the relativistic argument cannot get off the ground.

Boghossian's original argument for this conclusion is opaque, so we will be relying on the important clarification in his reply to John MacFarlane's objections (Boghossian 2008b; MacFarlane 2008). Suppose that (REVELATION)

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9   See already Kusch (2009).

has the status of a fundamental principle. Given that (OBSERVATION) and (REVELATION) yield conflicting verdicts in some cases, Bellarmine needs what Boghossian calls an "adjudicating principle" to decide between such conflicting verdicts, in the same way that we need principles to adjudicate, for instance, between (OBSERVATION) and (Induction) when they yield conflicting verdicts. The adjudicating principle that Boghossian ascribes to Bellarmine runs as follows:

> BELLARMINE'S ADJUDICATING PRINCIPLE. (OBSERVATION) trumps (REVELATION) for ordinary life, but [...] (REVELATION) trumps (OBSERVATION) when it comes to the make-up of the sky. (2008b, 425–26)

The problem, as Boghossian sees it, is that this principle does not sit well with what he calls the "no arbitrary distinctions principle," the relevant part of which reads as follows:

> If an epistemic system (or its user) proposes to treat two propositions *p* and *q* according to different epistemic principles, it must recognize some epistemically relevant difference between *p* and *q*. (2006, 98)

According to Boghossian, Bellarmine's adjudicating principle "would only make sense if he believed that propositions about the heavens are different in kind from propositions about earthly matters, so that vision might be thought to be an inappropriate means for fixing beliefs about them" (2006, 104). He continues: "But doesn't [Bellarmine] use his eyes to note that the sun is shining, or that the moon is half full, or that the clear night-time Roman sky is littered with stars? And doesn't he think that the heavens are in a physical space that is above us, only some distance away?" (2006, 104). Boghossian seems to see the situation as follows. Before Galileo's observations, there was no conflict between (OBSERVATION) and (REVELATION) with regards to propositions about the heavens, so that Bellarmine did not need his adjudicating principle to justify his use of the Bible to justify geocentrism. After those observations, however, there is a conflict. At this point, Bellarmine decides that the Bible trumps observation with regards to the make-up of the heavens. But this, Boghossian charges, is *ad hoc*. Bellarmine does not let (REVELATION) trump (OBSERVATION) with regards to earthly matters, so what reasons are there to suddenly do so with regards to the heavens, beyond a dogmatic adher-

ence to what he regards as an article of faith? On pains of being epistemically irrational, one cannot simply immunize the Bible *à la carte* against contradictory evidence whenever such evidence happens to arise. Thus, if Bellarmine was indeed using (Revelation) as a fundamental principle with the above adjudication principle, his epistemic system was irrational, so that it does not constitute a genuine alternative in Boghossian's more demanding sense. To save Bellarmine's position from being irrational in this way, we must take (Revelation) as a derived principle that arises from his alternative theory about the world rather than pertaining to the fundamental make-up of his epistemic system as such. This, however, means that Bellarmine does not have a different epistemic system at all, leaving the relativist without a case upon which to base their argument.

As will become apparent, there are several aspects of Boghossian's argument that are problematic. For now, however, let us simply notice that the relativist may resist the argument by showing that Bellarmine had more principled grounds for his adjudication principle than Boghossian allows. Here, Boghossian's cavalier treatment of the historical evidence becomes all the more striking, since the claim that Bellarmine's principle is *ad hoc* must be backed by a historical investigation of the considerations that he himself advanced in favor of it, an investigation which Boghossian does not provide. Such an investigation, it will now be shown, not only reveals Boghossian's claim to be unfounded, it also reveals that Boghossian's whole characterization of the relativist argument misses both the nature and importance of the issue of adjudication as such, misconstruing the way in which this issue figures in the historical case and thereby also misconstruing the thesis of Equal Validity which the relativist argument is meant to establish.

2 The historical evidence

2.1 *Some plain facts*

Let us first rehearse some plain facts. In March 1616, the Congregation of the Index suspended Copernicus' *On the Revolutions of Spheres* "until corrected" (Finocchiaro 1989, 148–49). A week before, the Roman Inquisition had concluded that the statement that "the Sun is the center of the world and completely devoid of local motion" was "foolish and absurd in philosophy, and formally heretical" (1989, 146). These decisions were the outcome of a prolonged and often public debate between Galileo and some of his opponents

that started soon after the publication of the former's *Siderius Nuncius* in 1610, announcing his first telescopic discoveries.

While this historical episode involves many relevant actors, most analyses have focused on the opposition between Galileo and cardinal Bellarmine. The latter was not only the most influential cardinal within the Congregation of the Index and the Roman Inquisition, but also the most important theologian in counter-reformation Rome, canonized and named "Doctor of the Church" in the early twentieth century. The central textual documents are two letters that Galileo wrote to defend the compatibility of Copernicanism with the Bible, the "Letter to Castelli" (1613) and the "Letter to the Grand Duchess Christina" (1615), and one letter from Bellarmine from 1615, reacting in part to Galileo's Copernican campaign, and which needs to be read against the background of his earlier theological writings.[10]

2.2 *The epistemic status of astronomy*

To correctly gauge what was at stake in the debate we first need to understand the epistemic status of mathematical astronomy in the period ranging from Copernicus' publication of his treatise in 1543 to its suspension in 1616.[11] Astronomers and philosophers had been debating the possibility of attaining knowledge of the true structure of the cosmos by astronomical means since Antiquity, as it was well known that incompatible mathematical models could account for the same observations. As a consequence, a majority of sixteenth century astronomers took a sceptical position, which has been characterized as one of "perpetually frustrated realists" (Barker and Goldstein 1998, 253). They saw their discipline as aiming for the knowledge of true causes, but they also believed that due to their limited earthly perspective they necessarily lacked the information that would allow them to pick out the true model. This scepticism was frequently coupled with an insistence on the fact that absent

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10 Finocchiaro's "documentary history" (1989) presents English translations of the most important documents. Fantoli (2003) provides a rich and up-to-date interpretation of the unfolding of the historical case. Blackwell (1991) gives much background on Bellarmine and offers translations of further relevant documents.

11 Historiographical views on this topic have a long history themselves, going back at least to the seminal work of Pierre Duhem. We will base our summary on Jardine (1984, chap. 7) and Barker and Goldstein (1998), which provide necessary corrections to many simplistic presentations.

any human means to directly observe the structure of the heavens, only God could provide the missing information.[12]

Copernicus and his (relatively few) followers stood out against this background for their insistence that they could demonstrate the truth of their preferred model. This confidence initially rested solely on the surplus mathematical virtues that they claimed for the heliocentric model, since there was no observational evidence available to break the tie between the Copernican and a Ptolemaic model.[13] This seemed to change with Galileo's telescopic observations. Most importantly, it became clear in 1610 that Venus showed a full cycle of phases, as our Moon does, indicating a path around the Sun for that planet.[14]

This did not settle matters, though. In the meantime, the model proposed by Tycho Brahe in the late sixteenth century was gaining many followers. In this model the Sun and Moon circle the Earth, whereas all planets revolve around the Sun (see Lattis 1994, chaps. 2, 205–211). It incorporated the surplus mathematical virtues of the Copernican model, predicted the Galilean observations of Venus, and retained a stationary Earth, as demanded by Aristotelian physics.

In sum, astronomers and philosophers were well aware of the underdetermination of astronomical theories by observational evidence, an underdetermination which remained after Galileo's telescopic observations. It was clear to everybody involved that additional, non-observational, grounds were needed if one wanted to establish the superiority of one model over its rivals.

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12 The underdetermination problem sketched in this paragraph was not the only factor in determining attitudes towards the epistemic status of astronomy. Related worries arose because all successful mathematical models seemed to violate at least some aspects of Aristotelian physics, and had to deal with some recalcitrant observations. These two latter factors were often invoked in justifying a sceptical attitude towards mathematical astronomy. As it is the underdetermination problem that figures most prominently in the debate between Galileo and Bellarmine, we will not treat these other factors explicitly in our text. Note, to be clear, that we will not infer relativism *from* underdetermination—a procedure that has been criticized extensively in the literature, e.g. Boghossian (2006, chap. 8), Seidel (2014, chap. 2). Rather, underdetermination figures in the debate *between* Bellarmine and Galileo as one of the relevant considerations for determining the epistemic status of Galileo's telescopic observations. Thanks to an anonymous referee for pressing us on the need for this clarification.

13 See Evans (1998, 410–13) for a clear and concise discussion of these mathematical virtues.

14 As usefully pointed out in Ariew (1987), this does not rule out all possible Ptolemaic models, as one can construct models in which the parameters are such that Venus, while moving on a sphere that revolves around the Earth, as a matter of fact also cycles around the Sun. This option does not appear to have been taken seriously by any astronomer at the time.

Such superiority could be motivated by general physical theories (such as the claim from Aristotelian physics that the Earth was necessarily stationary at the centre of the cosmos), by invoking theoretical virtues (as the Copernicans did), or on theological grounds.[15] As we will see, Galileo developed a fourth option: extrapolating from the early successes afforded by his telescope, he was confident that his novel astronomical techniques would generate further evidence that would allow astronomers to overcome the remaining underdetermination, thus implicitly introducing empirical progress as a criterion for something like truth-approximation.

2.3 *The theological worry, and two strategies to deal with it*

Even before the formal publication of Copernicus' theory, worries had already been raised about its compatibility with biblical passages that speak about the motion of the Sun.[16] As a result, Copernicus' published treatise was prefaced with an anonymous letter which urged the reader not to interpret the proposed model as a realist description of the cosmos. Following the sceptical tradition outlined above, it was claimed that mathematical astronomy was not in the position to offer such descriptions, and that the treatise should be seen as providing nothing but a new method for calculating planetary positions. In this way, the seeming contradiction between Copernicanism and the Bible was neutralized. This letter was added without Copernicus' knowledge, and clearly goes against the spirit of the treatise itself, but likely played a large part in delaying the vigorous public debate that was to arise more than half a century later as a result of Galileo's campaign.

Since Galileo claimed that the Copernican system provides a true model of the cosmos, he had to find a different way to deal with the relevant Bible passages. He did so by appealing to an already established aspect of the Christian theological tradition, namely the acknowledgment that the Bible requires interpretation. It was universally agreed that not all biblical passages should be read literally. The real problem, then, was how to determine which pas-

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15 Strictly speaking, there was a fourth source of information that was frequently appealed to: everyday observation. This was often intimately tied to the first (Aristotelian physics), as Aristotelian epistemology gives a privileged place to this kind of observation in grounding a physical theory. For that reason, we will not treat it separately. Some astronomers appealed to a combination of these different sources, sometimes including all three mentioned, as was the case with Tycho Brahe (cf. e.g. Howell 2002, chap. 3).

16 See Lerner (2005) for some early reactions to Copernicus' ideas.

sages should be given a literal reading, and which ones ought to be interpreted non-literally. If it could be argued that the passages on the motion of the Sun possibly belonged in the latter category, Copernicans would be free to uphold their theory without contradicting biblical evidence.

Broadly speaking, then, two strategies were available for anyone worrying about the tension between Copernicanism and the Bible. Either one could embrace a realist interpretation of the astronomical theory, and accordingly argue for a non-literal interpretation of the relevant passages in the Bible. Or if one had a reason to prefer the literal reading of these passages, one could appeal to the sceptical tradition and treat the Copernican model as nothing more than a convenient instrument for calculation. These are the two roads chosen by Galileo and Bellarmine respectively. In this way, both the astronomer and the theologian tried to exploit some established aspects of each other's disciplines (respectively the possibility of non-literal readings and of non-realist interpretations) to justify their position.

2.4 *Galileo and the principle of prudence*

Let us examine Galileo's strategy in more detail. We focus on Galileo's "Letter to Christina," which contains his most considered arguments on the matter.[17] Galileo shares two premises with his opponents: that the Bible contains the revealed word of God, and as such is a legitimate source of evidence; and that the truths revealed in the Bible cannot be inconsistent with the truths uncovered through human experience and reason. This implies that in case of an apparent inconsistency between the Bible and natural philosophy, it has to be decided whether the relevant passages have been misinterpreted, or whether the philosophical claim has not been properly demonstrated.

In the letter, Galileo (correctly) does not presume that he has a proper demonstration for the truth of heliocentrism. But he firmly believed that such a demonstration was possible, so that anyone advocating a literal reading of the relevant Bible passages would be acting prematurely. He does not argue that the passages have been misinterpreted, but rather that he and his contemporaries were not yet in a position to know the proper interpretation. To this end he introduces what has been called a "principle of prudence" (McMullin 1998, 292), which states that in case of statements the truth of

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17 To a certain extent, interpretations of this complex letter will always be controversial. Compare e.g. McMullin (1998) with Finocchiaro (1986) and Fantoli (2003, 146–68). Our reading is similar to the one defended by Finocchiaro and Fantoli.

which could possibly be demonstrated by appeal to experience and reason, we should not yet decide on Bible interpretations that possibly contravene that truth.[18]

   This leaves open two important questions. What are the statements that could possibly be so demonstrated? And what are we to do with statements that do not fall in this category? On the second question, Galileo was clear enough. If the Bible contains relevant information, we should adhere to the theologically established reading. This was evidently the case for all matters of faith and morals, but also for some natural phenomena. His example was "whether the stars are animate" (Finocchiaro 1989, 104). As God has not given us the resources to decide on the truth of this statement without further assistance, we should defer to the double gift of the Holy Writ and the inspired tradition of its interpretation. The appeal to this divine gift was crucial for Galileo, because it allowed him to stress that since God has also given us the capacities of observation and reason, we should use and trust them equally wherever they apply. This also implies that if the truth of some claim can possibly be decided using these faculties, this should guide us in interpreting the Bible.[19] In this way, Galileo could appeal to the traditional metaphor of God's two books. God has not only given us two books, but also the appropriate faculties to read these books (respectively inspiration, and reason and observation). He has moreover guaranteed harmony between both—provided we correctly adjudicate between them.[20]

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18 "I should think it would be very prudent not to allow anyone to commit and in a way oblige Scriptural passages to have to maintain the truth of any physical conclusions whose contrary could ever be proved to us by the senses and demonstrative and necessary reasons" (Finocchiaro 1989, 96)

19 Finocchiaro (1989, 105):

> [...] in questions about natural phenomena which do not involve articles of faith one must first consider whether they are demonstrated with certainty or known by sensory experience, or whether it is possible to have such knowledge and demonstration. When one is in possession of this, since it too is a gift from God, one must apply it to the investigation of the Holy Writ at those places which seem to read differently.

The limitation to "natural phenomena which do not involve articles of faith" was meant to exclude miracles, i.e. cases where the inspired interpretation of the Bible should be given evidential precedence.

20 Some interpreters have taken Galileo's inclusion of statements about natural phenomena in the category of statements about which Bible interpretation should be given evidential privilege to constitute an inconsistency on his part, as he seemed to deny all epistemic relevance of the Bible

This appeal to God's gifts also brings us as close to an answer to the first question as we can get. We are told that we "may firmly believe" that the truth (or falsity) of helio-centrism can be demonstrated by observation and reason (Finocchiaro 1989, 104). Galileo asserts that his observations "can never be reconciled with the Ptolemaic system in any way, but are very strong arguments for the Copernican" (1989, 103). He declines, however, to address the remaining underdetermination due to the Tychonic alternative, merely stating that "because of many new observations [...] one is discovering daily that Copernicus's position is truer and truer" (1989, 103). In sum, the reader is simply asked to trust that the process of discovery will go on until a unique astronomical hypothesis is definitively established. Independent evidence that astronomical methods will allow us to reach such final demonstrations is not on offer. The biblical and patristic passages that Galileo used to support the idea that God wants us to use our ingenuity to discover new things about the natural world are not specific enough. Appealing to the power of astronomical methods themselves, on the other hand, would be obviously circular—since it was exactly the reach of these methods that was in question. Galileo was, in fact, implicitly introducing a novel notion of demonstration, by treating progress in a research program as evidence for something like truth-approximation, an idea that went far beyond what it meant to demonstrate according to "observation and reason" as this was traditionally understood at the time.

2.5 *Bellarmine and the principle of consensus*

Bellarmine's reply was short but to the point. He immediately warned Galileo and his defenders that treating heliocentrism as a possibly true description of the cosmos was "damaging to the Holy Faith by making the Holy Scripture false" (Blackwell 1991, 265). In his view, it was already clear that the literal

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for statements about natural phenomena at other places (see e.g. McMullin 1998, 314–19). We believe that there are good reasons internal to Galileo's text to see these apparently conflicting statements as imperfect expressions of the underlying, more fundamental principle about God's two distinct gifts to mankind. The latter translates into a distinction that is not completely co-extensive with the one between matters of faith and morals on the one hand, and matters of nature on the other hand (see also the exclusion of miracles in footnote 19). The nature of the gifts implies that within matters of nature a further distinction has to be made between those about which we were given the means to find out the truth on our own, and the ones about which we lack such means—and where we are invited to lean on the Bible if it provides relevant information. (See Fantoli 2003 for more detail on this line of argument.)

reading of the passages in question should be preferred, on the grounds of what can be called a "principle of consensus," which had been explicitly codified at the Council of Trent (held between 1545 and 1563). According to this principle the preferred interpretation of the Holy Fathers should always be followed if there was consensus amongst them, as they spoke under holy inspiration. As Galileo was well aware, the wording of the relevant decree had seemingly limited the scope of the principle to "matters of faith," but according to Bellarmine this implied no real limitation: anything that is said in the Bible should be considered a matter of faith "ex parte dicentis" (because of the speaker). If something was the word of the Holy Spirit as spoken "through the mouths of the Prophets and the Apostles" it automatically became a matter of faith: there was no way in which we could question their authority (Blackwell 1991, 266).

Having thus addressed the main issue, Bellarmine conceded that something like the principle of prudence was a valid principle. Occasions can arise in which we have to adapt our reading of Scripture to observational evidence. But he also made clear that there was no reason to assume it was applicable in the debate at hand. The scope of observation is limited to things that can be directly experienced (among which, to be clear, Bellarmine included the motion of the stars and Sun), whereas the Copernican hypothesis could never be directly observed, due to underdetermination. Galileo's telescopic observations were perfectly legitimate astronomical data as far as they go, but could not be used to put the inspired consensus about geocentrism in doubt. It was rather the other way around: the inspired reading of the Bible taught that heliocentrism was false, thus confirming the impossibility of using Galileo's implicit notion of progress as a criterion of truth.

2.6 *Daring extrapolations and innovations*

After having seen Bellarmine's letter (which had not been explicitly addressed to Galileo, but was clearly intended for his eyes), Galileo wrote down some further notes on the matter. In one of these he accuses his opponents of committing "the error called 'begging the question' " (Blackwell 1991, 274). As he saw the situation, Bellarmine cannot use biblical passages to call into doubt the possibility of astronomical demonstrations, when the "true sense of the Scripture will already have been put in doubt by the force of the [astronomical] argument" (1991, 274). It is easy to see, however, that Bellarmine could have leveled exactly the same accusation at Galileo: he was begging the question if

he wanted to argue that these astronomical arguments could put in doubt the "true sense of the Scripture," when their purported conclusions had already been put in doubt by the force of theological argument concerning the true sense of Scripture.

Both Galileo and Bellarmine accepted that the Bible and observation are equally bona fide sources of evidence. Both agreed that further guidelines were needed to decide what to believe on their basis. Neither the Bible nor the Book of Nature can be read without proper assistance—assistance which should also provide the means to adjudicate in cases where the readings seem to lead to contradictory conclusions. Galileo and Bellarmine also shared a tradition that provided a number of ways to deal with such cases. Crucially, however, this tradition provided no clear-cut treatment of the fundamentally new epistemic situation created by Galileo's telescopic discoveries. Both Galileo and Bellarmine were extrapolating from past epistemic decisions to come up with their respective answers about how to proceed in this new situation. And they did so by claiming that their approach formed a natural continuation of what everybody had been doing (or at least should have been doing) all along: Galileo explicitly appealed to the authority of Augustine, one of the undisputed fathers of the Church, to justify his use of the principle of prudence, whereas Bellarmine drew on the instrumentalist tradition in astronomy. In other words, it was only by offering an interpretation of their shared tradition that the right "adjudicating principles" could be established and that the tradition could be continued in a coherent way, given the epistemic situation at hand.

It is important to stress that the diverging extrapolations by Galileo and Bellarmine were equally daring but that neither was unreasonable. Galileo's claim that his research program of making further mathematically analyzable discoveries with his new instrument would progress until one could identify the one true hypothesis was exhilarating but totally unprecedented. Still, this claim could be partly backed up by Galileo's observations of Venus' phases; and Bellarmine, who reasonably deferred judgement on this matter to the expert astronomers of the Collegio Romano, in no way disputed the observations themselves or their direct interpretation (i.e. that they were due to patterns of partial illumination, and that this ruled out some mathematical models). On the other hand, Bellarmine's extension of the principle of consensus to everything that was stated in the Bible was in line with important tendencies within the church, but surely not explicitly codified as such in the Council of Trent. Still, this extension was less of a stretch than might appear, given that

the relevant decrees of the council of Trent did not specify any criterion by which to determine what counts as "matters of faith and morals." Since Galileo never doubted the divine authorship of the Bible, he would have to show how to distinguish matters of faith from statements not having that status within the Bible without claiming any direct insights in God's intentions, and it is hard to see how he could have done so without simply begging the question in favor of his realist interpretation of the Copernican model.

There is a deep symmetry here: to Galileo, Bellarmine seems to select *ad hoc* principles with which to safeguard his theological convictions against astronomical evidence.[21] To Bellarmine, however, Galileo appears to select *ad hoc* principles with which to safeguard his astronomical convictions against theological evidence. What can make it hard for us (or, at least, many of us) to appreciate this symmetry, is that we are predisposed to disregard the very idea of there being such a thing as theological evidence against astronomical claims, precisely because we reject the Bible as a source of evidence, especially with regards to such empirical matters. What we have aimed to show, is that Bellarmine presents us with a principled epistemic stance—foreign as it may be to us—which incorporates Galileo's observations in such a way as to leave intact the justification of geocentrism on the basis of Biblical evidence.

3 Reconstructing the relativist argument

3.1 *The central role of adjudicating principles*

The threat of circularity is evident in the stand-off between Galileo and Bellarmine. But it is important to notice the precise point at which it arises.

To start, Boghossian's formulation of (REVELATION) must be corrected. Recall the formulation:

> REVELATION. For certain propositions *p* [...], believing *p* is prima facie justified if *p* is the revealed word of God as claimed by the Bible. (2006, 69)

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21 This is also how Bellarmine appears to Boghossian, as we have seen. Boghossian's failure to genuinely engage with the historical evidence renders him incapable of seeing that this characterization of Bellarmine's epistemic procedure as *ad hoc* relies on background premises regarding the relevant issues of adjudication that will appear equally *ad hoc* to Bellarmine, and which cannot be provided with a non-circular justification.

This neglects the special status of the Bible: If *p* is indeed the revealed word of God as claimed by the Bible, then *p* is true and must be believed, full stop. Both Galileo and Bellarmine accept that whatever is stated in the Bible is true. Instead, the interesting epistemic question is: what *does* the Bible say? And it is here that an epistemic principle comes in, which we could call (Inspiration):

> Inspiration. For any proposition *p*, if *p* is entailed by an inspired reading of the Bible, then believing *p* is prima facie justified.

What is fallible, is not the Bible, but our interpretation of it. The importance of this point can be illustrated by noticing how Boghossian's formulation invites analyses such as the one given by Markus Seidel, who argues that we can understand Bellarmine's reliance on Biblical evidence as an application of a more general principle about the testimonial reliability of books (2014, 177). In this way, Seidel compares Bellarmine's reliance on the Bible to our reliance on physics books. As long as (Revelation) is taken to be the operative principle, this does seem a natural interpretation of what Bellarmine is doing, and it straightforwardly renders his adherence to the Bible irrationally dogmatic. But this misconstrues Bellarmine's position. While physics books can make false statements, the Bible cannot. If it seems as if the Bible says something false, this must be because we have misunderstood it. The fault lies in us, not in the book. On this, both Bellarmine and Galileo agree. The proper analogy, then, is not between the Bible and physics books, but between the Bible and the Book of Nature: what is written in the Book of Nature, is *ipso facto* true. As with the Bible, the question becomes how to read the Book of Nature. Just as (Inspiration) is an epistemic principle on how to draw on the Bible as a source of truth, so (Observation) is a principle for how to draw on Nature as a source of truth. (Inspiration) is not a testimonial principle, but more like what Boghossian calls a "generation principle" (2006, 65)—a principle that generates justification for beliefs from something that is not itself a belief, in this case not a perceptual state as with (Observation) but a state of inspiration.

With this correction in place, we can see that Galileo and Bellarmine agree on fundamental epistemic principles like (Observation) and (Inspiration), but that they disagree about the proper way of adjudicating between them. It is not the validity of the epistemic principles themselves that is at issue, but the question how to *apply* them in the fundamentally new circumstances created by Galileo's telescopic observations in the aftermath of the Council of

Trent. This question is answered by appealing to the following adjudication principles:

> PRUDENCE. With regards to matters of possible demonstration, (OBSERVATION) combined with (Deduction) and (Induction) should take precedence over (INSPIRATION).

> CONSENSUS. With regards to matters of faith, (INSPIRATION) should take precedence over (OBSERVATION) combined with (Deduction) and (Induction).

We can even say that Galileo and Bellarmine agree to a large extent on the validity of both adjudication principles, when taken abstractly. Their disagreement arises once the question is raised which of the two is applicable in the case of the debate on heliocentrism: are we concerned with a "matter of faith," so that the principle of consensus applies, or with a matter for "possible astronomical demonstration," so that the principle of prudence must be followed? The principles themselves do not give the answer: this can only be found in a contestable judgment with respect to what can be "possibly demonstrated," or what is a "matter of faith." It is this judgment that determines the relevant procedures of adjudication, and that cannot be further defended in a non-circular way.

Once this crucial role played by matters of adjudication is highlighted—as Boghossian (2008b) admits it must be if we are to attain an adequate account of our epistemic practice—this puts considerable pressure on Boghossian's absolutism, according to which we can know the absolutely correct epistemic system that fixes which items of information justify which propositions. Boghossian seems to be caught in a dilemma. Either he accepts that his absolutism is limited to fundamental principles, excluding matters of adjudication, but then it becomes completely impotent with regards to our actual epistemic practices, wherein procedures of adjudication play an ineliminable role. Or he claims that there are absolute facts about the correct procedures of adjudication as well, so that it is objectively settled how to adjudicate between our fundamental principles in *any* epistemic situation. It is precisely this second claim that the Galileo/Bellarmine case shows to be problematic: it shows how situations can always arise in which we have to decide on new ways to adjudicate between our fundamental epistemic principles, and which are such that there are available different procedures of adjudication none

of which can be justified in a non-circular way. The relativistic conclusion to draw is that we have here a genuine case of Equal Validity, in the form of two equally valid procedures of adjudication which give rise to fundamentally different epistemic systems and which cannot be justified in a non-circular way.

Boghossian, if he wishes to hold on to his absolutism, would have to maintain that it is somehow always objectively settled which procedure of adjudication is the correct one and that we are always—at least in principle—in a position to know what this correct procedure is.[22] In this vein, while admitting that matters of adjudication are "complex and variegated" (2008b, 421), Boghossian stresses that they must nevertheless be decidable *a priori*, presenting the following argument: "If we can only think of ourselves as having epistemic principles that deliver determinate verdicts if they are a posteriori, then it is hard to see how we could *ever* figure out what the correct adjudicating principles are. To figure them out from the evidence, it would seem you would antecedently have to know what they are" (2008b, 419). Read as an argument against the relativist claim that the correct adjudication principles cannot be determined *a priori*, this seems to beg the question. After all, the relativist means to deny that we can figure out what the correct adjudicating principles are at all, if "correct" is read as "absolutely correct," since according to them there are no absolutely correct adjudicating principles.[23] Similarly, if Boghossian is claiming that any *a posteriori* grounds for a procedure of adjudication will be circular because they invoke that very procedure, this can be seen as a version of exactly the point the relativist wishes to make: both Galileo and Bellarmine can indeed only justify their procedures of adjudication in circular ways. At the same time, it must be emphasized that the relevant relativistic picture is not that of someone pulling up a whole epistemic system by their bootstraps, adjudication and all. What is crucial in historical cases such as the Galileo/Bellarmine dispute is that an existing

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22 Recall that Boghossian is—rightly—not interested in an absolutism according to which we cannot know what the correct epistemic principles are.

23 To be more precise: there are no *uniquely* absolutely correct adjudicating principles. Below, we will suggest that the relativist may adopt the view that it is absolutely settled—in each epistemic situation—which of the available procedures of adjudication qualify as epistemically rational, as long as it is maintained that there need not be a unique such procedure. As we construe the relativist position, its core commitment lies in the presence of a fundamental form of epistemic symmetry with regards to adjudication in cases such as the Bellarmine/Galileo debate, where the question whether this symmetry is itself "absolute" or "relative" in character is of lesser importance.

epistemic system is confronted with a fundamentally new situation. Galileo and Bellarmine, as we have emphasized, already have an epistemic system, including adjudicating principles, on which they more or less agreed before the advent of Galileo's observations. What needs to be settled, is not how to adjudicate between (OBSERVATION) (in combination with principles of reasoning) and (INSPIRATION) in general, but how to adjudicate between them specifically in the face of Galileo's new kind of empirical observations. As we have seen, Galileo and Bellarmine can rely on shared reasons—including the underdetermination problems in astronomy and disputes about the domain of matters of faith in theology—in order to articulate their respective answers to that question. What impresses the relativist in a careful study of cases like these, is a combination of the fact that this new kind of epistemic situation could not have been foreseen, and the fact that the existing epistemic system at the time yields no unequivocal answer on how to proceed. As we have tried to show, both Galileo and Bellarmine presented coherent ways to employ their epistemic system in the situation at hand, with incompatible results. From this, the relativist concludes that it makes no sense to conceive of such matters as objectively settled in advance. If we believe it to be obvious that, yes indeed, the make-up of the heavens is a matter for possible demonstration and not a matter of faith, we are simply projecting back into what is an inherently indeterminate epistemic situation the centuries of further development since Galileo's views came to be accepted.[24] Such development does not show that Galileo's answer was objectively correct and Bellarmine's objectively incorrect; it only shows that we have succeeded in fruitfully building upon the epistemic basis that Galileo laid out for us.[25]

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24 Note that this amounts to precisely the sort of *a posteriori* consideration that Boghossian needs to be irrelevant. Boghossian is committed to the claim that Bellarmine himself—given all the information he had—was in a position to rationally decide on the correct adjudication principles through suitable *a priori* reflection. What is *a posteriori*, is whether those correct adjudication principles render either geocentrism or heliocentrism the correct position to adopt, since this requires empirical evidence. If one admits, however, that reflection about the correct adjudication principles must *itself* rely on the further astronomical evidence that was gathered post-Galileo, one is thereby admitting that adjudication is not an *a priori* matter. Moreover, and most importantly, the invocation of such further evidence remains circular from Bellarmine's point of view, since it relies on Galileo's procedure of adjudication. Alternatively, if Bellarmine's procedure had been adopted, it is possible that further theological evidence against heliocentrism would have been gathered, the invocation of which would remain question-begging from Galileo's point of view.

25 In this regard, it should be noted—contrary to what Boghossian suggests—that Galileo's way of supporting heliocentrism with observational evidence is far from straightforward, and itself requires substantial theoretical work. It is all too easy to forget that Copernicanism itself flies

3.2 *Fundamental difference*

We claim that Bellarmine and Galileo should be seen as proposing fundamentally different epistemic systems, thus effectively countering Boghossian's argument (see section 1.3). There are two main reasons why one could doubt this. The first arises from the question whether a mere difference in adjudication can lead to fundamentally different systems. The second consists in questioning once again the status of (Inspiration) as a purportedly fundamental epistemic principle.

The first reason, we think, issues from an underestimation of what may be described as the epistemic depth of issues of adjudication. According to Boghossian, adjudicating principles "tell us when a piece of evidence for *p* is stronger than another piece of evidence that we might have for rejecting *p*" (2008b, 419). This leads to a picture of Bellarmine claiming that the Biblical evidence for geocentrism trumps the astronomical evidence against it.[26] On such a picture, it can only be a matter of time before the mounting astronomical evidence will tip the balance in favor of Galileo, even if Bellarmine was perhaps still rational to hold on to geocentrism. There is, on this conception, no fundamental difference between their epistemic stances, and thus no good reason to deny the existence of absolute epistemic facts. In response to our historical analysis, it will perhaps be admitted that Bellarmine was more rational than he had initially been made out to be, and that Galileo and Bellarmine were perhaps not yet in a position to decide on heliocentrism. But, crucially, this symmetry will now be interpreted in terms of a lack of sufficient evidence: there was not yet enough astronomical evidence to tip the balances in Galileo's

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in the face of much observational data. Does not Galileo, as Bellarmine made sure to point out (Blackwell 1991, 266), use his eyes to see that the Sun is moving? Does he then believe that propositions about the movement of the Sun are different in kind than those about the movement of earthly objects? Is this not an arbitrary distinction? And so on. Of course, it is to address such worries that Galileo developed his innovative analyses of the application of the concept of motion to observational deliverances in the *Dialogue concerning the two chief world systems* in 1632. Finding out new fruitful ways to adjudicate is indeed at the core of much scientific work. Boghossian, on the other hand, states that "the way of fixing beliefs that we call 'science' is in large part a rigorous application of these ordinary, familiar principles," referring to the principles of (Observation), (Deduction) and (Induction) (2006, 67). This completely ignores the question of how to adjudicate between those principles, as if it is always a straightforward matter *how* to apply them "rigorously."

26 See e.g. Baghramian and Coliva (2020, 183), who use this to argue that the difference between Galileo and Bellarmine is one in terms of derived rather than fundamental epistemic principles, and thus does not lead to relativistic conclusions.

favour—the astronomical evidence was not yet sufficiently strong to trump the Biblical evidence, due to the sort of issues of underdetermination that we laid out above—so that suspension of judgment was perhaps the appropriate response.

We claim, however, that such an account does not properly take into account the upshot of our historical analysis. As we have seen, Bellarmine does not use underdetermination and (Consensus) to weigh the Biblical evidence for geocentrism against the observational evidence against it. Rather, he invokes underdetermination to deny that Galileo's telescopic observations provide grounds for Copernicanism at all. Similarly, Galileo invokes his novel notion of demonstration and (Prudence), not to argue that the Biblical evidence is insufficient to support geocentrism, but rather to argue that the Bible does not provide independent evidence for geocentrism at all. Their way to disarm opposing evidence is not to claim that it is too weak, but rather to deny its relevance to the issue at hand. The issue of adjudication concerns what kind of information can be evidence for what kind of claim to begin with, and not merely the weighing of contrary evidence, as Boghossian claims.[27]

Once this is seen, it becomes hard to deny that different procedures of adjudication can give rise to fundamentally different systems. Even though Galileo and Bellarmine share their fundamental principles, and even nominally agree on the sort of adjudication principles that are in play, there is a deep mismatch between them concerning how to properly employ those principles of adjudication in the novel epistemic situation generated by Galileo's telescopic observations, so that they arrive at entirely different ideas of what it amounts to to gather the relevant evidence and use it to justify certain claims, resulting in radically different accounts of the justificatory status of Galileo's observations with regards to our beliefs about the make-up of the heavens. When faced with the question of heliocentrism, one will read the Holy Fathers' commentaries on the Bible, while the other will look through a telescope, and both will regard what the other does as fundamentally misplaced. Such differences cannot be brushed aside as merely "derivative" or "superficial," as

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27 In this way, our analysis of the historical debate allows us to flesh out Stephen D. Hales' suggestion that the kind of "genuine irreconcilable difference" that can motivate relativism arises when actors disagree over what evidence is relevant to the truth of a certain proposition to begin with, in a situation where "they cannot discover any mutually agreeable meta-evidence which would allow them to settle their dispute over first-order evidence" (2014, 80). What Hales calls "meta-evidence" corresponds to evidence ("reasons" is perhaps a better term here) for the procedures of adjudication.

is further brought out precisely by the deep incompatibility of both procedures and the way in which it is hopeless to try to justify them in a non-circular way.

This also allows us to re-evaluate the question whether (INSPIRATION) is a fundamental principle or not in Bellarmine's epistemic system. Boghossian characterizes as fundamental those principles "whose correctness cannot be derived from the correctness of other epistemic principles" (2006, 67). Both Boghossian and Seidel wish to suggest that (REVELATION) is not fundamental in this sense, because it is a derived principle that is justified by other epistemic principles. Presumably, they would say the same about (INSPIRATION).[28] Again, however, this slides over the issue of adjudication. The question is not whether an epistemic principle, abstractly formulated, *could* be derived from other principles, but whether it *is* so derived. It is a matter of how the principle is *used* in justifying beliefs.[29] Is it a principle that is taken to be only conditionally valid, on the basis of certain evidence and the use of other principles? Or is it a principle whose validity is not up for question, and which independently grounds the justification of beliefs and other principles? As our previous argument shows, this depends on the procedures of adjudication. If it is merely a matter of weighing the Biblical evidence against other evidence, it is plausible to take (INSPIRATION) to be a derived principle. But if it is a matter of granting Biblical evidence independent authority over a certain domain of propositions, as Bellarmine wished to do, (INSPIRATION) becomes fundamental: its use cannot be accounted for in terms of other fundamental principles. To properly understand the status of (INSPIRATION) in Bellarmine's epistemic system one must first understand his procedures of adjudication. These cannot be separated.

This also helps better to see what was at stake in Bellarmine's discussion with Galileo. Galileo's procedures of adjudication move us in the direction in which (INSPIRATION) may still be regarded as true, but will progressively become epistemically irrelevant with regards to matters of natural fact. Because the Bible is no longer regarded as having any self-standing authority regarding such matters of natural fact, it will simply be interpreted in accordance with the deliverances of science, thereby losing its status as an autonomous

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28 Compare Seidel's suggestion, discussed above, that the epistemic role of the Bible in Bellarmine's system can be accounted for in terms of the testimonial reliability of books.

29 See Kusch (2017) for related considerations, yet without highlighting the role of adjudication.

source of evidence.[30] In this way, choices in adjudication can give rise to the phenomenon that fundamental principles lose their epistemic standing. If the procedures of adjudication evolve in such a way that a principle no longer plays any independent role in justifying beliefs, it becomes merely derivative or even wholly irrelevant. This is what happened to (INSPIRATION) in the centuries following the dispute.

Such considerations also impact how we think about the purported absolute correctness of our epistemic principles. It might well be the case that principles like (OBSERVATION), (Deduction) and (Induction) play a role in all coherent epistemic systems that we can conceive of, which is definitely not the case for a principle like (INSPIRATION). But it is not clear what is gained by concluding from this that these principles must be absolutely correct. They are epistemically impotent if not embedded within an epistemic system that complements them with procedures of adjudication.[31] When deciding what to believe, we can never simply defer to the fundamental principles in isolation. Thus, if our epistemic procedures have an absolute grounding that renders them uniquely rational, this must be because the adjudicating principles themselves have such an absolute grounding. It is precisely this claim, we have argued, that the relativist calls into doubt on the basis of historical evidence.

3.3 *Blind entitlement and equal validity*

We are now in a position to revisit Boghossian's notion of blind entitlement. We already noted that Boghossian's appeal to blind entitlement begs the question against the relativist, insofar as he assumes that it allows one to establish a system's absolute correctness (see section 1.2). Relativists will agree, of course, that epistemic agents find themselves with an epistemic system that they are entitled to use. What is revealed by cases such as the Galileo/Bellarmine dispute, however, is that this does not thereby put these agents in a position to unequivocally address any novel epistemic situation that arises. In some

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30 As we saw, Galileo's own position was slightly more complicated in that he allowed (INSPIRATION) to provide evidence for the limited domain of claims about the natural world about which empirical research methods had to remain silent.

31 Note that this includes adjudications between applications of one and the same principle, e.g. when confronted with two seemingly conflicting observations. Compare how Galileo had to find a way to deal with the seemingly straightforward observation of the movement of the Sun in the sky, as mentioned above.

cases, their epistemic system, with its existing procedures for adjudication, simply does not provide a clear-cut answer to novel questions of justification. Thus, their blind entitlement does not put them in a position to establish the absolute correctness of whatever extended procedures of adjudication they end up settling on.

This line of reasoning allows us to locate more precisely at which exact point historical evidence militates against an appeal to absolute facts. Boghossian writes:

> As in the case of our linguistic and conceptual abilities, our ability to form rational beliefs is productive: on the basis of finite learning, we are able to form rational beliefs under a potential infinity of novel circumstances. The only plausible explanation for this is that we have, somehow, internalized a rule that tells us, in some general way, what it would be rational to believe under varying epistemic circumstances. (2008a, 483) [32]

No one can deny the minimal point that that what we learn puts us in a position to form rational beliefs in novel circumstances. The question is how it does so. Boghossian seems to think that it does so by antecedently fixing the rules that determine which beliefs it is rational to have in any novel circumstances whatsoever, so that our only task is to find out what those rules are, and apply them to our current situation. Our way of elaborating this minimal point, however, would be to say that what we have learned puts us in a position to develop new procedures of adjudication when required, in ways that rationally extend our existing epistemic system. Such rational extensions, however, can be open-ended, in the sense that nothing contained in the conjunction of our epistemic system and the novel circumstances need always determine a unique such rational extension (to be sure: it often does, but not always, and the difficult cases are usually those circumstances that are, in some sense, fundamentally novel). This is not to say, to be clear, that anything goes. To say that such rational extensions are not necessarily uniquely fixed, is not to say that they are not constrained. For instance, in the case of Bellarmine, resisting Copernicanism by disregarding Galileo's telescopic observations altogether would indeed be irrational. *Pace* Boghossian, however, this is not what Bellarmine did. Instead, he developed a principled way to assign an

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32 As we already pointed out, these internalized rules would have to include rules on how to adjudicate, if Boghossian's absolutism is to have any bearing on our actual epistemic practices.

epistemic status to those observations within his existing epistemic system, something he did by invoking antecedently acknowledged considerations of underdetermination and antecedently established practices of Bible interpretation. Here, one could proceed to ask: are such constraints on the rationality of such extensions then, at least, objective? That is: are there absolute facts of the matter as to which options are and which are not rational? We believe that it is not necessary for our project in this paper to take a stance on the matter. If we have shown that there can be fundamentally different yet equally rational ways of further developing an epistemic system when confronted with novel circumstances, we have established our target thesis of Equal Validity. It is not immediately clear to us what exactly would be at stake in the further question whether there are absolute facts of the matter with regards to which such developments are rational, and which are not. Indeed, for us, this is an indication that the initial way of framing the matter in terms of the absolute correctness of epistemic principles does not go to the heart of the matter.

Let us elaborate a bit on this point. Boghossian himself briefly considers what he calls "absolutist versions" of relativism (2006, 94fn5). He says that he wishes to take as his target "the much more radical 'postmodern' view which attempts to evade commitment to any absolute epistemic truths of any kind." He adds:

> It is easy to see what might motivate someone to take seriously the idea that there are no absolute epistemic truths of any kind; it is much harder to see what would motivate the moderate view that, while there are some absolute epistemic truths, there are many fewer than we had been inclined to suppose, or that they make essential reference to such parameters as a thinker's starting point.

We propose, however, that it is exactly historical cases such as the Galileo/Bellarmine dispute that could motivate such a "moderate" view. Boghossian does not seem to have a stable account of the relativist's main motivation. In his book, his point of departure is not the abstract claim that there are no absolute epistemic facts, but the thesis he calls "Equal Validity." It is this thesis that Boghossian finds proclaimed by his colleagues in the humanities and social sciences, and which he wishes to reject. Now, we have shown how careful attention to the historical evidence can be taken to confirm a thesis of Equal Validity, more precisely the thesis that there can be, in a given epistemic situation, multiple, equally valid ways of extending the procedures of

adjudication of an epistemic system. One upshot of this view is that different epistemic agents such as Galileo and Bellarmine can be equally justified in using fundamentally different epistemic procedures to justify their beliefs, procedures that result in their adopting contradictory beliefs on the basis of the same available information. We submit that it is such versions of Equal Validity, grounded in what we might call localized phenomena of symmetric open-endedness of epistemic systems with regards to matters of adjudication raised by certain novel epistemic situations, that are the primary focus of many relativists. Moreover, it seems to us that Boghossian would not be prepared to accept the existence of such thoroughgoing cases of epistemic symmetry—irrespective of whether that symmetry is taken to be "absolute" or "relative" in character—since he is at pains to argue that Bellarmine's epistemic procedures were indeed irrational, and that it is Galileo who should be said to have locked onto *the* correct epistemic system. By downplaying the issue of adjudication, the very nature of the issue that occupies the relativist threatens to remain invisible, since this open-endedness of matters of adjudication cannot be captured in terms of the absolute correctness (or not) of a certain set of self-standing fundamental epistemic principles. Once this is seen, the further technical question whether the Equal Validity at issue is itself to be conceived in absolutist or relativist terms, is of lesser importance. If it would turn out that, indeed, a relativist construal is incoherent, we expect relativists to respond along the lines of: "So be it. Let us become absolutists about Equal Validity". Rather than issuing in a blanket denial of the existence of absolute epistemic facts, the thesis of Equal Validity issues in a denial of the existence of a specific kind of absolute epistemic facts, facts that are meant to preclude the possibility of there being fundamentally different yet equally rational procedures for adjudication in a given epistemic situation. Even if Boghossian's argument that there must be absolute epistemic facts goes through, this does not refute Equal Validity, and thereby does not refute the sort of position that he initially presented as his target. Whether that position is in the end to be described as "relativist" or as "moderately absolutist" or something similar is a terminological question that is peripheral to the real philosophical issues at hand. What matters, is that it results in the claim that Bellarmine's and Galileo's epistemic procedures were equally rational in a way that is fundamentally at odds with Boghossian's absolutist commitments, commitments that themselves move beyond the mere blanket

assertion that there exist absolute epistemic facts.[33] To frame the debate as between a blanket assertion and a blanket denial of the existence of absolute epistemic facts, is to paint it with such a broad brush that all the underlying subtlety and complexity that renders it so interesting is erased, resulting in a picture that fails to adequately capture *both* the absolutist and the relativist position. It is the thesis of Equal Validity—and the question of how exactly to understand it—that should be the true locus of the debate.

4 Conclusion

As Boghossian characterizes the relativist argument, the relativist conclusion is meant to arise by considering how a confrontation with a fundamentally different epistemic system brings us to doubt the correctness of our own epistemic system. Our discussion reveals that this is not necessarily a good way to capture what the relativist is after. A more adequate formulation would be: the relativist conclusion arises from the observation that no epistemic system can, by itself, unequivocally settle all potential matters of adjudication that might arise in fundamentally novel epistemic situations. Although this is meant, of course, to undermine the idea that our own epistemic system, with its historically developed procedures of adjudication, is absolutely correct, this is not meant to bring into doubt the rationality of our using that system in deciding epistemic matters. Rather, it is meant to make us reconceive that rationality. If the focus is on historical cases, this reconception will have a backwards-looking character. By coming to recognize that Galileo's development of his epistemic system was only one of multiple equally valid ways to go, we come to recognize that an acknowledgment of the rationality of our own epistemic system—which is a product of Galileo's views—need not preclude the recognition that there were, at certain historical crossroads, other options available that were equally rational. At the same time, this recognition also has a forward-looking effect. After all, there is no way to exclude that we will encounter similar cross-roads, where we will ourselves be confronted with genuinely novel questions of justification to which our current epistemic system offers no clear-cut answers. What our analysis is

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33 Recall that Boghossian himself is quite aware of this, as is made apparent both by his recognition that he needs absolute epistemic facts to be in some sense accessible to us, and by his recognition that his position requires that we are able to establish *a priori* which principles of adjudication are correct. Neither of these claims are entailed by the mere claim that there are absolute epistemic facts.

meant to bring to the fore, is that to conceive of ourselves as rational does not mean to conceive of ourselves as being in the possession of an epistemic system that somehow deals in advance with all novel epistemic situations that scientific, technological, cultural, political, or any other kind of evolution may throw at us. It is meant to help us recognize that our capacity to deal with such situations is precisely that: a capacity to *deal* with them, to develop novel ways of proceeding where the epistemic tools we have at our disposal yield no determinate answer. It is meant, we could say, to help us self-consciously exercise our *creative* rationality, which is just as essential to who we are with regards to epistemic matters as it is with regards to any other.

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Perspectivity and Rationality of Perception

Kristjan Laasik

Susanna Schellenberg has presented several arguments for the "situation-dependency thesis" (SDT), i.e. the claim that (visual) perceptual experiences are necessarily situation-dependent, insofar as they represent objects' situation-dependent properties. In my critical response to her paper, I focus on her argument from the "epistemic dependence thesis" (EDT), according to which "perceptual knowledge of intrinsic properties is epistemically dependent on representations of the relevant situation-dependent properties" (Schellenberg 2008, 75). I consider what support she musters for EDT, so as to make an objection to her argument from EDT. To address this objection (or, rather, to bypass it), I will re-formulate the EDT, as a different but closely related thesis that I will call EDT*, informed by the admittedly radical Husserlian view that perception is epistemically rational.

In a paper titled "The Situation-Dependency of Perception," Susanna Schellenberg presents several arguments for the "situation-dependency thesis" (SDT), i.e. the idea that (visual) perceptual experiences are necessarily situation-dependent. One of her arguments involves an appeal to the "epistemic dependence thesis" (EDT), i.e. the claim that "perceptual knowledge of intrinsic properties is epistemically dependent on representations of the relevant situation-dependent properties," where intrinsic properties are the properties that do not depend on the object's relations to other individuals distinct from itself: e.g. its intrinsic size or shape, irrelative to the perceiver's viewpoint (2008, 75). In my critical response, I focus on the argument from the EDT, including what support she musters *for* the EDT, so as to make an objection to this argument. To address, or rather, to bypass, this kind of objection, I will re-formulate the EDT as EDT*, modifying Schellenberg's ideas concerning perspectival perception and perceptual epistemology along Husserlian lines.

In particular, EDT* is informed by the admittedly radical view that perception is epistemically rational, in the sense of being responsive to evidence.

As part of her account of the SDT, Schellenberg fleshes out the idea of situation-dependency in terms of perceptual experiences' necessarily representing situation-dependent properties, e.g. an object's situation-dependent size or shape, relative to the perceiver's point of view (2008, 56–57). The notion of a situation-dependent property provides a way of regarding the perspectival properties, and, thus, the perspectivity of perception, in mind-independent terms. Schellenberg's argument for the EDT is based on the claim that the defeat of one's perceptual evidence for situation-dependent properties necessarily brings with it the undercutting defeat of a line of evidence for intrinsic properties (but not vice versa). She complements such "asymmetry of defeat" with a similarly conceived "asymmetry of warrant." I will challenge these ideas by means of a counterexample.

However, I will also propose a peculiar re-formulation of EDT, viz., as EDT*, which, I believe, does not fall prey to this kind of objection, while still doing justice to Schellenberg's basic underlying intuition that "one perceives an object's intrinsic properties precisely because of the way the object is presented" (2008, 56–57). By contrast with Schellenberg's EDT, I will defend a thesis, according to which the perspectivity of perceptual experience is accounted for in terms of (subjective) appearance properties, not situation-dependent properties, and the pertinent relation of epistemic dependence is construed as obtaining between perceptual experiences and their aspects, not between beliefs or judgments. So, it is built into my account that perceptual experiences not only provide, but also receive evidential support—a radical idea which renders perceptual experiences epistemically rational, and which I propose to articulate in terms of the Husserlian notions of fulfillment and disappointment, i.e. a kind of experiential confirmation and disconfirmation.

Schellenberg argues for a view of the perspectivity of perceptual experience by appeal to the contributions that perceptual experiences make to our epistemic rationality. I choose the same starting point and try to deepen her line of thought, viz., by proposing that perceptual experiences themselves be regarded as rational.

1 The SDT and the Argument from the EDT

I will set the stage for the arguments of the present paper by giving an exposition of Schellenberg's central ideas and arguments. In general, Schel-

lenberg addresses the issue of how we can be said to perceive the intrinsic properties of physical objects, while perceiving such objects from different perspectives. In Section I of her paper, she contrasts her approach with naïve realist views, which downplay the perspectival nature of perception, insofar as they regard perception as direct and thus appear to have no need to appeal to situation-dependent properties, and traditional views which account for the perspectival aspect of perception by invoking mind-dependent objects or properties, like sense data or appearances. In Section II, she proceeds to set forth her own view, articulated by appeal to situation-dependent properties. According to her, situation-dependent properties are functions of the intrinsic properties of the object, and of the situational features, e.g. the perceiver's location or the lighting conditions. She adds that situation-dependent properties are, furthermore, ontologically dependent on and exclusively sensitive to intrinsic properties and situational features. Such a view renders the situation-dependent properties just as objective and mind-independent as the intrinsic properties, and can be presented in a rigorous way by invoking Christopher Peacocke's notions of scene and scenario content.

Schellenberg offers several lines of argument for SDT. Most prominently, in Section II, there is an argument based on the point that her view can do a good job accounting for Peacocke's example of a perceptual experience of two same-sized trees located at different distances from the subject. The example provides a way to scrutinize different accounts for whether they can render the content of perspectival perceptual experience consistent: we clearly want to avoid the idea that one perceives the two trees as being both the same size and not the same size. Schellenberg, however, addresses the problem elegantly, viz., by providing a formulation according to which we perceive the two trees as having the same intrinsic size and different situation-dependent size. Her title for her Section II, "The Argument for the Situation-Dependency Thesis," surely refers to this particular argument, insofar as it is the most prominent of all the considerations that she offers in support of her view in Section II. However, Section II also contains other considerations in favor of SDT, as well as an indication that still others will be put forward in the rest of her paper.[1]

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1 Thus, Schellenberg argues that her view has four advantages over alternative accounts. First, it brings with it the putative advantage of rendering the accuracy conditions of perceptual experiences richer, viz., by situation-dependent properties. The second putative advantage is that recognizing situation-dependent properties allows us to do justice to the fact that there is a wide range of viewing conditions that count as normal. Third, we are now in a position to appreciate the epistemic dependence of intrinsic properties on situation-dependent properties (Section

As for the EDT, Schellenberg gives a detailed discussion of this claim in her Section III, titled "The Argument for the Epistemic Dependence Thesis." The argument for the EDT is rightly regarded as part of an argument for the SDT. Schellenberg makes this clear at the end of Section III, "If representing intrinsic properties is [epistemically] dependent on representing their situation-dependent properties, then the representation of situation-dependent properties must be a necessary part of perceptual content" (2008, 80). In other words, by Schellenberg's lights, the EDT counts as support for the SDT. The argument from EDT appears to be dialectically at least as weighty as the argument concerning consistency of content (the pre-eminent argument in her Section II, as we have seen). I am saying this because several accounts of perspectivity unquestionably succeed in avoiding inconsistency of content—yet the argument from EDT can be viewed as providing a further principled consideration, enabling Schellenberg's view to prevail over this group of alternative views. Also, Schellenberg particularly directs it against the kind of "naïve direct realism" which proposes to altogether do away with the perspectival aspect of perception (2008, 75).

2 Defeat, Warrant, and the Argument for the EDT

As part of her argument for SDT from EDT, Schellenberg makes a case for EDT. I will proceed to clarify the EDT and her argument for it, and to bring a counterexample to EDT. Schellenberg articulates the argument for EDT by once again invoking Peacocke's tree example, viz., as focus of considerations pertaining to defeat of evidence,

> The subject has experiential evidence that the two trees are the same size. This evidence is, however, parasitic on her evidence that the nearer tree is presented as larger than the tree that is further away from her. Both layers of evidence are liable to defeat. However, if evidence for the situation-dependent properties is defeated, the subject's evidence for the intrinsic properties is de-

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III). And, fourth, if we accept her view, it will be possible for us to embrace the motivations of phenomenalism and indirect realism, while remaining direct realists (Section IV). This is a rich variety of ideas. In what follows, I will set aside all but the third claim of an advantage for her view, i.e. the argument from EDT: I believe that Schellenberg regards it as a more important argument than the other advantage-based considerations that she invokes, insofar as these are either un-developed (the first and the second) or clearly conceived as dependent on EDT (the fourth).

feated, but not vice versa. Defeaters can be understood in two ways. While undercutting defeaters block the line of evidence from which the warrant actually arises, rebutting defeaters provide independent lines of evidence warranting the contrary conclusion. If the subject's evidence for the situation-dependent properties is defeated, then her evidence for the intrinsic properties is undercut (and not just rebutted). (2008, 76–77)

This undercutting defeat claim is not devoid of prima facie plausibility.[2] Take P to be the experiential evidence to the effect that R, i.e. the nearer tree is presented as larger than the other. Take Q to be the experiential evidence to the effect that S, i.e. the nearer tree is the same size as the other. Schellenberg's claim is that if $\sim(P\rightarrow R)$ or $\sim R$, then $\sim(Q\rightarrow S)$. If we accept $\sim(P\rightarrow R)$, i.e. the idea that the evidence for R is undercut, then it appears not implausible that the line of evidence for S is also undercut. In other words, it seems quite plausible that if we cannot trust our experience with regard to R, i.e. the nearer tree's being presented as larger than the other, then neither can we trust it with regard to S, i.e. the nearer tree's being the same size as the other. On the other hand, we can make sense of the rebuttal of R as, likewise, undermining our trust in our senses. Thus, accepting $\sim R$ leaves us with an inconsistent triad P, $P\rightarrow R$, and $\sim R$. Now it is, again, natural to accept $\sim(P\rightarrow R)$, yielding the same situation as before.

Notice that these considerations seem to point to an *asymmetry* of defeat. Granting, for the sake of argument, that if I take a pill that distorts my experience of a tree's situation-dependent size, it will always also distort my experience of its intrinsic size, should we also concede the converse point, in cases where I take a pill that, first and foremost, distorts my experience of the tree's intrinsic size? We clearly should not, because the latter kind of pill might achieve its effect by confusing me about my distance from the tree, while leaving uncompromised my experience of its situation-dependent size.

To achieve a well-rounded appreciation of how these ideas bear on EDT, let us continue the above quotation,

> If the subject's evidence for the situation-dependent properties is defeated, then her evidence for the intrinsic properties is undercut (and not just rebutted). It follows from this that the asymmetry of defeat is grounded in an asymmetry of warrant. It is because

the evidence for the situation-dependent property is in the line of evidence for the intrinsic property that defeat of the former entails defeat of the latter. And it is because the evidence for the intrinsic property is not in the line of evidence for the situation-dependent property that defeat of the former does not entail defeat of the latter. Thus, evidence for intrinsic properties is dependent on evidence for situation-dependent properties both with regard to defeat and warrant. (2008, 77)

Remember that in our introduction we have already quoted EDT as the claim that "perceptual knowledge of intrinsic properties is epistemically dependent on representations of the relevant situation-dependent properties" (Schellenberg 2008, 75). We have now learned that it is to be unpacked as a claim about "asymmetry of defeat" *and* "asymmetry of warrant," where the former asymmetry claim is argued for directly, and the latter by a kind of inference to the best explanation. Schellenberg invites us to accept certain ideas concerning defeat, and then also that they are best accounted for by accepting certain points concerning warrant. Notice that, insofar as she makes specifications concerning rebutting and undercutting defeat, they are precisely what it takes to argue that the evidence for a certain situation-dependent property is, so to speak, part of the line of evidence that can warrant perceptual knowledge of an intrinsic property. In order to establish this, the defeat of the experiential evidence for situation-dependent properties needs precisely to result in the undercutting of the evidence for intrinsic properties, and not just, somehow, in its rebuttal by other lines of evidence. (There may or may not also be additional rebutting defeaters of the evidence for intrinsic properties.)

As far as I am concerned, there are just two points that Schellenberg has not made explicit. For one, she has not said what she means by asymmetry of warrant. In view of her detailed explanation of the asymmetry of defeat, it seems apt to conceive of the asymmetry of warrant in analogous terms: in the absence of experiential evidence that could yield a warrant for the relevant situation-dependent property, there also fails to obtain a line of experiential evidence that could yield a warrant for an intrinsic property; but not vice versa.[3] As for the second inexplicit aspect of Schellenberg's account, she never, in her discussion of the EDT, says what kind of item she has in mind as the recipient

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3 I take it that a body of evidence can fail to yield a warrant while not having been defeated. Instead, there may not have been sufficient evidence, or the evidence may not have been suitably interpreted.

of evidential support. She appears to be discussing perceptual experiences, and yet there is a philosophical consensus, which remains unchallenged (and even unmentioned) by her, to the effect that perceptual experiences, while providing evidential support for beliefs and judgments, cannot themselves receive evidential support.[4] It is therefore safe to assume that she is really talking about the defeat and warrant of evidence for perceptual beliefs or judgments.

EDT is open to objection by the following counterexample. Suppose that I am looking at a tree that is three meters tall. It has the situation-dependent property of appearing, from where I stand, to be the same height as the length of a pencil in my outstretched hand. But I am reliably informed that I have ingested a pill which makes it as likely as not that I experience the situation-dependent size of a tree as considerably smaller. E.g. if its real situation-dependent height is the same as the length of a pencil, I may experience it as being the same as the length of half a pencil. I am also told that whenever the pill does this to me, it will also increase the apparent distance between me and the tree, so that it will still appear to have the intrinsic property of being (of a height that could be described as) three meters tall. Now the evidence for the situation-dependent property has been undercut, but the evidence for the intrinsic property has not been in any way defeated. Our experiential evidence, in this case, does not yield a warrant for the tree's situation-dependent size, but it does for its intrinsic size.

It is possible to reply to this counterexample by arguing that it does not confute EDT, because in this case we are getting it right about the intrinsic size only by accident. Yet, I have been reliably informed that the pill regularly, predictably achieves its effects in me, altering my experience of distance whenever it alters my experience of situation-dependent size. We could, likewise, even conceive of perceivers whose visual system functions this way by default, without any need for the pill, and who are aware of this fact. Bear in mind also that by attaining such awareness of the workings of the pill, the perceivers cannot re-gain their warrant concerning situation-dependent properties: according to the setup of our counterexample, the pill may or may not alter one's experience of the situation-dependent properties. Therefore, the counterexample really does tell us something about situation-dependent properties, viz., that they cannot perform the epistemic role that EDT accords to them, and that they are not well-suited for developing the intuition that

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4 For a discussion of this view, and a challenge to it, see Siegel (2017).

"one perceives an object's intrinsic properties precisely because of the way the object is presented," as Schellenberg has sought to do (2008, 75).

Another worry about the counterexample is that if I am, indeed, informed by somebody that the pill has such an effect on me, then my evidence for the intrinsic properties is a combination of perceptual and testimonial evidence, with the upshot that we are no longer, strictly-speaking, dealing with perceptual knowledge of the intrinsic properties, and the scenario we have envisioned is therefore not a counterexample to EDT. It seems that here we can reply that our talk of the informant, just as our talk of the pill, is simply a convenient pointer, a device that we could, in principle, dispense with,[5] and invoke a situation where the subject has found out about the effects of the pill by himself, e.g. by comparing, as it were, a sample of visual experiential data from after taking the pill, with samples from other times. Perhaps, even in that case, our evidence would not be all perceptual, but would also have to involve memory and thought, leaving our counterexample with a problem? However, it seems to me that here, if we do accord a minimal role for (something like) memory and thought, they can be regarded, not so much as raising issues peculiar to our case, but as being, more generally, part of the enabling conditions of epistemic perception.[6] We could make a very limited appeal to (something like) memory and thought, so as not to have to invoke them as independent sources of evidence, but merely as part of what it takes to organize and interpret the perceptual evidence. I believe that such ideas fit naturally not only with a broadly Kantian outlook, but also with the Husserlian outlook that we will proceed to articulate in section 3 of this paper, viz., as functioning towards the "constitution" of the perceptual experience and its objects, with an emphasis on coordinating aspects of the diachronic experience, some of them retained and others merely anticipated.

Someone might voice the yet different concern that, in our scenario, we invoke a piece of evidence that defeats the perceptual evidence for the situation-dependent property, and then reach for additional evidence, e.g. from additional testimony, to ensure that we still have evidence for the intrinsic property.[7] Thus, our informant tells us that the pill may make the situation-dependent size appear smaller, *and* then she also tells us that when it does so,

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5 The idea of modifying our example, so as to leave out the pill, was already discussed in the previous paragraph.

6 For a discussion of the enabling conditions of epistemic perception, see Cassam (2007, sec.1.4), with various examples given, 37–38.

7 Here, too, keep in mind that testimony per se can be eliminated from our account.

it accordingly makes the distance appear greater. This may not seem right. But let us recall what claim we are questioning: "If the subject's evidence for the situation-dependent properties is defeated, then her evidence for the intrinsic properties is undercut [...]" (Schellenberg 2008, 77). We need to invoke a case where the evidence for the situation-dependent properties is defeated, but the evidence for the intrinsic properties is *not* undercut. Now, the worry is that if we invoke the evidence that, so to speak, *really* defeats the the subject's evidence for the situation-dependent property, viz., from the first piece of testimony, then the evidence for the intrinsic properties *is* undercut. Yet surely the evidence from the combination of the first and the second items of testimony *also* defeats the evidence for the situation-dependent property, but without undercutting the evidence for the intrinsic property. We thus have a counterexample to the EDT.[8]

3 An Alternative Account of the Epistemic Dependence

Taking as my starting point this objection to EDT, I will proceed to offer an alternative account of what I take to be the pertinent epistemic dependence relation. This is not to say that there may not be other ways of tackling the objection; I will try to do it in a way that relates interestingly to Schellenberg's own approach and builds on aspects of it. So as to bypass the problem that arose for situation-dependent properties, I will formulate my account in terms of subjective appearance properties.[9] Also, remember I pointed out that Schellenberg aims to give a kind of account of the nature of our perceptual

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8   Reviewers of this paper have suggested that there might be yet another way to challenge my counterexample to EDT, viz., by arguing that even if the counterexample renders false the conditional that Schellenberg uses to argue for EDT, the EDT could still be true. However, I have difficulty seeing how this could be so. Assume that the conditional is false: the evidence for the relevant situation-dependent properties is defeated, but the perceptual evidence for the intrinsic properties, instead of being undercut in its entirety, is either rebutted or remains undefeated. What this means is that, in addition to the line of evidence from representations of situation-dependent properties, there must also be some other line of perceptual evidence for the intrinsic properties, by virtue of which one could have perceptual knowledge of intrinsic properties, while not having representations of the relevant situation-dependent properties. If this is so, perceptual knowledge of intrinsic properties is *not* epistemically dependent on representations of the relevant situation-dependent properties, i.e. EDT is false.

9   In her 2008 paper, Schellenberg speaks of appearance properties as subjective or mind-dependent (2008, 72). In a more recent paper, the terminology has shifted, and she and her co-author argue that appearance properties can be understood either in mind-dependent or mind-independent terms (Green and Schellenberg 2018).

experiences, and yet, if rigorously spelled out, it instead seems to amount to an account of the epistemology of our perceptual beliefs. I merely put forward this thought as an observation, not, in any way, an additional objection to Schellenberg's view. But I will re-phrase EDT in such a way that it really does capture what I take to be a significant point about perceptual experiences: *perceptual experience* of intrinsic properties is epistemically dependent on experiences of the relevant appearance properties. This thesis—call it EDT*—I take to be supported by the consideration that if the experiential evidence in support of one's experience of an appearance property is either rebutted or undercut, then a line of experiential evidence in support of one's perceptual experience of an intrinsic property will be undercut. Likewise, absent the experiential evidence warranting an experience of an appearance property, there also fails to obtain a line of evidence warranting a perceptual experience of an intrinsic property.

The most pressing question our account would need to deal with is how we can be mistaken about subjective appearance properties—to be able to make sense of EDT* as being non-vacuous.[10] This is, prima facie, a quite difficult issue, but I believe we will be able to address it by drawing upon a Husserlian view of perceptual experience, especially the ideas of fulfillment and disappointment, i.e. kinds of experiential confirmation and disconfirmation. Let us briefly sketch this view.[11] On the assumption that the back sides of objects are perceptually experienced by us, the experience, the presence, of the back side must clearly be phenomenally different from that of the front side. To mark this distinction, let us call the experience of the front side "full" and the experience of the back side "empty." Now we can also call the transition from "empty" to "full" experience, e.g. as I turn the object around, "fulfillment." We can think of such fulfillment as a gradually cumulative process, as I examine the object in various ways. It yields a view that we can extend to our perceptual experience of the different perceptual properties, e.g. shape, size and color, not just as a way to think about the experience of the object's

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It also needs to be said that, in speaking of appearance properties, I am using the term "property" differently from Husserl. Husserl speaks about properties (*Eigenschaften*) in a more restrictive sense that would not be applicable to size, shape, or color appearances (1989, sec.1, chapter 2).

10 Schellenberg believes that we cannot be mistaken about subjective, mind-dependent appearance properties (2008, 74). (To remind the reader, Schellenberg's situation-dependent properties are conceived as objective, mind-independent.)

11 For a detailed, authoritative introduction to Husserl's view of perceptual experience, including a discussion of the significance of fullness and emptiness, see Bernet, Kern, and Marbach (1993, chap. 4).

back sides vs. front sides.[12] On the Husserlian view, the "empty" experience is conceived entirely in terms of more or less tacit anticipations of ways in which the experience might continue, or, as we might put it, anticipations of fullness. Indeed, according to this view, the presence of objects and their aspects in perceptual experience is conceived entirely in terms of such anticipations of fullness, realizing the fulfillment conditions for these objects and their aspects. We can thus say that the contents of perceptual experience are fulfillment conditions, rather than, say, accuracy conditions, which a certain mainstream view takes them to be. (Here, we need not argue that this mainstream view is in any ways problematic or even untenable.)[13]

I will illustrate certain salient aspects of our Husserlian view with a quotation from Husserl, viz., informing us that the perceptual object is given through "adumbrations" (*Abschattungen*), i.e. perspectival appearances, with the present adumbrations always pointing to the ones to come, insofar as the perceiver anticipates them, and it is by virtue of the having and fulfillment of such anticipations that one's experience of the object's front side can be integrated into an "omni-sided" experience,

> If, [...] in a perception, the series of appearances runs its course in continuous unity, then the first determination of the change, the so-called differential of movement, already defines the "direction" of the course, and thereby is given a system of intentions that are continuously setting out and continuously getting fulfilled. In normal perception, these are anticipatory intentions. (The series of appearances is dominated by a certain teleology.) Every phase refers to the following one. That, of course, should not be taken to mean that we focus on the appearances, since we are indeed directed to the object as the perception flows on. But every adumbration is precisely an adumbration of the square; every one "brings the square to appearance," but each in a different way. And every one brings to appearance something that previ-

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12 It has been argued that Schellenberg's idea of a situation-dependent property will not so readily generalize from size properties to other, e.g. shape, properties (Jagnow 2012, sec.2).

13 For a discussion of perceptual contents as accuracy conditions, see Siegel (2010, pt. 1).

Should anybody, at this stage, express the worry that our fulfillment-based view is a phenomenalist or an idealist one, whereas Schellenberg is concerned with accommodating perspectival perception as part of a realist account, we may reply that there are renowned Husserl scholars who regard such a view as realist, or at least compatible with realism. See, e.g. Crowell (2013, 16, 18), and Drummond (1990, 264–70).

ously did not appear, not precisely that way. In addition, each
one points forward: in the stream of appearances, the stream of
objective adumbrations, we feel ourselves drawn on from adum-
bration to adumbration; each one points forward objectively in a
continuity, and in this forward referral, the adumbration is an in-
timation of what is now coming, and the intimation, the allusion,
the intention, is fulfilled. The one-sided view opens out to what
is "omni-sided." Already in the case of a single determination, we
experience what this determination is, not in the one view with its
single adumbration, although it indeed stands there as self-given,
but only by traversing the adumbrations, whereby the determina-
tion is brought to a complete, "omni-sided" givenness. And this
complete givenness is constituted in the consciousness of unity
which produces a perpetual fusion of intention and fulfillment.
(1997, 86–87)

Having thus presented the basics of our Husserlian view, let us return to
the issue of how to make sense of our being mistaken about appearances.
According to our view, objects and their intrinsic properties are present to us
in terms of structured series of appearances, past, present, and future. The
future appearances are experienced by virtue of certain anticipations. There
is, of course, considerable leeway concerning what exactly one may be antic-
ipating while having a perceptual experience, say, of a red ball, but insofar
as the object is indeed experienced as being red, and as being a ball, and in
certain experiential circumstances rather than others, there are constraints on
the anticipations. Moreover, the thus constrained anticipations can be disap-
pointed (yielding disappointments of the relevant perceptual experience). My
point is that we can be wrong about appearances qua anticipated appearances.
Insofar as we are pursuing a certain structured line of appearances, e.g. in
perceptually experiencing a red ball, we are achieving fulfillments which
constitute the experiential evidence for certain upcoming appearances. If a
different appearance turns up, incompatible with these anticipations, it rebuts
the evidence based on which we had formed our anticipations, leading to the
formation of other anticipations, and to a re-configuration of our experience
in terms of another constellation of fulfillment conditions.

Someone might object to these remarks by suggesting that perceivers do not,
in fact, anticipate appearances. Instead, they may have anticipations about the
objects that they perceive. In reply, perceivers can indeed have anticipations

about the objects they perceive, but on the present view, the givenness of these objects is accounted for in terms of other anticipations that are not about objects but appearances, amounting to a condition of possibility of our perceptually experiencing objects. Such are the anticipations that Husserl speaks about in the above block quotation. As we have just learned, his according a role for these anticipations and appearances "should not be taken to mean that we focus on the appearances, since we are indeed directed to the object as the perception flows on" (1997, 86). Nevertheless, reflection can reveal the requisite anticipations and their fulfillments as aspects of perceptual experience.

Having spoken about the disappointment of tacit anticipations concerning appearances, we now turn to the disappointment of perceptual experiences themselves. Insofar as EDT* is about the undercutting of experiential evidence for intrinsic properties, it may not be very clear how it could be understood in terms of the Husserlian language of disappointments, because superficially it may seem that the Husserlian disappointment is in all cases basically a kind of rebuttal, rather than undercutting. Prima facie, the disappointment seems to consist in one's realizing, e.g. as the light changes, or as one takes a closer look, that what one took to be a red object, is really a green one, or that what one took to be a large object, is actually a small one.[14] This seems like a kind of rebuttal: e.g. the object cannot be red, despite appearing to be red before, in view of the new evidence that we just received, in the improved lighting, to the effect that it is green.[15]

However, this cannot be the general account of disappointments, insofar as a disappointment with regard to the object's redness does not necessarily yield an experience of the object as having some other color, such as green. It can also give way to a more or less deep perceptual confusion, or indeterminacy, where one is not sure what color one may be seeing. The nature of a disappointment does not consist in a rebuttal by a new perceptual constellation, but, rather, in the interruption of a series of appearances that was projected to continue into the future, even infinitely. The disappointment of a perceptual experience involves a kind of rebuttal, viz., of the anticipated appearance,

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14 To be clear, this is how Husserl himself describes some cases of disappointment. See e.g. (1973b, 88). I am not claiming that these are not really disappointments. Instead, I argue that not all aspects of such cases are necessary for a disappointment (in the Husserlian sense).

15 A rebuttal, say, of a hypothesis, does not generally require that one produce a superior alternative hypothesis. In our case, however, the rebutting evidence would, by the same token, also support an alternative "hypothesis."

yet does not itself consist in a rebuttal, but in an undercutting. The evidence, e.g. for redness, is corrupted or compromised, rather than just outweighed by new evidence. The fulfillment conditions for redness yield a set of structured infinite series of color appearances, ways in which one's experience of red can go, and any finite series of color experiences constitutes evidence for redness insofar as it forms part of any such infinite series. But once an appearance turns up that does not fit into such an infinite series, the support from the foregoing appearances is lost. Indeed, they are typically incorporated into another series, e.g. one consistent with the fulfillment conditions for greenness. Thus, Husserl argues that, in such a situation, a modification "takes place *retroactively* in the totality of the preceding series," e.g. as "the earlier apprehension, which was attuned to the harmonious development of the"red and uniformly round," is implicitly "reinterpreted" to "green on one side and dented" (1973b, 89).

Apart from the above point concerning the inapplicability of the idea of rebuttal, what other reason is there to believe that this is an adequate account of the phenomenon of perceptual disappointment? While there are, presumably, both gradual and abrupt perceptual disappointments, it seems to me that if disappointment were regarded as fundamentally a kind of rebuttal, the paradigmatic case would have to be that of a gradual disappointment, as new evidence emerges and gradually outweighs the previously existing evidence. But I think that in the paradigmatic cases the defeat is abrupt, e.g. as one just suddenly sees that the shape, size or color is not as one took it to be. This suggests that we are dealing with an undercutting: the existing evidence is vitiated by a new development.

But why should the cases of an abrupt disappointment, and not the others, be regarded as paradigmatic? One way to think about it is that the abrupt cases most straightforwardly realize the principle at work, viz., fullness prevailing over emptiness. Absent other considerations, present appearances win out against ones that have sunk back into the past—underscoring the fact that we do not adjudicate first-personal evidence from some detached perspective but respond to it, so to speak, from the midst of things, where this just means that present fullness impresses itself upon our consciousness in a privileged way. None of this militates against the consideration that if we add on other factors, e.g. the inertia of habit, or cognitive penetration, the past appearances may prove resilient, resistant to undermining.

We can now see that it is possible to make sense of EDT* in terms of the Husserlian view: if the experiential evidence for the appearance properties

is defeated, the pertinent evidence for the intrinsic properties will be under-cut. Therefore, perceptual experience of intrinsic properties is epistemically dependent on experiences of the relevant appearance properties. Thus conceived, EDT* is a compelling claim about the nature of perceptual experience and perceptual presence. Before, we did not look very deep into whether the problem highlighted by our counterexample was due to the idea of a situation-dependent property specifically, or the idea of a mind-independent perspectival property more generally, but with our view we have distanced ourselves from all such conceptions, and refrained from attempting to purge our conception of the perspectival nature of perceptual experience, of subjective ingredients. Our subjectivization of perspectival properties has the effect of rendering it more difficult (though not impossible, as we have seen) to defeat the experiential evidence in favor of them. It should therefore be unsurprising that it is now also more difficult to conceive of a case where the evidence for the perspectival property is defeated, but the relevant evidence for the intrinsic property is not. Indeed, I cannot think of a way to do it. Our above counterexample to Schellenberg's view has no bite against the present view. If, as in our scenario, I ingest a pill and it alters my experience so that the tree perspectivally appears the same height as the length of half a pencil in my outstretched hand, instead of how it might otherwise have appeared, then this is the perspectival property in terms of which the intrinsic height of the tree is experienced, consistent with the idea that the latter is epistemically dependent upon the former. By contrast, if we were experiencing the tree in terms of a series that led us to anticipate its appearing the same height as the length of half a pencil in our outstretched hand, and it surprised us by appearing otherwise, then the evidence for this perspectival height would be defeated, but so would the evidence for the relevant intrinsic height—unless the system of appearances associated with this intrinsic height allows for greater perspectival variety, at this juncture.

We have seen that, on the present view, perceptual experiences can be either supported or undermined by experiential evidence, in sharp contrast with the more commonly held view that perceptual experiences can *provide* evidential support for beliefs, but not *receive* evidential support themselves.[16] This amounts to the view that perceptual experiences are rational, in the sense of being responsive to evidence—providing a way to render cogent Schellenberg's talk of a relation of epistemic dependence between perspectival

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16 For a discussion of this topic, see Siegel (2017).

and intrinsic properties, in perceptual experience.[17] To be sure, we cannot revise our perceptual experiences in quite the same ways that we can revise our beliefs. Nor can we choose, or decide, to be disappointed. Nevertheless, we can be responsive to experiential evidence in choosing where to take our perceptual experience, which fulfillments to seek and how to render ourselves open to disappointments. In these regards, we can be praise- or blameworthy as perceivers.

In this paper, we have built towards the idea of the rationality of perception by invoking considerations specific to perceptual experience, but be it said that our position conforms with Husserl's general view of intentionality. For Husserl, every kind of intentional experience is associated with kinds of evidence which could support it by bringing the pertinent object or objectivity to fullness, which he also refers to as original givenness or self-givenness, "*The concept of any intentionality whatever*—any life-process of consciousness-of something or other—and *the concept of evidence, the intentionality that is the giving of something-itself, are essentially correlative*" (1969, 160). Husserl further elaborates on this point in relation to the idea of objectivity as such,

> *Category of objectivity and category of evidence are perfect correlates. To every fundamental species of objectivities*—as intentional unities maintainable throughout an intentional synthesis and, ultimately, as unities belonging to a possible "experience"—*a fundamental species of "experience," of evidence, corresponds*, and likewise a fundamental species of intentionally indicated evidential style in the possible enhancement of the perfection of the having of an objectivity itself. (1969, 161)

It is a natural and well-known part of this picture that perceptual experiences provide the requisite evidence for our beliefs and judgments, but we must not neglect the fact that perceptual experiences themselves harbor emptiness, which they can overcome (though never entirely) as they unfold through time. This shows perceptual experiences to be self-supporting, viz., by fulfillment.

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17 The present view of the rationality of perception bears considerable similarities to Susanna Siegel's ideas regarding the same topic (2017). I have already explored this connection elsewhere (cf. Laasik 2021).

4 Conclusion

Susanna Schellenberg argues for the situation dependency thesis (SDT); one of her arguments is by appeal to the epistemic dependence thesis (EDT), a claim of a kind of asymmetry of both defeat and warrant, which she phrases in terms of situation-dependent properties. I have objected to EDT by counterexample, and circumvented the objection by re-phrasing EDT as EDT*, viz., in terms of subjective appearance properties, rather than situation-dependent properties. I have developed this view as an aspect of a Husserlian view of perceptual experience, involving the ideas of fulfillment and disappointment. The view has the intriguing upshot that perceptual experience is rightly viewed as rational, viz., as responsive to experiential evidence—enabling us to deepen Schellenberg's central concern with the epistemic significance of the perspectivity of experience.*

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