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Hume Studies Volume XXIV, Number 1 (April, 1998) 171-194.

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SYMPOSIUM

A version of this paper was presented at the symposium on *Cognition and Commitment in Hume's Philosophy* by Don Garrett, held at the XXIVth International Hume Conference, Monterey, California, July 1997.

Ideas, Reason, and Skepticism: Replies to my Critics

DON GARRETT

I.

I am grateful to my good friend Margaret Wilson for her many useful and probing questions about Hume, his predecessors, and my interpretations of them. Her questions concern three main topics: the evidence for Hume's "Copy Principle," the implications of Hume's description of abstract ideas, and the views of pre-Humean rationalists. I will try to address these three topics in that order.

The Copy Principle

The questions to which Wilson devotes the most attention concern Hume's evidence for the Copy Principle. This is the principle that simple ideas, and hence ultimately the complex ideas that are composed of them as well, are causally derived from simple impressions that exactly resemble those simple ideas. As Wilson observes, I claim that the Copy Principle is "a relatively straightforward empirical claim" for which the evidence "within the context of Hume's cognitive psychology is reasonably strong." She also represents me as maintaining that the Copy Principle is supportable both by "introspection" and by "more strictly empirical observations in its favor." This is not a distinction among kinds of evidence that I meant to suggest. On the contrary, I think that nearly all of Hume's evidence for the Copy Principle depends, in one way or another, on *somebody's* introspection—either one's own or the reported introspections of others—and I regard such

introspections as being themselves fully “empirical,” as I understand that term.

Hume’s own procedure is to argue first for what I called the *Resemblance Thesis* of the Copy Principle (i.e., the thesis that every simple idea has a *resembling* impression that corresponds to it) and then for the further *Causal Thesis* of the Copy Principle (i.e., the thesis that those simple ideas that resemble simple impressions are partly *caused* by their resembling simple impressions). In the passages where he argues most directly for the Copy Principle, Hume’s explicit strategy for establishing the Resemblance Thesis is an appeal to common experience, coupled with a challenge to produce a counterexample. However, I argued that he has three main reasons for believing that this appeal (with its accompanying challenge) will be successful: (i) his confidence in Locke’s and his own handling of the best-known putative counterexamples; (ii) his awareness of the prevalence of disputes about the content and implications of non-imagistic “ideas of intellect” among many of those who claim to have methodological access to them; and (iii) his belief in the adequacy of his own theory of abstract ideas to explain the generality of thought without appeal to ideas that do not resemble impressions. His evidence for the subsequent Causal Thesis may be distinguished into two kinds of cases. In cases of the first kind, someone has a specified idea and is found to have previously had the corresponding impression. In cases of the second kind, someone has not (at a given time) had a specified idea and is found not to have previously had the corresponding impression.

Wilson raises two main objections to the evidence that Hume cites for his Copy Principle. The first objection is that much of the evidence, as he describes it, involves the claim that someone has previously had an impression with a certain content, even though impressions themselves are notoriously fleeting. Humean memory produces only ideas; it does not literally reproduce past impressions for direct comparison. So, Wilson concludes, “there is something like a conceptual absurdity...in the enterprise of trying to determine whether [an idea] was or was not copied exactly from an impression.”

I agree that the Humean faculty of memory uses ideas to represent certain objects—including impressions with certain contents—*as having existed*. But on *any* plausible theory of memory, memories represent at a later time how things were at an earlier time that is not literally co-present with those memories for purposes of direct comparison. This is so whether we take the memory and its object to be imagistic, as in Hume’s theory, or not. Does this lack of co-presence require us to reject our memories when they represent certain earlier mental contents as having existed? If we do *not* generally accept the deliverances of our memories, then it is hard to see how

we can suppose that we have much empirical evidence for anything—certainly we cannot have much *inductive* evidence for anything.¹ As I read Hume, he directly considers radical skepticism about memory in tandem with radical skepticism about induction and radical skepticism about the existence of bodies, near the beginning of *Treatise* I v 7; and he quite clearly rejects all three of these radical skepticisms (along with others) by the end of the same section, for reasons I tried to describe in Chapter 10 of *Cognition and Commitment*. That he does not consider such skepticism earlier in the *Treatise* is entirely in keeping with his procedure of investigating the operations of our faculties by means of our faculties, deferring the consideration of any skeptical worries until we have learned as much as we can of what those faculties say about their own and one another's operations.

Perhaps, however, Wilson does not mean to urge that Hume must be a skeptic about memory in general but rather to make a more Wittgensteinian claim: namely, that we cannot affirm the reliability of a kind of "private" memory—such as memory of past perceptions—without some external, independent, and/or public check. But Humean cognitive psychologists are presumably entitled to accept hypotheses about the production of particular impressions by particular physiological processes and to accept hypotheses about the production of particular pieces of verbal or other behavior by particular impressions; and if so, physical records of these physiological processes or bits of behavior can provide a person with some evidence that extends beyond his or her own memory about the content of previous impressions. Perhaps, as some followers of Wittgenstein suggest, a set of common physiological causes and behavioral effects even plays some logical role in *constituting* the "similarity" of private perceptions and in *underwriting* people's right to report introspectively on the similarities and differences of their experiences. Although Hume does not entertain such a suggestion—and it is by no means obviously right—I do not see why granting the suggestion would necessarily undermine his appeal to introspective reports about the similarities of past and present perceptions to support the Copy Principle. Why would it not instead simply be one explanation of how such appeals are underwritten?

Wilson's second objection to Hume's argument for the Copy Principle is that reasoning from cases of sensory deprivation does not "in itself, provide convincing support for" that principle. She grants that those who are systematically deprived of the impressions of a given sense may lack the corresponding ideas; but she remarks that it is conceivable that the occurrence of merely *some* simple ideas of a given sense would be sufficient to allow a person to *invent* other simple ideas of that sense for which no resembling impression had preceded. (She gives the example of an artist inventing a new color that he or she has never seen before.) On my

interpretation, however, Hume does not intend cases of very broad sensory deprivation to provide convincing support for the Copy Principle *in isolation*. Rather, he intends that they should be one part of a body of evidence that also includes: (i) many cases in which ideas are known to have followed their corresponding impressions and (ii) many cases of persons' narrow circumstantial failure to have just one *particular* simple impression and its corresponding idea. Hume's most memorable example of this latter kind is the taste of pineapple. As for artists inventing new colors, Hume might well concede that they may be able to imagine new shades of color on the basis of familiarity with a variety of *similar* colors (this is just a further extension of the example of the missing shade of blue); but he would certainly question whether someone familiar with only red and yellow could on that basis successfully imagine blue. That anyone could perform the latter feat is unlikely, but still *conceivable*—for Hume readily agrees that the Copy Principle is a just a matter of fact, not a relation of ideas.

Let me emphasize, however, that my defense of Hume's argument for the Copy Principle was only a limited one. In particular, I claim only (i) that Hume did not contradict himself by treating the principle as a priori or necessary, and (ii) that *within the context of Humean cognitive psychology* (i.e., within a context where his theory of abstraction and his accounts of the nature and origin of particular key ideas are regarded as at least plausible), Hume provided some relevant and reasonable evidence for the Copy Principle. I believe, as I think Wilson does as well, that Hume radically overestimates the role of images in cognition.² I also agree with her that committed Rationalists would not be forced merely by considering the temporal priority of impressions over imagistic ideas to abandon their own theory of intellectual, non-imagistic representations, representations that are not copied from impressions at all. I believe I suggested in *Cognition and Commitment* that Hume, too, would agree with this latter assessment of the force of his own arguments about images, and that he therefore rightly invested considerable effort in more specific arguments, in particular cases, that no such intellectual representations as had been alleged were actual, or even possible.

Before leaving the topic of the Copy Principle, Wilson raises two "quick questions" about it. The first is whether, in Humean cognitive psychology, coming to assent to the Causal Thesis of the Copy Principle involves a "felt determination of the mind" to associate impressions and their corresponding ideas. The answer to this question is surely "yes." One will feel this determination whenever one uses the Copy Principle to predict, upon having a new impression, that a resembling idea will sometime follow. One will also tend to associate the idea of an impression with the idea of its corresponding idea. This latter claim, of course, invokes Hume's explicit but only briefly explained distinction between an idea and an idea of that idea.

The other “quick question” concerns the normative use of the Copy Principle. I would answer it by saying, as I did in *Cognition and Commitment*, that the Copy Principle is meant to establish only a *prima facie* case against the existence of a particular idea, and that Hume always supplements this case with at least one other argument that is specific to the topic at hand.

Abstract Ideas

Wilson raises two main questions about Hume’s theory of abstract or general ideas. According to that theory, an abstract idea is a determinate idea of an exemplar, associated with a general term that disposes the mind to call up, as needed, any of what I called a “revival set” of ideas of other exemplars. Her first question is this: Given the essential roles played by the general term and the revival set in fixing the meaning of an abstract idea, how important is the determinate idea that actually serves as the abstract idea itself?

From a purely logical standpoint, Hume must agree that the determinate idea serving as the abstract idea itself is no more important than any other member of the revival set: for that idea could be relegated to the revival set and replaced by one of the members of that set without changing in any way the meaning of the word. Indeed, different people, and even the same person at different times, can use different ideas for this purpose. Still, he does say that we always invoke one.³ Here is one reason why he might think the occurrence of such an idea is important from a psychological standpoint. When he describes the role of the revival set in general judgments, it is always as a source of *counterexamples*. Thus, whenever we *dissent* from a general claim (i.e., a universal generalization), it is because we recall a counter-example from the revival set that is associated with a general term, a counterexample whose conception induces or constitutes dissent. Hence, when we *assent* to a general claim, it is presumably because *no* counter-example from the revival set comes to mind. But belief, for Hume, consists in the liveliness of some idea. So we still need at least one idea actually before the mind whose liveliness can constitute belief in the general claim affirmed; and it may well be that one important intended purpose of the exemplar serving as the abstract idea is to play this role.

The second question about Hume’s theory of abstract ideas is this: How determinate must ideas be? As Wilson herself rightly suggests, Hume’s short answer to this question will be that ideas must be exactly as determinate as impressions must be, since ideas differ from impressions in force and vivacity but not in content or logical character. For example, a visual idea must be determinate in the sense that the *minima imaginabilia* composing it each have a determinate color and occupy determinate spatial positions relative to one another—just as a television image must be determinate in

the sense that the pixels each have a determinate color and occupy determinate spatial positions relative to one another. But just as a determinate image of a dog on a television screen or the determinate impression of a dog in the mind may not determine a unique number of hairs being represented as being on the dog, so a determinate idea of a dog in the imagination need not determine a unique number of hairs represented either. Of course, an idea of a dog whose number of hairs cannot be determinately counted from a standard view of that dog is not the same thing as an idea of a dog whose hairs, when each counted individually via sufficiently close inspection, would *still* yield no determinate number. An idea of the latter kind is contradictory and impossible, in Hume's view.

The Rationalists

Wilson's first question about my treatment of the rationalists concerns my grounds for thinking that Descartes regards ideas of imagination as "inherently inadequate." 'Inadequate' is a quasi-technical term that Descartes uses to refer to cognition that does not capture all of the properties of the thing cognized. I did not use that term myself—I wrote only that he took ideas of imagination to be a lower form of cognition than ideas of intellect—but I certainly do think it evident that Descartes regarded imagination as inadequate cognition in his technical sense; he makes it clear in Meditation II, for example, that imagination cannot adequately represent the nature of substances. I also believe that he regarded ideas of imagination as inadequate in the somewhat stronger and less technical sense of being insufficient alone to provide any genuine knowledge; for example, he writes in Rule 8 of the *Rules for the Direction of the Mind* that "there can be no truth or falsity in the strict sense except in the intellect alone." All of this is completely compatible, in my view, with Descartes's frequent applications of the terms 'clear' and 'distinct' to images—where what is under discussion is their clarity and distinctness *as images*, with no implication that what is thus imagined exists as it is imagined. (This is "clarity" and "distinctness" as defined in *Principles of Philosophy* I 45, where the "clear" is what is "present and accessible to an attentive mind," and the "distinct" is what "contains within itself only what is clear.") It is also compatible with his stated view that imagining a figure can aid the intellect in finding truths about it and preventing the intellect from becoming fatigued.

Wilson's second question concerns my reasons for saying that Descartes thought that "causes can be seen to entail their effects." My main point about Descartes was simply that he seeks to derive the laws of nature from the a priori consideration of causes; my use of the term 'entailment' was rather analogical, and I wouldn't insist on it. Only in Spinoza, with his doctrine of the parallelism and identity of the causal orders of thought and

extension, does the entailment model of causation become an explicit and literal doctrine. Nevertheless, there is some evidence for such a conception operating in Descartes's thought, evidence which I cited. One modest bit of evidence is his Causal Containment Principle, to the effect that causes must contain their effects either formally or eminently. This principle is certainly a view about the *intelligibility* of causal relations, and it is at least suggestive of entailment: causes contain their effects, and premises contain their conclusions. In one respect, this principle still seems weaker than a full-fledged entailment model of causation; for although it sets strong a priori limits on what can cause what, it does not demand that causes *must* actually produce whatever is contained within them, as premises arguably *must* entail whatever follows from them. More important as a piece of evidence, however, is the particular *nature* of Descartes's a priori derivation of the laws of motion. Wilson cites Descartes's invocation of God's causal conservation of bodies, which seems tantamount to God's causal re-creation of them at particular places. In deriving the laws of motion, Descartes appeals to such aspects of the divine nature as immutability and simplicity in order to determine what these places will be. It seems plausible to think that he invokes this *divine* causation precisely because he finds that he cannot confidently see (i) the existence of a body at time t_1 as *alone entailing* the existence of the same body at time t_2 and (ii) the outcome of a collision as *alone entailed* by the motions and natures of the colliding bodies.

Wilson's third question about the rationalists is whether Malebranche did not find natural causation in general to be unintelligible, and not just the Cartesian mind-body interaction that I mentioned in connection with Descartes. I agree that he did; and I am happy to end on this note of complete agreement.

II.

My friend Peter Millican and I have been arguing with each other at Hume Conferences for several years—on the excursion buses and in the hotel lounges of several nations—about Hume's use of the term 'reason' in general and about the meaning of Hume's famous conclusion that (what we call) inductive inferences "are not determin'd by reason" in particular. I have benefited enormously from these discussions. My view is that (i) Hume uses the term 'reason' consistently and univocally, as a term in cognitive psychology, to designate our faculty of reaching assurance or assent by inference; and (ii) that his famous conclusion about induction, reflecting that sense, states that inductive inferences—while they are of course *instances* of reason or reasoning—are not *caused by* any instance of inference or reasoning. (They are not caused by any *further* instance of inference or reasoning *beyond* themselves; and although they are instances of reasoning,

they also cannot *cause themselves*, any more than anything else can on Hume's account.) Millican, in contrast, now proposes (if I understand him correctly) that Hume uses the term 'reason' in at least three different senses. The first of these is a "Lockean" sense, according to which reason is inference (either demonstrative or probable) that depends on "mental perception." This "Lockean" sense is supposed to be Hume's sense prior to and during the famous argument of *Treatise* I iii 6. According to Millican, that argument results in a "strict Humean" position—though, contrary to what one might guess from Millican's three-part diagram, a new sense of 'reason'—according to which *neither* demonstration *nor* probable reasoning is a kind of reason at all. This position is supposed to occur in a few pages that immediately follow Hume's famous conclusion about induction, although the alleged exclusion of demonstration comes only afterwards, at *Treatise* I iv 1.⁴ The *second* sense of 'reason' is a still later "loose Humean" sense that once again allows demonstration and probable reasoning to be kinds of reason, but this time *not* as kinds of "mental perception." In addition to these two senses, Millican acknowledges in two footnotes a "neutral" sense of 'reason' that evidently corresponds to what I claim is Hume's *only* sense of the term.

Millican begins by setting out sympathetically my arguments against the two dominant interpretations—which both of us reject—of Hume's famous conclusion about induction. He then argues that my interpretation does not meet the criteria of adequacy that are implicit in my own arguments: (i) compatibility with Hume's own use of the term 'reason'; (ii) compatibility with Hume's own assessments of the strength and consequences of his conclusion; and (iii) compatibility with the actual structure of Hume's argument. He thinks that I am thus hoist by my own petard, while his own three-sense interpretation—although initially suspect for its remarkable unannounced semantic promiscuity—is left alone to carry the day. Perhaps unsurprisingly, I find my interpretation to be relatively unscathed, and myself to be relatively unhoist, on all three counts.

'Reason'

In pressing the first count, Millican agrees with me that we should expect Hume to use a term such as 'reason' in its Lockean sense unless Hume indicates otherwise.⁵ We apparently disagree, however, about what Locke's sense of 'reason' is, and also on whether anything Hume says has any tendency to "indicate otherwise." I claim that, for both Locke and Hume, 'reason' always means the faculty that produces assurance through inference. Millican holds, if I understand him correctly, that for Locke and those following his usage "demonstration and induction fall unequivocally within the domain of reason" not because they are species of inference but only

“because they are founded on mental perception of evidential connexions.”⁶ It is certainly true, as Millican emphasizes, that just as Locke’s theory of how we perform demonstrative reasoning gives a prominent role to perception of the connections between ideas, so too Locke’s theory of how we perform probable reasoning gives a prominent role to perception of the probable connections—or, alternatively, the probability of connections—between ideas. I agree that Hume rejects this Lockean view in favor of the theory that probable reasoning is rooted in custom or habit. But I see the perception of connections and *probable connections* simply as a central element of Locke’s *theory* of how reason works, not as part of the *meaning* he assigns to the term ‘reason’; whereas Millican evidently sees such an assignment of meaning as providing the basis for an implied proliferation of senses of ‘reason’ in Hume.

Regardless of how we interpret Locke himself, however, I do not see how to make credible Millican’s suggestion that Hume successively deploys (at least) the three senses of the term ‘reason’ already described. Consider, for example, Hume’s famous argument itself. Hume affirms in the *Treatise* that neither of two species of “reasoning” that he holds to be jointly exhaustive and mutually exclusive (namely, demonstrative reason and probable reason) can play a certain specified role, and he then concludes that inductive inferences—which he also continues to call “reasonings”—are “not determin’d by reason.” Millican interprets Hume’s conclusion to mean the following: those mental operations that *are* instances of inductive “reasoning” in one sense of that term are *not* instances of reasoning in a “strict Humean” sense that is here making its paradoxical yet unannounced debut. But what then is the sense of the term ‘reasoning’ that occurs in Hume’s *premises*—the same sense, presumably, in which they really are “reasonings” that are said not to be determined by reason? If it is the “Lockean” sense, then Hume must regard his own argument as ultimately unsound—for he must *deny* the claim that probable reasoning *is* a kind of reasoning in either of those two proposed senses. Is Hume then using the term in its “loose Humean” sense, a sense that Millican also describes as “honorific”? But that doesn’t square with Millican’s account either, for he claims that the “loose Humean” sense is introduced only some pages *after*, and *in response to*, the famous argument about induction (and also after a dozen or so pages of the “strict Humean” position. (This looser “honorific” sense, by the way, is supposed to dominate the *Treatise* after T 103, although it is hard to see how the use of ‘reason’ in *Treatise* I iv—and especially in the section “Of scepticism with regard to reason”—could be regarded as in any way “honorific.”) The only remaining alternative is that these uses of ‘reasoning’ are to be understood in the neutral or naturalistic sense that Millican mentions only in his footnotes—that is, the sense of ‘reason’ that I propose. But now, since Hume’s famous argument does not actually mention

the mental “perception” that is crucial to Millican’s “strict Humean” sense, but does seem to be structured around the question of what produces inductive inferences, why not interpret Hume’s famous conclusion to mean what it seems to say: that is, that inductive inferences are not “determined by”—i.e., themselves causally produced by—other inferences, either demonstrative or probable? This makes Hume’s use of ‘reason’ in his conclusion consistent with his use of it in the rest of the section; and once the conclusion is understood in this way, there is no need to interpret any earlier or later occurrences of the term ‘reason’ in any other sense.⁷

This interpretation may perhaps be somewhat original—although it shares some common ground with Fogelin’s incisive treatment of Hume’s argument about induction as a “no-argument argument”⁸—but I do not think that it is as “radical” in its consequences as Millican suggests. For as I have argued previously, Hume’s project is to investigate our faculties by using our faculties, and reason—the inferential faculty—is one of the chief faculties that is both investigated and investigating. Hence, to say that Hume uses the term ‘reason’ “neutrally,” as a term for the inferential faculty, is not at all to say that what he discovers about that faculty has no normative epistemic *fallout*. What he discovers about reason—which includes his famous conclusion about induction, but considerably more as well—has *enormous* normative epistemic fallout. Hume does, however, wisely defer the *assessment* of that fallout until after he has collected together *all* of his discoveries in the final section of Book I of the *Treatise* (and, largely, to the final section of the first *Enquiry* as well).

Millican is absolutely right to emphasize that Hume’s famous conclusion involves a rejection of Locke’s view that all reasonings, including what we call inductive inference, involve a mental perception of either connections or probable connections between ideas—a point that I did not emphasize in my book. I simply do not think that Hume’s implicit rejection of this view involves a proliferation of senses of ‘reason’. In the course of discussing Millican’s second count against my interpretation, I will explain how Hume’s conclusion—on my understanding of it—*does* serve to reject (among other things) a view of probable reason suggested by Locke.

Hume’s Conclusion

Millican’s second count concerns Hume’s own view of the strength of his famous conclusion. In *Cognition and Commitment* I sometimes characterized Hume as rejecting the view that induction is caused by “higher-level argument concerning induction’s reliability.” From this, Millican understandably infers that on my interpretation “it is only the *general practice* of induction that fails to be determined by reason, and each of our *particular* inductive inferences is itself an instance of the operation of our reason.” He

therefore goes on to cite several passages in which Hume seems to apply his famous conclusion about the lack of determination by reason to the making of particular inductive inferences. The crucial distinction for Hume, however, is not between individual inductive inferences and inductive inferences as a class. Rather, it is the distinction that I have been emphasizing, between an inference being an *instance* of reasoning and the same inference being *caused by* (another instance of) reasoning. It is by overlooking this distinction—i.e., by treating “determin’d by reason” and “is an instance of the operation of our reason” as equivalent, so as to emphasize a distinction between the general and the particular—that Millican’s just-quoted paraphrase of my interpretation unintentionally misses the mark. If we are to understand Hume’s claim that inductive *reasonings* are not determined by *reason*, we must distinguish between (i) the claim that inductive inferences are each *instances* of reasoning and (hence) exercises of reason and (ii) the claim that inductive inferences (whether as a class or in a particular instance) are acts of inference that are themselves *caused* by some *other* inference or act of reasoning—for example, an act of reasoning about the relation of observed to unobserved cases. The first of these two claims Hume consistently accepts; the second he consistently denies and argues vigorously against.

It is crucial, as Millican suggests, for us to be able to see what Hume’s famous conclusion rules out. In addition, it is important to the plausibility of an interpretation of the famous conclusion that it allow Hume to use it to rule out something that he would really want to rule out. Of course, one thing the famous conclusion rules out, on my reading, is the theory that we make an inductive inference as the result of another inference about the reliability of such inferences. Millican, in effect, expresses reasonable doubts about whether any of Hume’s contemporaries actually held any such theory and hence about whether Hume would have had any motive to rule it out. Thus, Millican writes that “it was no part of Lockean orthodoxy to suppose than our inductive practices were founded on an argument establishing their reliability.”

Even if we were to assume that the theory just mentioned were the *only* thing ruled out by Hume’s conclusion under my interpretation, I don’t think that Millican’s remark would settle the matter. For since Hume states that his conclusion is the answer to a “question [that] is yet new” [EHU 36], it seems that it could well be possible for that conclusion to “throw men loose from all common systems” *without* a particular theory that Hume rejects having occurred as an explicit *part* of those systems. Furthermore, Locke does notoriously claim, in his account of our idea of “power,” that after observing inward and outward changes the mind “concludes from what it has so constantly observed to have been, that the like Changes will for the future be made, in the same things, by like Agents, and by the like ways...” (ECHU

II xxi 1). Not surprisingly (given the self-proclaimed novelty of Hume's question), this remark of Locke's is ambiguous between the mild claim that each of these inductions is itself an inference and the stronger claim that we engage in these inferences because of an inference or argument about the general relation of past to future; but the remark at least strongly suggests the latter. Similarly, Locke's discussion of "the conformity of any thing with our own Knowledge, Observation, and Experience" as one of the two "grounds" of probable judgment (ECHU IV xv 4; testimony is the other) might well be taken to imply that something could be said in defense of treating it as such. Certainly Hume writes as though this theory is at least *part* of what he wants to rule out when he summarizes his conclusion in EHU XII ii as follows:

[W]e have no argument to convince us, that objects, which have, in our experience, been frequently conjoined, will likewise, in other instances, be conjoined in the same manner; and that nothing leads us to this inference but custom or a certain instinct of our nature....
(EHU 159)

In fact, however this theory is *not* the *only* thing ruled out by Hume's famous conclusion, on the reading I have given of it. This is an important point that I did not fully appreciate until after I wrote *Cognition and Commitment*, and it is one that I likely would never have discovered were it not from the stimulating pressure of conversations and correspondence with Millican and with David Owen.⁹ In order to explain what else it rules out, I must first say just a bit more about Locke's theory of demonstrative reasoning.

For Locke, demonstrative reasoning is a process in which we come to see "agreements" or connections between ideas by the use of intermediate ideas (which he calls "proofs"); the immediate perception of such a connection is an *intuition*, and demonstrative inferences are accomplished, via proofs, by trains of two or more intuitions. In a one-step demonstration, then, we *begin* with the perception of a connection between idea A and idea B, and we *conclude* by perceiving that there is a connection between idea A and idea C. The original perception of the connection between A and B itself is an *intuition*. But there is a gap between this initial intuited premise and the final conclusion. This is a gap about the connection between B and C; and it is only *by* perceiving a connection between B and C that we can come to perceive the connection between A and C. This gap is filled by a second intuition, which is precisely an intuition of the connection between B and C. In this simple case, then, the inference from our previous knowledge of the connection between A and B to our new knowledge of the connection between A and C is *caused by* (or, we may say, "determin'd by") the intuition of the connexion between B and C. This second intuition is not itself an

argument or inference for Locke, because it is too simple to have any steps, but it is certainly an *act of the understanding*.

Few Lockean demonstrations, however, will consist of a single step. Now consider a Lockean two-step demonstration, which is more complex. We *begin*, as before, by perceiving a connection between A and B, and we *conclude* by perceiving a connection between A and D. In this case, the initial gap concerns the connection between B and D; and this gap will be filled, not by a single intuition, but by a *demonstrative argument* that *begins* with an intuition of the connection between B and C and then *proceeds through* an intuition of the connexion between C and D *to reach a conclusion* about the connexion between B and D. The latter two intuitive steps just mentioned together constitute a demonstrative argument in their own right—a demonstration of the connection between B and D—which is all that we need to add to our original intuition (of the connection between A and B) to reach a conclusion about the connection between A and D. Hence, the inference to the relation between A and D that these two steps serve to mediate is *caused* (i.e., *determin'd*) *by reason*, in the form a piece of reasoning about the connexion between B and D.

Accordingly, Locke's view of demonstration entails that a demonstrative inference is always *caused* by either a single intuition or by another piece of demonstrative inference. Locke's account of probable reasoning is less clear—something that Hume no doubt noticed and found suggestive. However, Locke does make it plain that probable reasoning always involves “proofs” (plural); that it uses these to take as connected or agreeing ideas not actually perceived to be connected or agreeing; and that in doing so it relies on relating these proofs to the conclusion assented to via one or both of the two “grounds” of probability—namely, testimony and “conformity to past experience.” Locke at least strongly suggests that probable reasoning, too, is often or always caused by some intermediate process of reasoning involving “proofs” or intermediate ideas.

Hume can largely accept Locke's account of demonstration (although he says less that he should about the difference there is on his own view between perceiving and not perceiving a “relation of ideas”). However, Hume's famous argument about induction entails that a theory of probable reasoning giving similar prominence to causation by reasoning cannot be right. In an inductive inference, Hume argues, the transition of thought is from our assurance (impression or memory) of the cause (this paper being ignited, say) to assurance of the effect (this paper being consumed). In order for reasoning to cause an inference from one to the other, it would have to produce (via another inference) a conclusion about the gap that the first inference spans. We cannot span such a gap prior to having had any experience, Hume argues, because we make these inferences only after experience of constant conjunction. That is, we proceed from the observed

constant conjunction of earlier Cs with later Es, together with an observation or memory of *this* C, to a conclusion that constitutes a belief in the occurrence of *another* E. Hence, the gap in this case between premise and conclusion is a gap between a set of observed conjunctions and an unobserved one. If, therefore, *reason* were to cause us to make this inductive inference, it would produce assent to a judgment about the *relation or connection* between these observed cases and one or more unobserved ones. According to Hume, however, there are only two kinds of reasoning: demonstrative and probable; so if reason were to bridge the gap by causing an inference about this relation, it would have to perform this feat either via demonstrative reasoning or probable reasoning. Hume then argues that neither kind of argument can accomplish this task. Hence, *reason* does not determine inductive inferences. Although he does not happen to say so in the *Treatise*, his argument that no demonstrative argument could bridge the needed gap also shows that it cannot be bridged by intuition, since the denial of intuited truths is also always inconceivable. The *Enquiry* version of Hume's famous argument makes this point satisfyingly explicit; and it also casts more explicit doubt on the need for any intermediate "proofs" at all, functioning in any way, by asking "by what medium" this inference could be accomplished. At the same time that Hume expands his argument in the *Enquiry*, however, he also expands the famous conclusion to rule out any "reasoning or process of the understanding," thereby eliminating such non-inferential processes of the understanding as intuition or the perception of a probable connection between even a single "proof" and a conclusion.

Of course, it may well happen, as Millican notes, that one piece of probable reasoning is part of another piece of probable reasoning; this is something that Hume surely does not mean to deny, since he allows that not all probable inferences are immediate. But as Hume states his conclusion (in a passage that Millican cites), it is that "in all reasonings from experience, there is a step taken by the mind which is not supported by any argument or process of the understanding" (EHU 41; emphasis added). Where a piece of probable reasoning does occur as part of a second piece of probable reasoning, it will involve a crucial step that is not supported by any argument or process of the understanding; that is the point of Hume's discovery. Reasoning cannot cause the crossing of an inductive gap.

Millican urges one other reason for thinking that my reading of Hume's famous conclusion does not match Hume's own view of it. He does so by citing a well-known passage from *Treatise* I iii 12 (T 139), where (as elsewhere) Hume remarks that we can "give no reason" for making inductive inferences. I had cited this passage to support my interpretation, but Millican suggests that I am not entitled to do so, since the passage expresses a stronger claim than the conclusion I attribute to Hume. I am afraid that I don't see the force of this objection; for on my interpretation, Hume shows

that there is no piece of reasoning or argumentation that causes us to make inductive inferences, and to have "a reason" to give for making an inductive inference would be to have the basis of an argument or a piece of reasoning.

Of course, if we ask whether a stone will fall, we have, in a way, many "reasons" to give for our affirmative *conclusion*. These reasons consist simply in the observed past instances in which stones have fallen, and we can reason directly from them to our conclusion about the future stone. But if we ask the further question of *why* past instances of stones falling should be expected to be followed by future instances of the same behavior, there we have *no* reasons to give for making our *inference*. Our confidence *that future stones will resemble past ones* is not a result of reasoning or inference, but is rather the *precondition* of probable reasoning about stones. Our belief in the conclusion either about a particular stone or about stones generally is a product of probable reasoning; but our belief in the connection between the premises and the conclusion of the inference is not, and we do not generally *make* the inductive inference that produces the belief about stones as a *result* of any other inference.

Hume's Argument

In pressing his third and final count—concerning the structure of Hume's argument—Millican claims that my interpretation of Hume's famous conclusion is in one respect too weak and in another respect too strong to match the argument itself. Rightly or wrongly, however, I persist in thinking that I have offered a "Goldilocks" interpretation—neither too strong nor too weak, but just right. I will take up the two objections in order.

Millican holds that my version of Hume's conclusion is *too weak* because Hume takes care in the *Enquiry* to rule out not only *inferential* causes for induction but also sensory and intuitive ones. I cannot see Hume clearly considering the idea that sensation *itself* causes inductive inference—just the idea that it provides premises about sensible qualities that could cause inductive inferences via an argument from sensible qualities to "secret powers." I do see, thanks to Millican's observation, that Hume's argument in the *Enquiry* (unlike that in the *Treatise*) rules out the alternative that inductive inferences are produced by an *intuition* of a connection between past and future. But, as I have just noted, Hume's *conclusion* in the *Enquiry* is also different from that in the *Treatise*, in a precisely corresponding way. There, he claims that inferences from experience are not "founded on reasoning, or *any process of the understanding*." As I read Hume, 'the understanding' signifies all of our cognitive processes—including intuition—and is therefore rather broader than the inferential 'reasoning'.¹⁰ Hence, it appears that Hume wisely broadens his conclusion in the *Enquiry* to match his broader argument. I did not note this addition in my book, and it is worth

noting; but it does not affect my interpretation of Hume's use of the terms 'reason' and 'reasoning' as univocal.

Finally, Millican claims that my version of Hume's conclusion is also *too strong*, on the grounds that Hume's actual argument concerns only what can be justified by normatively legitimate reasoning, and not what can be caused by any reasoning whatsoever. Millican and I agree, I think, that this is his most important objection. However, I still read Hume as arguing from the causal limits of actual human reasoning, reasoning whose nature he has already investigated. In particular, he has already provided an account of what all demonstrative reasoning does (that is, it provides apprehension of relations-of-ideas whose denials are inconceivable) and of what all probable reasoning does (that is, it projects observed constant conjunctions onto unobserved cases). Of the various "musts" and "cannots" that Millican cites, some are definitional. (For example, any argument for the Uniformity Principle *must* be either demonstrative or probable because these two kinds of arguments are defined in terms of jointly exhaustive kinds of "evidence" or assurance; things whose contraries are conceivable cannot be demonstrated, because demonstrations are defined as arguments producing *knowledge* with inconceivable denials.) Others are causal. (For example, we *cannot* "be engaged by arguments to put trust in past experience" [EHU 35] by probable arguments because the operation of the mechanism of any probable argument itself already involves *de facto* trust in past experience.) None of these "musts" and "cannots," so far as I can see, is normative.

Hume's philosophical project, as I understand it, is to investigate our cognitive faculties for the light that such an investigation can shed on a variety of moral, religious, and epistemological questions. Hume disparages what he calls (in EHU XII i) "antecedent skepticism"—i.e., doubting our faculties prior to an investigation of their operations. This means that he carries out his investigation of our common cognitive faculties using—"what else?" as Quine is fond of saying in similar contexts—our own common cognitive faculties. The results of this investigation tend to call into question *some* operations of some of these faculties, and hence they provide a prospect for improving our cognitive operations as a result of turning them onto themselves. Each individual cognitive operation is thus treated "descriptively" or "neutrally" rather than "normatively," in the sense that none is beyond question. But because the investigation is also carried out *with* our faculties, there are many contexts in which what they say can and must carry at least provisional epistemic weight. I therefore reject Millican's claim that I interpret Hume as holding that reason has no "presumption to normative intellectual authority"; as one of our most important cognitive faculties, it does carry a—rebuttable—presumption to authority. This does not mean, however, that we must distinguish separate "neutral" and "normative" senses of 'reason'.

Consider an analogy with the term 'the senses' as it might be used by a cognitive psychologist. She may conceive of *the senses* as the usual source of a certain kind of experience—sensation. Naturally, the cognitive psychologist will begin investigating the senses by using her senses, and she will give at least provisional weight to the information that she derives from them in doing so. She may then determine, by investigation, that there are five (or more) distinct sensory mechanisms. Defining *hearing* as the sense all of whose sensations are sounds, she may conclude that digestion "cannot" be a function of hearing. Finding only five human senses, she may conclude that *any* sensation "must" result from one or another of them. Based on her empirical causal investigation of their mechanisms, she may also conclude that there are certain things that the senses causally "cannot" do—say, provide information about a distant place without physical signaling. She may also conclude that some deliverances of the senses involve disturbances of their natural operations due to external forces, and that some apparent operations of the senses (e.g., hallucinations) are not really operations of the senses at all. Finally, she may conclude that some deliverances of the senses are to be rejected as illusory or unreliable. But none of this means that she uses the term 'the senses' ambiguously, in three (or more) different senses. Rather, she is simply distinguishing a set of processes by distinguishing their typical products and then investigating their mechanisms through a cognitive procedure that grants provisional epistemic weight to the results of those very processes. This procedure might even lead her to consider, and to reject, total skepticism about the senses—just as a similar process led Hume to consider, and to reject, total skepticism about reason.

As Millican rightly emphasizes, Hume is well aware that people can offer bad as well as good arguments. Hume does not provide any arguments specifically designed to block the rather implausible hypothesis that we are caused to perform inductive inferences by a fallacious attempted demonstration—the denial of whose conclusion would be conceivable. But such attempts, even if convincing, would not be instances of genuine demonstration operating alone—he often calls them "supposed demonstrations"—but rather of what Hume calls (in T I iv 1) "the irruption of other causes" operating in addition to or in place of the operations of our inferential faculty. (In a similar way, hallucinations are not genuine operations of our senses but are events that may be mistaken for them.) As for "bad" (or "unphilosophical") *probable* inferences, even the worst of them, according to Hume, already rely on the prior mechanism of projecting an observed regularity onto unobserved cases. Hence, they can no more be the underlying cause of inductive inference than "good" or "philosophical" ones can.

Millican argues that the "simplicity and elegance" of my interpretation come at considerable costs in coherence. I have argued that we need not pay

any such costs. Nor need we pay the costs that Millican suggests in “relevance” or “interest.” Hume’s project of using cognitive psychology to *first* investigate and *then* evaluate our cognitive faculties—the project that leads him to prefer what he calls “consequent scepticism”—is, I propose, methodologically far more sophisticated and far more relevant to contemporary epistemological projects than are the projects attributed to Hume in older interpretations. In short, we can have the full benefits of Millican’s insight—that Hume rejects theories of probable reasoning of the kind likely to be suggested by a reading of Locke—without paying the cost of attributing to Hume several unannounced equivocations on one the most central terms of his “science of man.”

III.

I have learned a great deal about Hume (and many other philosophical topics) from my friend and former teacher, Robert Fogelin. However, he has long influenced my thinking about Hume in another, more immediate way as well. My own personal abstract idea of “Humean virtuous person”—that is, of a person with a generous array of mental qualities useful and agreeable to the possessor and others, mixed in just the right proportions—has many ideas in its associated “revival set.” But for many years now, the idea that serves as my *exemplar* of “Humean virtuous person” is my idea of Bob Fogelin.

Fogelin begins his remarks by chronicling three stages in the evolution of his attitude towards inconsistency. He then gently chides me for failing to recognize the irreconcilable contradictions among the positions of the “gentlemanly Hume,” the “wise Hume,” and the “Pyrrhonian Hume,” all of whom he takes to represent unresolved aspects of, or perspectives within, Hume’s philosophy. Specifically, he claims that I paint too “pretty” a picture of Hume’s philosophy, a picture in which the “wise” Hume reigns supreme. I do indeed believe that Hume’s philosophy involves a clear, if somewhat uneasy and costly, victory of the wise Hume over both unphilosophical gentlemanliness and Pyrrhonism. Before explaining how and why I believe that, however, I would like to emphasize that Fogelin’s three theses about inconsistency are not inconsistent with my own views.¹¹

I am rather agnostic about Fogelin’s claim that it is “characteristic of most great philosophic positions to be deeply impregnated with inconsistency or other forms of incoherence.” I do find myself convinced that the philosophers to whom I have devoted the most interpretive energy are relatively free from downright inconsistency (although they do exhibit some, and they certainly affirm many falsehoods). Perhaps I should infer from this that, with more study on my part, other philosophers would prove

largely consistent as well. But perhaps the explanation for my experience of consistency is simply that many of us work on certain philosophers more than on others in part because we *like* them as philosophers, and that one of the main things that I like in a philosopher is a high level of consistency, so that I do not continue working as hard on philosophers who do not seem to reveal it.

I agree with Fogelin in rejecting “the hermeneutical principle that, in the interest of charity, one should make every effort not to attribute an inconsistency to a philosopher of stature.” He also writes of “avoid[ing] attributions of inconsistency at all costs”; and I certainly agree that some costs for avoiding attributions of inconsistency would be too high. My own experience with Hume—and with some other philosophers, historical and contemporary—has convinced me of the tactical value of *focusing* on apparent contradictions with an eye to determining *whether* they are real or only apparent. If, however, the Hume I present is freer from inconsistency than the one presented by some others, that is not because I began with the hermeneutical principle that Fogelin rejects, but because study of Hume’s writings and intellectual context led me to conclude that what appear at first reading to be contradictions in his works are more often the results of natural misunderstandings—including some that occur when we assume that our own ideolect, preoccupations, or presumptions are also Hume’s. For my part, I find the resulting, more consistent Hume to be deeper and more philosophically interesting than the inconsistent one he replaces.

Finally, I agree with Fogelin that a system of beliefs can be useful while containing inconsistencies. I would go further, and add that such a system can even be mostly true, so long as a way is provided to prevent the system’s inconsistencies from permitting the derivation and subsequent inclusion within the system of all propositions whatever. Indeed, I emphasized in my book that Hume himself argued in *Treatise* I iv 1 (“Of scepticism with regard to reason”) that the set of probability assessments producible by standard probable reasoning from any body of experiential evidence that includes experience of one’s own cognitive errors *constitutes* an inconsistent system; and, as I noted, the question of how to prevent this finding from destroying the usefulness and acceptability of our entire system of beliefs is one of the primary topics of *Treatise* I iv 7 (“Conclusion of this book”).

Fogelin mentions Wittgenstein as the primary influence on his own attitude toward inconsistency, and it is Wittgenstein who remarks at several points that one response to Russell’s paradox would be simply to say, “Do not go that way.” (Perhaps in more contemporary lingo: “Don’t go there.”) Wittgenstein means by this that a defensible reaction to finding an inconsistency in an otherwise useful system might simply be to avoid *using* it to derive contradictions or other unwanted results. On my interpretation, this is precisely the attitude that Hume himself ultimately adopts and

recommends about “scepticism with regard to reason.” His ultimate solution to the conundrum posed by this section, I claim, is precisely to forswear certain refined and elaborate but otherwise standard uses of reason by adopting what I call the “Title Principle”:

Where reason is lively and mixes itself with some propensity, it ought to be assented to. Where it does not, it can never have any title to operate upon us. (T 270)

In the battle among the gentlemanly, wise, and Pyrrhonian Humes that Fogelin describes, I do indeed think that the wise Hume emerges victorious and is meant to emerge victorious—although at some cost, and subject perhaps to periodic, predictable, and unsuccessful temporary Pyrrhonian insurrections. Fogelin concedes—an important concession—that “the wise Hume is the dominant Hume in Hume’s philosophical writings.” He nevertheless alleges that in portraying a victory for the wise Hume over his competitors, I represent only “the state of play at the end of Part iii of Book I of the *Treatise*” and ignore the import of Part iv, where Hume “ends up” in “a morass of skeptical doubt.” Because I agree that Part iv of Book I details a severe skeptical crisis for Hume’s philosophy, I devoted the final chapter of *Cognition and Commitment* to an outline and discussion of that crisis and of the five skeptical arguments that provoke it. I also agree that it is essential to understand how Book I of the *Treatise* ends. But as I see it, Hume does not *end* Book I in a morass of skeptical doubt. Rather, he spends the first half of the final section of Book I (“Conclusion of this book”) getting into such a morass¹² and the second half of it getting *out*. His ultimate escape is provided by his reflective endorsement of the Title Principle already mentioned. The victory provided by the acceptance of this principle is indeed won at some cost. It requires not merely that we accept modes of reasoning that we cannot justify by further reasoning; it also requires that we simply reject the conclusions of some pieces of subtle and refined reasoning in favor of the conclusions of other pieces of reasoning despite the fact that we have no very convincing diagnosis of why the reasonings of the first kind are erroneous. It thus requires us to regard reason as a faculty that sometimes leads us astray even when it operates without external interference and in accordance with its own natural processes.

It would be too long a digression to outline again the entire process leading up to Hume’s adoption of the Title Principle. But here is an analogy that I find helpful. Descartes begins his *Meditations* with the realization that his senses contradict themselves. That is, they provide contradictory judgments about the very same objects, depending on how the senses themselves are deployed. For this reason (among others), Descartes sets out to reassess his senses, employing his whole cognitive nature. Despite the

contradictions to which the senses are sometimes prone, however, his investigations do not lead him simply to reject them. He concludes instead that their information is acceptable when they are governed and regulated with the assistance of another faculty—namely, reason. He thus finds that he can and must endorse a stable way to accept most, though not all, of the deliverances of his senses. Hume, in “Of scepticism with regard to reason,” concludes that reason, too, sometimes contradicts itself. That is, it provides contradictory assessments of the probability of the same beliefs depending on how reason itself is deployed. Accordingly, he proceeds to re-evaluate reason, employing the resources of his whole human nature. Despite the contradictions to which reason is prone, Hume’s investigations do not lead him simply to reject reason. He concludes that reason’s results are acceptable when they are governed and regulated with the assistance of another faculty—namely, passion or inclination. He thus finds that he can and must endorse a stable way to accept most, although not quite all, of the deliverances of reason.

What is my evidence for this interpretation of Hume’s philosophy as a victory of the wise Hume? Perhaps the single most important piece of evidence is the actual course of the second half of *Treatise* I iv 7, where the Title Principle is explicitly endorsed and adopted, never to be abandoned. Another piece of evidence is the Introduction to the *Treatise*, which I read as an unequivocal claim to a victory of Wisdom by an author who has completed his book and knows what is in it. A third important piece of evidence lies in the fact that, even when Hume speaks in the Pyrrhonian voice that Fogelin describes—that is, even when he is in the throes of the skeptical arguments that precede the adoption of the Title Principle—Hume’s language carefully avoids *endorsing* Pyrrhonian conclusions. Instead, he rigorously adheres to a policy of merely *expressing* Pyrrhonian doubts in the first person, *describing them as* his current feelings about a particular topic. A final piece of evidence is that, following the endorsement of the Title Principle, Hume himself never writes in the Pyrrhonian voice again—not in the remainder of the *Treatise* and not in the rest of his writings. He does not write in that voice even in the first *Enquiry*, where Pyrrhonian scepticism is *discussed* and *criticized* but is never *expressed* in the first person. There, as in the *Treatise*, endorsement goes instead to a mitigated or academic brand of skepticism.

Fogelin gives an argument intended to show that the positions of the three Humes *cannot* be resolved. First, he writes that Hume “seems to adopt a principle of the following kind”:

What a person believes and the degree to which he believes it is a function of the light in which he surveys the subject at that particular time.

I agree that Hume endorses such a principle: It is simply a version of epistemic determinism, which is entirely in keeping with his general determinism. But Fogelin goes on to claim that Hume also endorses the following principle of “radical perspectivism”:

When we survey something in a particular light, we will think it *fitting and proper* to assign the degree of belief to it that we do.

I deny that Hume endorses any such principle as this. There is, to be sure, a natural resistance that one feels, while holding any occurrent belief that *p*, to the contrary suggestion that not-*p*. But it is one thing to have a natural resistance to the denial of one’s judgments, and quite another to form a second-order judgment about the normative status (i.e., the fittingness and propriety) of a first-order judgment and its origins. The formation of such second-order judgments is surely a much rarer and more philosophical achievement. Whereas the first-order belief results only from the exercise of one’s cognitive faculties on some evidence about a given proposition, the second-order belief is not fully fixed until one has exercised one’s faculties upon evidence about one’s faculties. One can find oneself holding a belief with a certain current degree of strength while still investigating whether that degree of belief is, in the final analysis, a fitting and proper degree of belief to assign it. Indeed, the dialectical starting-point of Descartes’s *Meditations* is the confrontation between (i) his natural assent to each of his first-order beliefs and (ii) a second-order belief that, because of their origin, many of them are or are likely to be false. Such a state is no more paradoxical than having a first-order desire while also having conflicting second-order desires about whether that first-order desire should be motivationally effective. (Of course, just as conflicts among levels of desire often tend to work themselves out, so too do conflicts among levels of belief.)

Fogelin writes that Hume takes up the gentlemanly, wise, and Pyrrhonian standpoints each “with complete commitment to the standpoint he finds himself in.” That is just what I am here denying. Hume’s own expressions of Pyrrhonian scepticism carefully *refrain* from expressing any second-order judgments about the ultimate correctness of the degree of doubt that they express, as close attention to the text will reveal. The Title Principle, in contrast, is precisely a higher-order generalization telling us what lower-order judgments of reasoning *should and should not* be accepted—i.e., what lower-order judgments it would be *fitting and proper* to accept. The Title Principle is Hume’s master principle of acceptable belief, and it is a principle that, once endorsed, he never relinquishes. In his own view, it is what ultimately allows him—despite his intimate familiarity with “the weakness and narrow limits of human reason and capacity”—to be such a wise guy.

NOTES

1 On this approach, there may be something like a conceptual absurdity not just in trying to establish theses about impressions but in trying to *do anything*—for there will be no reason to suppose that one will correctly retain the task one proposes to undertake for even one second after undertaking it.

I refer to the following works of Hume throughout this paper: David Hume, *A Treatise of Human Nature*, edited by L. A. Selby-Bigge, 2nd ed. revised by P. H. Nidditch (Oxford: Clarendon Press, 1978), hereafter referred to as “T” with references inserted parenthetically in the text; and David Hume, *Enquiries Concerning Human Understanding and Concerning the Principles of Morals*, edited by L. A. Selby-Bigge, 3rd ed. revised by P. H. Nidditch (Oxford: Clarendon Press, 1975). References to the first *Enquiry* abbreviated as “EHU” with references inserted parenthetically in the text.

2 Hence, if the term ‘idea’ is understood to cover all concepts used in cognition, then I too reject the Copy Principle and hence also the ultimate adequacy of Hume’s argument for it. If “ideas” are restricted to images, on the other hand, then I think that the Copy Principle is at least a plausible generalization, although it may have some exceptions (and perhaps more than Hume himself acknowledged). This is another way of saying that I think his empirical evidence for the Causal Thesis (understood as a claim about imagistic ideas) is stronger than his empirical evidence for the Resemblance Thesis, although I do insist—contrary to many of his critics—that he raised some serious and relevant considerations on behalf of both.

3 More precisely, he says that we always use *at least* one: early on in the acquisition of a general term, he thinks, we are likely to “spread out” several ideas before the mind to assure ourselves of its meaning.

4 The claim that *Treatise* I iv 1 excludes demonstration from the scope of reason is surely a mistake; although that section of the *Treatise* argues that the assurance resulting from demonstration can be *replaced* with a weaker degree of assurance via the mechanism of “probability of causes,” this has no tendency to show that demonstration is not a variety of reasoning. Indeed, Hume expresses his conclusion as one concerning what *reason* does to *itself* in such cases.

5 An example of such an announced change is Hume’s elaborate introduction of his second, more restricted sense of ‘probability’, which he opposes to ‘proof’ (T 124).

6 Those present at the original symposium in Monterey may recall that Millican there held that the term ‘reason’ designates for Locke a kind of perception that is “not essentially inferential” and hence includes the kind of non-inferential intuition that Locke often opposes to reason (e.g., IV iii 2 and IV xvii 2 in John Locke, *An Essay concerning Human Understanding*, edited by P. H. Nidditch [Oxford: Clarendon, 1975]; hereafter referred to as “ECHU” with references inserted parenthetically in the text). Millican has omitted this

claim from his remarks as revised for publication, and hence I have deleted some material concerning it from the present version of my reply. Millican has also added a good deal of new material, to which I have added some new responses.

7 Millican sees the footnote at T 117n as announcing a new sense of 'reason' that he thinks succeeds the Lockean sense. I see the passage as doing precisely what Hume says it is doing: namely, clarifying his two senses of 'imagination'. The first of these is a broad one, in which imagination is the primary representational faculty (opposed only to memory); the second is a narrower one, in which imagination is opposed to reason, in the sense of 'reason' that Hume has used and will use throughout the *Treatise*. If 'reason' were used ambiguously in the *Treatise*, the second sense of 'imagination' that Hume distinguishes in the footnote would itself be ambiguous.

8 In *Hume's Skepticism in the Treatise of Human Nature* (London: Routledge and Kegan Paul, 1985).

9 Locke's theory of reasoning has been well-explored in recent years by David Owen, particularly in his forthcoming book, *Hume's Reason* (Oxford: Oxford University Press).

10 Millican's citations do not persuade me that 'reason' and 'understanding' are synonyms; since reason is the main element of the understanding, whatever can be ascribed to reason can also be ascribed to the understanding. Hume can appropriately entitle Book I of the *Treatise* "Of the understanding," but it would not be nearly as appropriate to entitle it "Of Reason." Similarly, Locke's entire book is called *An Essay concerning Human Understanding*; one of its chapters is "Of Reason."

11 Fogelin also mentions some specific points of interpretation on the topic of Hume on miracles. The primary point at issue between us is whether Hume thinks that the existence of a full "proof" against a miracle *by itself* guarantees the impossibility of there being a balance of evidence in favor of the miracle, or whether Hume's phrase (in the conclusion of Section X, part ii, EHU 115) "unless the testimony be of such a kind, that its falsehood would be more miraculous, than the fact that it endeavors to establish" plays a substantive role by describing a conceivable (but non-actual) alternative. I think that the existence of a full "proof" against a miracle does not by itself guarantee the impossibility of there being a balance of evidence in its favor, for Hume, and that the cited phrase does describe a conceivable alternative. Indeed, I think that Hume's later example of "eight days of darkness" is just such an example. However, I have already laid out my reasons for this interpretation as fully as I know how in my chapter on miracles, so I won't repeat them here.

12 Entering this morass involves, in part, further reflections on previous topics from Parts iii and iv.