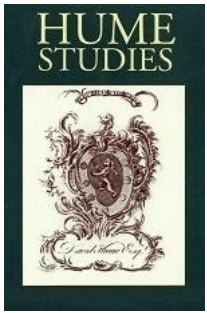

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Is Hume an Inductivist?

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Abstract: De Pierris has argued that Hume is what she calls an inductivist about the proper method of scientific inquiry: science proceeds by formulating inductively-established empirical generalizations that subsume an increasing number of observable phenomena in their scope. De Pierris thus limits Hume's understanding of scientific inquiry, including his own science of human nature, to observable phenomena. By contrast, I argue that Hume's conception of science allows for the positing of, and belief in, unobservable theoretical entities on purely explanatory grounds. I present the details of De Pierris's interpretation of Hume, and the reasons and means for rejecting it. I then consider Hume's explicit statements on his science of human nature to show that all of these are compatible with Hume's accepting a more expansive understanding of scientific explanation. Finally, I briefly consider some examples from the *Treatise* of Hume's employing just such a methodology.

Across a series of papers and again in her recent book, Graciela De Pierris has argued that Hume is what she calls an inductivist about the methods of science. De Pierris takes Hume to follow Newton in holding that the ultimate aim of science is to seek "assurance concerning objects, which are removed from the present testimony of our memory and senses" (EHU 7.29; SBN 76),[1] and its method therefore to consist in the subsumption of observable particulars under inductively-established universal generalizations. As De Pierris puts it, "[t]he central idea of the Newtonian inductive method, as summarized in his Rules, is that exceptionless or nearly exceptionless universal laws are inductively derived from 'manifest qualities' of

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observed 'Phaenomena,' and only further observed phenomena can lead us to revise these laws" (*Ideas, Evidence, and Method*, 150). Notice that according to this characterization, Newton, and by extension Hume, understand science as concerned only, or at the very least primarily, with *manifest* phenomena, or what is observable. What I will argue here is that *contra* De Pierris, Hume's understanding of science comprises not only what is observable, but also what is in-principle unobservable as well. In particular, while De Pierris does allow for "conjecture and hypothesis" insofar as these are required to make the scope of our inductive generalizations as wide as possible, she also holds that these do not amount to proofs until further observations of regular and uniform constant conjunctions are found (*Ideas, Evidence, and Method*, 153–54). That is, while De Pierris's Hume countenances positing unobserved phenomena as a means of widening the scope of our inductive generalizations, such phenomena are accorded only a tentative epistemological status until such time as they can themselves be confirmed via observation. What I will argue is that Hume also allows for a more robust kind of theoretical activity wherein posited entities need not be observed (or observable) but can nonetheless be known to exist on the grounds that they provide an *explanation* of some manifest phenomena in need of explaining.

Mine is a controversial claim to which I must work up slowly. So, I will begin with the details of De Pierris's interpretation. I will argue that while De Pierris successfully shows that, (a) Hume accepts induction as a legitimate form of scientific reasoning, (b) Hume rejects Locke's appeal to *primary qualities* as having any explanatory force, and (c) Hume rejects Locke's commitment to the *a priori* ideal of scientific explanation, this is not sufficient to show that Hume is what she calls an inductivist or that he countenances only observable phenomena. What I will argue is that while De Pierris's (a)–(c) represent important conditions that Hume places on the legitimacy of theoretical posits, their conjunction leaves open the possibility that whether or not a posited entity is in-principle observable, Hume holds that whether we ought to believe in its existence depends only on its explanatory success. Regarding (a), I will show that while De Pierris is correct that Hume endorses the legitimacy of induction, establishing empirical generalizations is not the *end* of science, but rather what first *initiates* its explanatory activities. That is, it is inductively-established empirical generalizations that themselves stand in need of explanation.[2] For example, it is because we infer the lawful correlation of a gas's temperature, pressure, and volume from observation that we find the need to posit the existence of molecules to explain these constant conjunctions. The inductively-established empirical generalization, for example, Boyle's Law, is the beginning, not the end of scientific activity here.[3]

Regarding (b), another condition that Hume puts on legitimate theoretical representations is that they be formed by specifying determinate similarities and differences between that which is posited and some observable phenomenon—

what I call a *perceptible model*. For example, the Bohr model of the atom explains the spectral line emissions of hydrogen by modelling the structure of the atom on that of the solar system (e.g., small particles orbiting a larger central body) while also specifying determinate differences between these (e.g., held together by electrostatic rather than gravitational forces). Importantly for Hume, requiring this sort of model disallows the use of mere *via negativa* intended to explain some observable phenomena, but doing so in name alone. It is *that* condition that underlies Hume's rejection of Locke's appeal to primary qualities, not a rejection of appeals to unobservable theoretical-explanatory entities *per se*.[4]

Finally, with respect to (c), De Pierris's thesis is that Hume rejects Locke's commitment to the *a priori* ideal of science, the notion that the ideal of scientific knowledge would be to apprehend causal powers of individual substances, "in the same way that all the properties of a triangle are contained in the idea of this figure" (*Ideas, Evidence, and Method*, 12–13). Again, though, while De Pierris is right that Hume rejects this Lockean conception of ideal explanation, there is no reason why that rejection must be paired with the severe limitation of the scope of scientific knowledge or "proof" to manifest phenomena on which De Pierris insists. Having this sort of demonstrable intuition of the essence of objects would be *one* way to come by knowledge of their nature, but I will argue that in rejecting this ideal, Hume endorses a different means to the same end. We posit that the objects of scientific inquiry have a certain nature as a means of *explaining* certain manifest phenomena, and the explanatory success of such posits justifies our taking those objects have that nature. Since the picture of scientific knowledge just sketched follows from a combination of *rejecting* the inductivist interpretation that De Pierris offers even while accepting (a)–(c), I conclude that De Pierris's evidence for (a)–(c) is insufficient to demonstrate that Hume accepts anything like her inductivist conception of scientific knowledge.

In the first section below, I will present the details of De Pierris's interpretation of Hume, and the reasons and means for rejecting it.[5] In the second section, I will consider a litany of Hume's most explicit statements on the methodology of his science of human nature to show that all of these are at least compatible with the account of scientific explanation that I outlined above. In the third and final section, I will briefly consider some examples from the *Treatise* of what looks like Hume employing just such a methodology.[6]

Two notes before commencing with that work. First, in what follows I will be concerned with Hume's understanding of only what he calls the science of man or the science of human nature. As Boehm argues, Hume conceives of all of the sciences (including mathematics, physics, morals, politics, religion, and so on) as depending on the science of human nature,[7] and as Hazony argues, this dependence amounts to an explanatory reduction of the phenomena observed and explained by these sciences to explanations comprising only the entities and laws

discovered by the science of human nature.[8] Thus, while I will argue that Hume is a scientific realist about the entities posited by the science of human nature, I will remain neutral regarding his understanding of the other sciences.[9]

Secondly, in contrast to De Pierris's dubbing Hume an inductivist, I will refer to my own interpretation using the term "scientific realism." By "scientific realism" I will mean the conjunction of three theses. The first thesis is that explanatory science can make legitimate use of theoretical representations of unobservable entities. The second thesis is that the explanatory success of a theoretical posit is sufficient for warranting commitment to the existence of that object; importantly, it need not also be observable. Finally, when successful, it is explanatory science, as opposed to "common sense" or "direct experience," that provides a true representation of the world.[10] Thus, my thesis is that, at least for all that De Pierris has argued, it remains possible to interpret Hume as holding that his science of human nature employs theoretical-explanatory posits of unobservable entities that represent the true nature underlying certain manifest phenomena. With those points duly noted, I now begin with the details of De Pierris's arguments.

De Pierris's Newtonian Inductivist Hume

Again, De Pierris argues that Hume allies himself with Newton, against Locke, in endorsing a blanket prohibition on the kind of robust theoretical-explanatory posit that I believe that Hume not only permits, but regularly employs. De Pierris takes Hume to follow Newton in believing that the end of scientific inquiry is the subsumption of observed particulars under inductively derived empirical generalizations: "Hume's notion of inductive proof, which is at the heart of his conception of causation and scientific methodology, consists in a universalization . . . of our past and present uniform experience, with the attendant assumption that nature is, in Newton's words, 'ever consonant with itself'" (De Pierris, "Hume and Locke," 279). De Pierris proposes that we should understand Hume's account of scientific methodology as consisting entirely in this notion of inductive proof (which, she argues, Hume takes over from Newton). That is, she argues that for Hume scientific practice consists in "universalization": subsuming particulars under "exceptionless universal" empirical generalizations. Note that De Pierris deliberately limits the scope of such generalizations to "our past and present uniform experience," or as we saw earlier, to certain, "'manifest qualities' of observed 'Phaenomena'" (*Ideas, Evidence, and Method*, 150).[11] This contrasts with my own proposal for understanding how Hume conceives of scientific practice in several respects, but most straightforwardly insofar as this restriction in scope implies that science is a purely descriptive enterprise that leaves no place for theoretical activity other than in anticipating what is not currently but must eventually become observable. That is, it implies that the ultimate legitimacy of belief in the objects

of theoretical-explanatory posits, or "hypotheses and conjectures," depends on their being observed, and that an appeal to their explanatory success can at most accrue to them a tentatively positive epistemic status. De Pierris rejects such a conception of theoretical-explanatory posits as a live possibility for Newton and Hume by way of Rules III and IV from Newton's *Principia*, which she understands as aimed not only at rationalist advocates of the mechanical philosophy, but also Locke's version of that theory.

> Nonetheless, for both the rationalist mechanical philosophers and for Locke, the ultimate causal explanations of what we observe reside in precisely this hypothetical hidden microstructure. By contrast, Newton, as we have seen, is especially concerned that the favored hypothetical causal explanations of the mechanical philosophy do not interfere with his use of the inductive method. (De Pierris, "Hume and Locke," 286)[12]

In fact, this passage undersells the strength of De Pierris's view. It is not just that Newton demands that theoretical-explanatory posits be *compatible* with the results of the inductive method, but also that such empirical generalizations are the only kind of explanation that can accrue genuine evidentiary force. "Generalizations grounded by this method have the "highest evidence that a proposition can have in this [experimental] philosophy," with which no corpuscularean hypothesis or conjecture can possibly compete" (De Pierris, "Hume and Locke," 292).

Of course, it is precisely this conception of evidence that leads to De Pierris's impoverished conception of the theoretical activity of science and eventually radical skepticism. If the evidentiary force of a scientific theory is exhausted by its subsuming the observed phenomena under empirical generalizations, then it is easy enough to be skeptical of such hypotheses on the grounds that they are mere speculations, the justification for which extends only as far as the observed phenomena. That which extends beyond what is directly observed, relying as it does merely on the principle of the uniformity of nature, remains unsupported. Thus we find De Pierris also arguing for an interpretation of Hume as a radical skeptic:

> Hume's version of the traditional theory of ideas thereby leads to a radical skeptical outcome: a quasi-perceptual form of inspection of items directly present before the mind is, from this peculiarly philosophical standpoint, the ultimate standard of justification of both our a priori and our a posteriori methods; beliefs that go beyond what is directly inspectable before the mind are then subject to skeptical attack. ("Causation," 506)[13]

Because introspection provides the ultimate justification for all of our reasoning, any theory that moves *beyond* introspection is thereby subject to skeptical

doubt, whatever its explanatory force may be. As we will see, though, while there may be a great many subjects with regard to which Hume is a kind of skeptic, the theoretical-explanatory hypotheses of the science of human nature is definitely *not* among these.[14]

The question, then, is what leads De Pierris astray here. To see the answer to this question, we can begin by noting that De Pierris presents five aspects of Locke's conception of explanation that Newton rejects:

1. "Locke is an advocate of the mechanical philosophy."
2. Locke holds, "that any proper causal explanation of the operations and qualities we observe in bodies reduces to a hidden configuration of the primary qualities of their 'insensible Parts.'"
3. Locke understands this microstructure by way of his account of primary qualities.
4. "Locke retains the a priori ideal of knowledge of nature."
5. "Locke does not anticipate an experimental method leading to the formulation of inductively established, exceptionless universal laws" (De Pierris, "Hume and Locke," 285–90).

De Pierris goes on to argue that Hume takes over Newton's rejection of Locke in its entirety. Her argument has two main parts: showing that Hume rejects Locke's account of primary qualities, and that Hume accepts the principle of the uniformity of nature (and is thus free to accept Newton's claims to have at least provisionally discovered certain exceptionless empirical generalizations). Establishing those two theses, however, eliminates only 3 and 5 above. Granting that Hume also rejects 4—the *a priori* ideal of knowledge of nature—and putting aside the details of Hume's rejection of the mechanical philosophy, nothing in De Pierris's argument addresses 2, the picture of scientific methodology as proceeding via appeals to the hidden microstructure of the insensible parts of observable phenomena. That is, while De Pierris is certainly right that Hume rejects Locke's notion of primary qualities, and that he accepts the legitimacy the practice of induction, establishing those two theses is not sufficient to establish further that Hume holds the impoverished conception of scientific practice that De Pierris attributes to him by way of Newton. That is, one can accept both of these theses and still hold that Hume agrees with Locke, "that any proper causal explanation of the operations and qualities we observe . . . reduces to a hidden configuration of . . . their insensible Parts" (De Pierris, "Hume and Locke," 286), so long as these insensible parts are not construed as Lockean primary qualities. For example, I take Hume to reject Locke's conception of primary qualities for the reasons that he presents in *Treatise* 1.1.7 and 1.4.4 (SBN 18–20 and 227–31),[15] but doing so does not commit him to the rejection of theoretical-explanatory posits *simpliciter* because he is free to hold that

these must always be represented via what I have been calling a *perceptible model*. To deploy a perceptible model is to form a theoretical-explanatory representation by specifying the determinate ways that the posited object resembles and differs from a model manifest phenomenon. The admittedly-hackneyed example I gave earlier was the Bohr model of the atom, which attempts to explain the spectral line emissions of hydrogen by modelling the structure of the atom on that of the solar system by specifying both determinate similarities between these (e.g., small particles orbiting a larger central body) and also differences between them (e.g., held together by electrostatic rather than gravitational forces).[16] This is precisely the condition that Locke's account violates in positing substance as something that is *in no way like* any observed phenomena. In the following sections I will present evidence that Hume in fact holds such a view, but the point here is only that, whatever the force of that evidence, for all that De Pierris has argued, Hume *could* still hold such a view.

Furthermore, Hume's acceptance of certain inductively-discovered empirical generalizations alone does not commit him to an inductivist view of scientific practice (according to which it ultimately consists *entirely* of discovering such generalizations). Notice, for example, that on the kind of account that I presented earlier, the *explicandum* of a scientific theory is exactly that the observed particulars obey the empirical generalizations that they do, a phenomenon that would be impossible to encounter were induction not legitimate. What De Pierris's Newton rejects is the claim that *a priori* reasoning can legitimately be employed in justifying the use of a theoretical-explanatory posit. Hume can accept that claim without having to accept the further claim that the only way to justify such a posit is by eventually observing it. For example, one can reject the *a priori* method for formulating such hypotheses, and also hold that it is a theoretical posit's success in explaining some otherwise surprising or puzzling manifest phenomenon that justifies our believing in its existence. For example, one might justify a belief in the particulate theory of gases on the basis of its success in explaining the Ideal Gas Law (which is itself an inductively-established universal generalization).

Finally, De Pierris presents texts that she takes to show that Hume endorses Newton's rejection of theoretical-explanatory posits in their entirety, for example, the following passage from the *Enquiry*:

> It was never the meaning of Sir ISAAC NEWTON to rob second causes of all force or energy; though some of his followers have endeavoured to establish that theory upon his authority. On the contrary, that great philosopher had recourse to an etherial active fluid to explain his universal attraction; though he was so cautious and modest as to allow, that it was a mere hypothesis, not to be insisted on, without more experiments. (EHU 7.25n16; SBN 73n1)

As De Pierris notes, Hume is here referring to Newton's attempt in Query 21 of the *Opticks* to explain the law of universal gravitation by appealing to the pressure exerted by the differing densities of the Aether produced by massive bodies. De Pierris takes Hume's description of Newton's conjecture as "mere hypothesis" to indicate Hume's agreement with Newton that while such explanatory posits might be tried, their ultimate justification can come only by way of direct observation. Notice, though, that Hume does not claim that the existence of ether needs to be *directly observed* to be proved, but only that "more experiments" are necessary to establish it. Such experiments might yield a direct observation of the ether, but that is not the only kind of experimental evidence that can speak in favor of it. For example, one way that a theoretical posit might be proved is by collecting further instances of otherwise surprising phenomena that the posit can explain and that other competing hypotheses cannot. For example, its ability to successfully explain a wide variety of experimental results that no other theory can is the most significant piece of evidence in favor of the so-called Standard Model of contemporary particle physics, despite the fact that the objects that it posits cannot be directly observed. Merely assigning the ether theory a tentative status does not commit Hume to assigning that same status to all theoretical-explanatory posits *per se*.

Notice also, that Hume refers to Newton's conjecture as an attempt "to *explain* his universal attraction." The law of universal gravitation is an empirical generalization that holds of all observed particulars (massive bodies). If the aim of science were merely to subsume manifest phenomena under universal generalizations, then there would be no demand to *explain* such discoveries: scientific inquiry would end with their formulation, and no such explanation would be necessary.[17] Hume acknowledges, however, that the law of universal gravitation does call for an explanation in terms of the nature of the underlying substances at hand. Newton's conjecture does just this: it explains the inductively derived generalizations by positing an unobserved fluid that resembles and differs from the observed phenomenon on which it is modelled in determinate ways. Thus insofar as Hume approves of the *form* of Newton's conjecture, if not it's then current evidentiary force, it is because he is a scientific realist, not an inductivist.

If Hume does approve of the form of Newton's conjecture, though, what then of his idiom of "mere hypothesis"? The first thing to note about that phrase is that it is not one that Hume explicitly endorses, but rather it occurs in his description of how Newton himself understood his conjecture. The second thing to note about it is that Hume takes Newton to have understood his conjecture in this way because "he was so cautious and modest," and as Schliesser has noted, it is not at all clear that Hume's descriptions of Newton as cautious and modest are meant to be flattering.[18] Hume employs the same idiom in his description of Newton in *The History of England* to deliver a series of subtle backhanded compliments:

> In Newton this island may boast of having produced the greatest and rarest genius that ever arose for the ornament and instruction of the species. From modesty, ignorant of his superiority above the rest of mankind; and thence, less careful to accommodate his reasonings to common apprehensions: More anxious to merit than to acquire fame: He was, from these causes, long unknown to the world; but his reputation at last broke out with a lustre, which scarcely any writer, during his own life-time, had ever before attained. (*History* 6:542)

While Hume is generally laudatory here, he does also cite Newton's modesty as the source his misconstruing, and more specifically his *underestimating*, the credit that is due to him (and of his failure to properly disseminate his work). It is easy enough to think of Hume as holding a similar position with respect to Newton's modesty regarding the explanatory force of his account of gravity, that is, of taking Newton's epistemic modesty to result in his misconstruing the nature of his theory and underestimating its explanatory force.[19]

Thus, while De Pierris makes a compelling case that Hume follows Newton in rejecting certain of Locke's theses regarding scientific methodology, I find that the evidence that she presents that Hume also adopts what she takes to be Newton's inductivism insufficient. As that is a deliberately impoverished understanding of scientific activity that has radically skeptical consequences, we should require a great deal of evidence before attributing it to Hume, especially since, as we are about to see, Hume appears to rely on a very different understanding of science in his own pursuit of the science of human nature, and since his most explicit pronouncements of the subject support this practice. Hume agrees with Newton on the importance of empirical generalizations for the explanatory work of science, but also allows for the deployment of genuinely *explanatory* hypotheses, which not only describe both the observed and as of yet unobserved phenomena, but also explain these by appeal to their underlying, sometimes unobservable, nature. Where Hume differs from Locke is in demanding that such theoretical posits be made intelligible via being modelled on some more familiar observable phenomenon. Thus, De Pierris is correct to cite, "the postulation of a hidden microstructure of primary qualities or properties of bodies," as something that Hume finds objectionable in Locke, but wrong to think that what is objectionable here is the *postulation* of something hidden rather than the fact that the idea of *what* Locke takes to be hidden, "primary qualities," has no content.

Thus, I conclude that the case for reading Hume as an inductivist about scientific methodology is insufficient, and that given the philosophic implausibility of that account, we ought to seek to understand Hume as having a more plausible approach to his own scientific methodology. Thus, in the next section, I will examine Hume's most explicit statements about the science of human nature to show

that these are all compatible with a rejection of inductivism and the adoption of a more plausible account of theoretical activity.

The Scope of Theoretical Activity

Before I begin, I want to repeat a note that I made at the close of the introduction: I will here limit my concern to Hume's statements about *the science of human nature* for a few reasons. Firstly, since Hume holds that all of the other sciences themselves depend on the science of human nature (T Intro. 4; SBN xv), one might well think that insofar as any of those sciences have genuine explanations to offer at all, those will be provisional on the explanations offered by Hume's own science.[20] Secondly, Hume is explicit that he takes his pursuit of the science of human nature to be *successful* (T 1.4.7.14; SBN 272–73), and the same cannot be said of his opinion of the other sciences. So, if what we are after is an account of how scientific explanation *ought* to proceed, the safest ground on which to stand will be Hume's reflections on his own practice. Notice that this restriction makes the realism that I will articulate importantly different from that defended by the so-called New Humeans. New Humeans understand Hume as countenancing the real but hidden causal powers of the material world, typically via the deployment of "relative ideas" that refer to the causes of our perceptions without providing any description of those causes.[21] Not only will I limit my treatment to Hume's science of *the mind*, but my understanding of Hume as ruling out the possibility of explanatory hypotheses that are not modelled on observable phenomena is incompatible with this interpretation of relative ideas.[22]

Hume is perhaps most explicit about his vision of the science of human nature in the Introduction to the *Treatise*, and while scholars seem to find support there for reading Hume as engaging in a purely descriptive project,[23] a closer consideration reveals that Hume's statements there all include important overlooked caveats that temper his apparent contempt for theoretical-explanatory activity. What I will do here, then, is walk through the passages in the Introduction where Hume's attention is focused on describing the methodology and anticipated results of the science of human nature (roughly Intro. 7–10; SBN xvi–xix), paying careful attention to the ontological commitments that Hume does and does not undertake and reject. We can begin with Hume's comparison of the epistemological status of science of human nature to that of the science of external bodies.

> For to me it seems evident, that the essence of the human mind being equally unknown to us with that of external bodies, it must be equally impossible to form any notion of its powers and qualities *otherwise than from careful and exact experiments, and the observation of those particular*

> *effects, which result from its different circumstances*. (T Intro. 8; SBN xvii, emphasis added).

Far from claiming that we cannot know the "powers and qualities" or the human mind, Hume is in fact presenting the precise methods that the science of human nature will use to discover these. It is through "careful and exact experiments" and "the observation of those particular effects, which result from its different circumstances" that we *do* come to know "the essence of the human mind." Of course, Hume does start out this sentence by declaring that this essence is unknown to us (or, more precisely, as unknown as that of external bodies), but he is there making a claim about our state of knowledge *before* we make such experiments and observations. There is no other way to make sense of his methodological recommendation, the end of which is to remedy this ignorance. Thus, this passage is entirely compatible with a view of science as adopting "realist" aims.[24]

Additionally, notice that what this passage endorses—experimentation and observation of the behavior of the objects of study in various circumstances—are precisely what provide the foundation for our theoretical-explanatory activities according to the scientific realist. What is discovered by such activities, when properly conducted, is the essence, the real powers and qualities, of the object studied. Again, far from being a condemnation of theoretical speculation, this passage is a guide to its proper implementation and an endorsement of its realist aims. This should make us wonder, though, to whom Hume takes himself to be aiming this recommendation, and the answer to that question is easy enough to find, given that Hume credits the pioneers of this method as "some late philosophers in *England*" (T Intro. 7; SBN xvii), and specifically cites Locke, Shaftesbury, Mandeville, Hutchenson, and Butler. That list certainly includes philosophers with a much more robustly realist approach to scientific explanation than Hume has been taken to have, and contrasts most directly with their Continental "rationalist" counterparts. That is, when Hume recommends experimentation and observation, he is not warning against scientific realism, but only against entirely non-empirical (*a priori*) methods.

Now consider the passage that immediately follows this one.

> And tho' 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, 'tis still certain we cannot go beyond experience; and any hypothesis, that pretends to discover the ultimate original qualities of human nature, ought at first to be rejected as presumptuous and chimerical. (T Intro.8; SBN xvii)

The opening of this passage does appear to confirm the view of Hume as an inductivist: "we must render our principles as *universal* as possible" resonates with

De Pierris's interpretation according to which scientific methodology consists in subsuming particulars under universal empirical generalizations, and "'tis certain that we cannot go beyond experience," sounds like her claim that what is most real are observed phenomena, not the theoretical posits that explain these. Of course, the first clause is offset by what follows it—"explaining all effects from the simplest and fewest causes"—which makes explicit appeal to specifically *causal* explanations, which sits less well with the inductivist picture. And the second clause is followed by what appears to be an explication of the proscription against going beyond experience, which limits its scope to "any hypothesis that pretends to discover the ultimate original qualities of human nature." To understand this more limited version of the prohibition, we must first properly understand what Hume means by that phrase. What Hume is proscribing is the attempt to move beyond what the science of human nature must take as explanatorily basic. That is, in explaining the features of the human mind, the scientist of human nature will posit certain entities, faculties, and principles. To have the representational content required to fulfill their explanatory function, these posits must be modelled on directly observable phenomena, which for Hume must be perceptions themselves. One might inquire into the explanation of such entities or principles, and if there is a further perceptible model available to answer that inquiry, then a legitimate explanation might be given. At some point, however, such resources are exhausted and no further legitimate explanation is available. Hume believes that at that point philosophers are all too often tempted to make use of *illegitimate* explanatory hypotheses—for example, ones that do not employ perceptible models, and so are devoid of any real content—and this is what he is here recommending against: "any hypothesis, that pretends to discover the ultimate original qualities of human nature." This forbearance is not against explanatory hypotheses *in general*, but only against those that pretend to discover ultimate original principles where only provisional ones are legitimately possible.

Hume reprimands the philosopher that pursues such explanations in the following paragraph: "I do not think a philosopher, who wou'd apply himself so earnestly to the explaining the ultimate principles of the soul wou'd show himself a great master in that very science of human nature, which he pretends to explain, or very knowing in what is naturally satisfactory to the mind of man" (T Intro. 9; SBN xvii–xviii). It is not the philosopher that makes an initial appeal to explanatory principles that is Hume's target here, but rather the philosopher that aims to explain these principles themselves, which being ultimate can receive no such explanation. What such a philosopher misunderstands is that the ceaseless pursuit of further and further explanations is not what is satisfactory to the mind, but rather that it is the termination of such explanations in something explanatorily basic that is so. Hume's point here is not that we must not move beyond a mere descriptive phenomenology, but only that explanations must come to an end

where our ability to ground those explanations in experience does, and that we must resist the temptation to pretend to move beyond this point with "presumptuous" and "chimerical" hypotheses.

This entire dialectic of reaching explanatory rock bottom, being tempted to appeal to unfounded first principles, and the failure of philosophers who give in to this temptation is repeated numerous times throughout Book 1. Here is an example from T 1.1.4 in which Hume uses the same idiom of "original qualities of human nature":

> Here is a kind of ATTRACTION, which in the mental world will be found to have as extraordinary effects as in the natural, and to show itself in as many and as various forms. Its effects are every where conspicuous; but as to its causes, they are mostly unknown, and must be resolv'd into *original* qualities of human nature, which I pretend not to explain. Nothing is more requisite for a true philosopher, than to restrain the intemperate desire of searching into causes, and having establish'd any doctrine upon a sufficient number of experiments, rest contented with that, when he sees a farther examination wou'd lead him into obscure and uncertain speculations. (T 1.1.4.6; SBN 12–13)

Notice that here Hume's recommendation to the philosopher "to restrain the intemperate desire of searching into causes" is limited in its scope to "when he sees a farther examination wou'd lead him into obscure and uncertain speculations." That is, there is nothing *per se* illegitimate about searching into causes, but since the desire to do so is intemperate, one must be careful to recognize when its legitimate fulfillment has been completed. Here, for example, Hume recognizes that an attempt to explain the associations of ideas would go beyond the resources that are available to him, and so must rest content with taking these to be "*original* qualities of human nature," i.e., with taking them to be *explanatorily basic*, i.e., he must "pretend not to explain" them. What this series of claims suggests, though, is that the form that such an explanation *would* take, where legitimate, would be in appealing to the unobserved causes of these associations.

With this more modest interpretation in mind, we can now proceed to the next passage from the Introduction.

> *When we see, that we have arriv'd at the utmost extent of human reason*, we sit down contented; tho' we be perfectly satisfy'd in the main of our ignorance, and perceive that we can give no reason for our most general and most refin'd principles, beside our experience of their reality; which is the reason of the mere vulgar, and what it requir'd no study at first to have discover'd for the most particular and most extraordinary phænomenon.

> And as *this impossibility of making any farther progress* is enough to satisfy the reader, so the writer may derive a more delicate satisfaction from the free confession of ignorance, and from his prudence in avoiding that error, into which so many have fallen, of imposing their conjectures and hypotheses on the world for the most certain principles. (T Intro.9; SBN xviii, emphases added)

Again, while at first glance it might appear troubling to the scientific realist that Hume claims that, "we can give no reason for our most general and most refin'd principles, beside our experience of their reality," it is clear enough from the context that this is not a general forbearance on theoretical-explanatory activity, but rather that that proscription is limited in its scope specifically to those who fail to recognize the "impossibility of making any farther progress" past the point at which "we have arriv'd at the utmost extent of human reason." The first sentence here makes clear that the scientist of human nature is not only allowed to employ "general and most refin'd principles" (explanatory hypotheses), but also to reach these via the employment of not just experience, but also *reason*.[25] So, Hume's claim that we experience the reality of these principles should not be understood as a claim that the only legitimate explanations are the generalizations of descriptive phenomenology. Rather, it is that we reach the limit of our theoretical activity when we can no longer appeal to experience as its *ground*. That is, when our explanations are no longer held accountable to experience for their evidentiary support or are no longer represented via what I have been calling a perceptible model.[26] Once again, there is nothing here to which a scientific realist must object. Hume's critique is aimed at those who would pretend to explain the ultimate experience-based principles on non-experiential ones.

In fact, Hume returns to the matter of employing refin'd principles in the conclusion of Book 1, where he explicitly considers a general prohibition on these.

> Shall we, then, establish it for a general maxim, that no refin'd or elaborate reasoning is ever to be receiv'd? Consider well the consequences of such a principle. By this means you cut off entirely all science and philosophy: You proceed upon one singular quality of the imagination, and by a parity of reason must embrace all of them: And you expressly contradict yourself; since this maxim must be built on the preceding reasoning, which will be allow'd to be sufficiently refin'd and metaphysical. . . . Very refin'd reflections have little or no influence upon us; and yet we do not, and cannot establish it for a rule, that they ought not to have any influence; which implies a manifest contradiction. (T 1.4.7.7; SBN 268)

Hume concedes that his own procedure in conducting the science of human nature, "will be allow'd to be sufficiently refin'd and metaphysical," and wonders whether this implies that he should on those grounds reject it. His answer: such a forbearance would "cut off entirely all science and philosophy" and ceasing all such activity "implies a manifest contradiction." While philosophy and the science of human nature, both of which employ refin'd reasoning, might well cause melancholy and delirium (T 1.4.7.9; SBN 269), they are also entirely necessary to correcting the beliefs of the vulgar and of the false philosophy and avoiding manifest contradictions. As Hume notes just a few pages later, the science of human nature, for all of its refin'd principles, is itself the very pinnacle of human knowledge, and we have every reason to believe that it will ultimately triumph, because of its explanatory power, over its competitors: "But were these [specious] hypotheses once remov'd, we might hope to establish a system or set of opinions, which if not true (for that, perhaps, is too much to be hop'd for) might at least be satisfactory to the human mind, and might stand the test of the most critical examination" (1.4.7.14; SBN 272–73). Hume rejects wholeheartedly a prohibition on refined reasoning, and explicitly endorses the view that such reasoning is the only way that we will stand any chance of reaching a true and explanatory scientific theory.

All of which brings us to one last passage from the Introduction that again appears to cast Hume as an inductivist, but which is actually compatible with scientific realism: "None of them [all arts and sciences] can go beyond experience, or establish any principles which are not founded on that authority" (T Intro. 10; SBN xviii–xix). While the first half of this sentence can seem to lend support to the notion that Hume holds that the science of human nature must not include anything but descriptive phenomenology and perhaps a few defeasible empirical generalizations derived from such a description, the second half of the sentence extends the scope of the sciences to include those principles which are *founded on* that authority. Again, that is not something to which the scientific realist would disagree. Both the content of and evidence for a theoretical posit will be derived from the authority of experience in the sense that the theoretical posits of the science of human nature are legitimate just in case they explain some phenomenon using a *perceptible model*.[27] That perceptible model does "go beyond experience" insofar as it will be an analogical extension of what is found in experience itself, but Hume's disjunction here allows for precisely such extensions. Compare this with the following passage from Hume's discussion of the idea of body in T 1.4.2. Notice that Hume here contrasts what is an object of the senses with what is (legitimately) "deriv'd from experience and observation." That contrast, like the preceding one, depends on the scope of theoretical activity outstripping mere descriptive phenomenology: "As to the *independency* of our perceptions on ourselves, this can never be an object of the senses; but any opinion we form concerning it,

must be deriv'd from experience and observation" (T 1.4.2.10; SBN 191). Of course, such passages do not give any details regarding what it is to derive an opinion from experience and observation, or to give an adequate account from analogy, or to establish principles founded on the authority of experience. What they do, however, is indicate a contrast between mere descriptive phenomenology and a different kind of theoretical explanatory activity that at once moves beyond experience, while still bearing some content- and authority-dependent connection to experience. Once again, this is how the scientific realist, not the inductivist or the rationalist, understands the role of theoretical activity.

Now, finally, with those apparently damning passages situated in their proper context, we can turn to a passage from the Introduction that more clearly indicates a greater role for theoretical explanation in Hume's understanding of the methodology of the science of human nature. Here Hume proclaims that the truths at which his pursuit of this science aims cannot be either easy to observe or obvious:

> For if truth be at all within the reach of human capacity, 'tis certain it must lie very deep and abstruse; and to hope we shall arrive at it without pains, while the greatest geniuses have failed, must certainly be esteemed sufficiently vain and presumptuous. I pretend to no such advantage in the philosophy I am going to unfold, and would esteem it a strong presumption against it, were it so very easy and obvious. (T Intro. 3; SBN xiv–xv)

The conclusions that the scientist of human nature will reach are not ones that are "easy or obvious," but "lie very deep and abstruse." While it would certainly be no simple matter to provide a complete and accurate descriptive phenomenology of the human mind, such a study would be the precisely opposite of deep and abstruse: it would intentionally be restricted to what lies on the surface, so to speak, of that phenomenology and to what is least obscure about it. In fact, in the following paragraph Hume explicitly cites the main advantage that accrues to the scientist who begins his studies with human nature as being his or her ability to "*explain the nature* of the ideas we employ" (T Intro. 4; SBN xv). It is not that the scientist of human nature perfectly describes those ideas that makes that science the most fundamental, but rather that he or she is able to *explain* their *nature*.

So it seems that rather than containing vehement denials of the legitimacy of theoretical-explanatory activities and enthusiastic commitment to a purely descriptive phenomenology, Hume's explicit discussions of the science of human nature carefully hedge such pronouncements and leave plenty of room for an alternative interpretation of Hume's understanding of that science. What I would take away from those passages is that,

(a) In pursuing the science of human nature, something will have to be taken as explanatorily basic, and philosophers must resist pretending to explain

these ultimate principles by appealing to that which is not suitably derived from experience.

(b) Theoretical explanatory posits depend for their content and legitimacy on their derivation from experience, but this derivation can move beyond experience via reason and analogy.

These theses are not only compatible with the picture of scientific realism that I presented in the opening section, but in fact follow from it.[28] Having examined some of Hume's general reflections on the methodology of the science of human nature, it will now be instructive to examine a few brief examples of Hume's putting that methodology into practice in order to fill out the picture that we have just been sketching in its most broad strokes.

Representing Theoretical Entities

In the previous section, we saw that far from committing himself to conducting the science of human nature purely via descriptive phenomenology Hume's explicit statements about the methodology of that science are compatible with the use of theoretical-explanatory posits so long as these are properly derived from experience. In this section, I present examples of Hume employing this methodology in the hopes of giving some detail to the picture just sketched.[29] What I aim to show is that there is some evidence that Hume himself relies on the use of theoretical-explanatory entities. On this interpretation, Hume takes these to be derived from experience insofar as they are represented via models that are created by combining the deliverances of the senses with reason via the drawing of inferences to the best explanation.

To that end, I will begin with a telling passage from Hume's discussion of the idea of vacuum. An inductivist reading of Hume makes understanding this passage, as well as the others considered in this section, very difficult to understand, whereas a realist reading handles them with ease. In this first passage, Hume considers an objection to his thesis that we can form no idea of a vacuum that calls for Hume to account for what happens when we imagine a room to be emptied of whatsoever fills it, while the structure of the room remains the same. Here is Hume's description.

> When every thing is annihilated in the chamber, and the walls continue immoveable, the chamber must be conceiv'd much in the same manner as at present, when the air that fills it, is not an object of the senses. This annihilation leaves to the *eye*, that fictitious distance, which is discover'd by the different parts of the organ, that are affected, and by the degrees of light and shade; and to the *feeling*, that which consists in a sensation

> of motion in the hand, or other member of the body. In vain shou'd we search any farther. On which-ever side we turn this subject, we shall find that these are the only impressions such an object can produce after the suppos'd annihilation; and it has already been remark'd, that impressions can give rise to no ideas, but such as resemble them. (T 1.2.5.23; SBN 62–63)

This is a remarkable passage for a number of reasons. Firstly, Hume's argument here simply takes for granted that before engaging in the objector's thought experiment, we represent the room as filled with air that "is not an object of the senses." In itself, that strongly implies that Hume holds that we can represent unobservables, and that understanding how we represent a room as filled with such air should give us some insight into this issue. Secondly, Hume continues by noting that whatever the suggested annihilation might otherwise accomplish, it does not change the *image* that is the representation of the room: the same degrees of light and shade remain, as do the feelings of motion in the hand, and so on. So, again, what is represented by the senses in representing the room as filled with air or not is constant. "In vain shou'd we search any farther." What changes is not what is sensed, but something else, something that is not itself experienced.

Finally notice that despite the fact that neither the air nor the vacuum is experienced, Hume rejects the idea of the latter, but not that of the former, on the grounds that "it has already been remark'd, that impressions can give rise to no ideas, but such as resemble them." This implies that there is some sense in which the idea of air, even though it is an idea of something "that is not an object of the senses," does resemble some impression, and this in turn implies that we can represent that which is not experienced so long as such objects in some way resemble that which is experienced. That is, we can represent unobservable theoretical entities via perceptible models.

Before turning to the task of investigating such models further, there is a complication here concerning *why* the air is not an object of the senses that it will be instructive to address. The complication is due to the fact that one natural answer to this question is that our idea of air is the idea of particles that are too small to be perceived. To take a toy example, we model our idea of air on the idea of billiard balls, and think of particles of air as having the same physical properties, exhibiting the same behaviors, and being governed by the same laws, except that they move much more quickly and are much smaller. The problem with this account of our representation of particles of air is that Hume explicitly and repeatedly denies that we can represent anything as being (or that anything can be) smaller than our *minima sensibilia*. That thesis implies that atoms of air are not imperceptibly small, but that we can in fact form adequate ideas of them merely by observing

them under the proper conditions. So, these particles are not unobserv*able*, but rather only unobserv*ed*.

> 'Tis not for want of rays of light striking on our eyes, that the minute parts of distant bodies convey not any sensible impression; but because they are remov'd beyond that distance, at which their impressions were reduc'd to a *minimum*, and were incapable of any farther diminution. A microscope or telescope, which renders them visible, produces not any new rays of light, but only spreads those, which always flow'd from them; and by that means both gives parts to impressions, which to the naked eye appear simple and uncompounded, and advances to a *minimum*, what was formerly imperceptible. (T 1.2.1.4; SBN 27–28)

While particles of air might not be visible to us simply looking around the room, were we to look at a sample of air through a suitable microscope, we would be able to form adequate simple ideas of them (presuming that these particles were themselves simple). It would seem, then, that we might not use a perceptible model to represent the air at all. It might be that we represent the air using the senses alone: not via what we are *currently* observing, but what we have, will, or could observe in the proper conditions. That is, while the earlier passage appeared to indicate that the air and vacuum were similarly "not objects of the senses," perhaps there is an important difference in the way that this is true of each: the air is not a *current* objects of the senses, whereas the vacuum is not even so much as a *possible* such object.[30] The difference between a representation of a vacuum and a representation of air might reduce to the difference between that which is unobservable and that which is observable, after all.

There is reason, however, to think that this is not the case. While it may well be that particles of air are not unobservable in principle, the representation of such particles does not *typically* take the relatively straightforward form described above of a simple idea representing some simple impression. The more typical mode of representation of such particles, even if these turn out to be observable, deploys exactly those resources needed for a more robustly theoretical-explanatory representation. That is, while we may be *able* to represent a particle of air by perceiving it, what we typically do is something very different (as evidenced by the theoretical apparatus that was necessary to first formulate the particulate theory of gases). Hume himself notices just this in a passage immediately following the one above.

> The only defect of our senses is, that they give us disproportion'd images of things, and represent as minute and uncompounded what is really great and compos'd of a vast number of parts. This mistake we are not sensible of; but taking the impressions of those minute parts, which appear to the

> senses to be equal or nearly equal to the objects, and finding by reason, that there are other objects vastly more minute, we too hastily conclude, that these are inferior to any idea of our imagination or impression of our senses. (T 1.2.1.5; SBN 28)

The error on which Hume focuses here is one that results from our concluding from the discovery that what the senses have represented as simple is in fact complex, that there are objects that are smaller than any *minima sensibilia*. What is of interest for current purposes, though, is how he takes this error to be discovered. It is not that the *senses* come to represent what is simple as simple; we do not, at least at first, come straightforwardly to *see* simple particles of air. Rather, we discover this error via "finding by reason, that there are other objects vastly more minute" (T 1.2.1.5; SBN 28). That is, it is *reason* that plays the crucial role here, at least at first, of representing unobserved particles of air: the senses represent something as simple, but reason discovers it to be complex. We do not, at least typically, represent particles of air via the senses, but rather reason reveals to us that what the senses present cannot be taken at face value, and combines with what the senses have provided to formulate a more adequate explanation that phenomenon.

Garrett describes the role of reason in Hume as follows: "A belief is *produced by reason* if and only if it results from an operation of the inferential faculty" ("A Small Tincture," 80). What the case of the imperceptible air suggests is that we can accept Garrett's description so long as we understand "the inferential faculty" to include inference to the best explanation.[31] This suggestion will be born out in the rest of the examples to be presented. So consider next Hume's gloss on our experience of the world as consisting of three, rather than two, dimensions. "'Tis commonly allow'd by philosophers, that all bodies, which discover themselves to the eye, appear as if painted on a plane surface, and that their different degrees of remoteness from ourselves are discover'd more by reason than by the senses" (T 1.2.5.8; SBN 56). Here Hume notes in passing that it is not the image produced by senses alone that represents a three-dimensional space as three-dimensional, but that *reason* has an ineliminable contribution to make to that representation as well. What reason does in this case is the same as what it does in the case of the representation of unobservable air: it both discovers that what the senses provide requires further explanation, and contributes to forming a perceptible model to create a representation of an entirely new kind of object. What Hume seems to have in mind in the case of the representation of a three-dimensional space is using a representation of a two-dimensional plane as the model to posit another such plane orthogonal to the first that would explain the observed behavior of certain colored points. (E.g., when such-and-such a colored mass moves "farther away," we expect them to grow smaller and smaller until they finally disappear altogether, or that when one colored point "moves behind another," we see the

first approach the second from the side, then disappear, and we then expect it to reappear on its opposite side, and so on.)

As mentioned earlier, while reason has an essential role to play in the construction of such models—it is what allows us to move *beyond* experience—this role is only made possible through the combination of reason with sensation. Whereas it is reason that discovers the need for a theoretical-explanatory posit and the specific form that such a posit will take, it is sensation that provides the model itself, and thus makes that which is posited intelligible. Specifically, as we saw in the passage concerning the imperceptible air, Hume repeatedly stresses that any such model must *resemble*, at least to some degree, that on which it is modelled. Here is another example of this sort of theoretical-explanatory activity in which Hume emphasizes both of these aspects: the role of reason in moving beyond the images provided by sensation, but also the role of sensation as the basis for reason's extrapolation.

> 'Tis universally allow'd by the writers on optics, that the eye at all times sees an equal number of physical points, and that a man on the top of a mountain has no larger an image presented to his senses, than when he is coop'd up in the narrowest court or chamber. 'Tis only by experience that he infers the greatness of the object from some peculiar qualities of the image; and this inference of the judgment he confounds with sensation, as is common on other occasions. (T 1.3.9.11; SBN 112)

Once again, the image that is provided by the senses is by itself inadequate to the representational and explanatory task at hand: representing the vastness of the ocean. Thus one "*infers* the greatness of the object from some peculiar qualities of the image." Of course, when Hume writes here that this inference is "only by experience," he does not mean that *only* experience is involved in drawing that inference, but instead he is here referencing the fact that all of our *probable reasoning*, our inferences from causes to effects and vice versa, are grounded in experience. That is, that the inference that is drawn, the theoretical-explanatory entity that is posited, derives both its representational content and its evidential support from experience (here, e.g., the experience of sailing the ocean). Hume continues by noting that, "a man has a more vivid conception of the vast extent of the ocean from the image he receives by the eye, . . . than merely from hearing the roaring of the waters," and then explains this fact by appeal to, "the resemblance betwixt the image and the object we infer."

> But as the inference is equally certain and immediate in both cases, this superior vivacity of our conception in one case can proceed from nothing but this, that in drawing an inference from the sight, beside the customary

> conjunction, there is also a resemblance betwixt the image and the object we infer, which strengthens the relation, and conveys the vivacity of the impressions to the related idea with an easier and more natural movement. (T 1.3.9.11; SBN 112)

Hume's point here is that the greater the resemblance between the model and the explanatory posit, the more easily and naturally we will accept the existence of the latter. It is plausible to suppose, though, that should this resemblance be reduced to nothing, not only would we no longer believe in the explanatory posit, but we would not even be able to so much as form an idea of it. In fact, we find Hume making this exact claim a few sections later.

> Without some degree of resemblance, as well as union, 'tis impossible there can be any reasoning: But as this resemblance admits of many degrees, the reasoning becomes proportionally more or less firm and certain. An experiment loses its force, when transfer'd to instances, which are not exactly resembling; tho' 'tis evident it may still retain as much as may be the foundation of probability, as long as there is any resemblance remaining. (T 1.3.12.25; SBN 142)

Here, as in the previous passage, we find Hume noting that greater degrees of resemblance correspond to greater firmness and certainty of belief, but he now also completes the picture of reasoning that we had supposed he was employing by noting that, "without some degree of resemblance . . . 'tis impossible that there can be any reasoning." This part of Hume's conception of the combination of reason and sensation in theoretical-explanatory activity is of crucial importance because it is precisely the condition that he so often takes his predecessors to violate. That is, Hume finds such philosophers pretending to make some explanatory hypothesis in which there is *no* resemblance between the model and that which is posited. Consider the series of questions that Hume puts to the philosopher that posits an immaterial soul. "For how can an impression represent a substance, otherwise than by resembling it? And how can an impression resemble a substance, since, according to this philosophy, it is not a substance, and has *none* of the peculiar qualities or characteristics of a substance?" (T 1.4.5.4; SBN 233, emphasis added). Notice that the problem with the idea of an immaterial soul is that it shares *none* of the peculiar qualities or characteristics of a substance, which suggests that if it shared *some* of these qualities, it could be used to do the explanatory work that is its intended purpose. Next, here is Hume writing about probable reasoning in general. "Tho' the mind in its reasonings from causes or effects carries its view beyond those objects, which it sees or remembers, it must never lose sight of them entirely, nor reason merely upon its own ideas, without some mixture of impressions, or at least

of ideas of the memory, which are equivalent to impressions" (T 1.3.4.1; SBN 82). Notice that what Hume requires for a piece of causal reasoning to be legitimate is not that it not move beyond experience *at all*, but only that there be "some *mixture* of impressions" with its own ideas. That is, causal explanatory posits are legitimate just in case there is *some* resemblance between the model and that which is posited. Hume revisits this mixture of impressions with causal reasoning two sections later. "Were there no mixture of any impression in our probable reasonings, the conclusion wou'd be entirely chimerical" (T 1.3.6.6; SBN 89).

As we saw in examining Hume's explicit statements about the scope of theoretical activity in the previous section, a conclusion is not chimerical when it makes an explanatory posit that goes beyond experience at all, but only when it intemperately continues to seek explanation past the point at which no further explanation is possible. Here we see what that point is: when "there is no mixture of any impression" in it. The complement to this thesis is that without reasoning, we could not move beyond experience at all. "And were there no mixture of ideas, the action of the mind, in observing the relation, wou'd, properly speaking, be sensation, not reasoning" (T 1.3.6.6; SBN 89). Finally, Hume combines these two theses to give his most explicit endorsement of theoretical-explanatory posits: "'Tis necessary, that in all probable reasonings there be something present to the mind, either seen or remember'd; and that from this we infer something connected with it, which is not seen nor remember'd" (T 1.3.6.6; SBN 89). It might be that all that Hume is after here is perfectly ordinary inductive inference: something present to the mind has in the past been constantly conjoined with something else, and so from this present impression, we infer the existence of a new instance of this other phenomenon, which is not seen nor remembered, but expected. But I do not think that we *must* read Hume that way. For all that Hume says in these passages, that which is neither seen nor remembered *could* be something new, a theoretical posit represented by recombining the material of the senses with a view towards the explanatory aims of reason. All that Hume *requires* in the passages above is that our probable reasonings have some mixture of impressions and reason. Inductive inference is allowable under that rule, but so would be the kind of robust theoretical-explanatory posit that I have been urging makes for a more charitable and richer understanding of Hume's scientific methodology.

What I hope to have shown, then, is that it is possible that Hume's understanding of the science of human nature is more interesting and subtle than De Pierris casts it. Upon close examination, one can glimpse a picture of that understanding in which by combining reason with the deliverances of the senses we posit theoretical entities the nature, powers, and essence of which explain the empirical generalities that we discover in experience. On this reading, Hume is a kind of scientific realist. He takes the task of his science to be more than merely providing a descriptive phenomenology of the human mind or discovering

exceptionless generalities through induction. The task of science is to *explain* such generalities by employing a combination of reason and experience to represent the true nature of the human mind.

NOTES

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1 References are to Hume, *An Enquiry concerning Human Understanding*, ed. Beauchamp, hereafter cited in the text as "EHU" followed by section and paragraph number, and to Hume, *An Enquiry Concerning Human Understanding*, ed. Selby-Bigge, rev. by Nidditch, hereafter cited in the text as "SBN" followed by page numbers.

2 As a helpful anonymous reviewer at *Hume Studies* points out, Newton is also clear on this point insofar as the chapter just before Book 3 of his *Principia*, "Phenomena," undertakes to explain a number of phenomena, all of which are themselves empirical generalizations.

3 In fairness, the Ideal Gas Law does also allow us to expand the scope of antecedently-familiar laws of motion to include the behavior of gases, so the same example can be given an inductivist spin as well. See however, note 21 below. My thanks to an anonymous reviewer at *Hume Studies* for pointing this out.

N.B. Boyle's Law, which states roughly that the pressure and volume of a gas stand in an inversely proportional relationship when the temperature of the gas is held constant, is an experimental approximation of the Ideal Gas Law, and so is more obviously a mere empirical generalization than the latter. Of course, it is Daniel Bernoulli in *Hydrodynamica*, published a year before the *Treatise*, who derives Boyle's Law by applying Newton's laws of motion to posited collections of molecules, thereby explaining the empirical regularity via an appeal to a theoretical posit.

4 While Locke does model the causal relations among corpuscles on the "impact causation" of observable objects such as billiard balls, Hume nonetheless takes him to have failed to properly model such corpuscles themselves. That is, Hume takes Locke to posit corpuscles that consist of primary qualities alone, which, Hume argues in T 1.4.4, it is impossible to conceive.

5 For a different set of critiques of De Pierris's account of Hume's relation to Newton, see Hazony and Schliesser, "Newton and Hume," especially n68.

6 For what it is worth, the *best* examples of Hume employing this methodology would be provided by a detailed and thorough investigation of the most significant theoretical-explanatory moments from Hume's own pursuit of the science of human nature, for example, his drawing of the impression-idea distinction, or the simple-complex

distinction, or defending the Copy Principle, and so on. There is not space enough to perform such investigations here, but I have attempted to do so previously in Landy, "Hume's Impression-Idea Distinction," Landy, "A Puzzle about Hume's Theory," and Landy, "Hume's Theory of Mental Representation."

7 Boehm, "Hume's Foundational Project."

8 Hazony, "Netwonian Explanatory Reduction." In "Newton and Hume,"Hazony and Schliesser argue that this reduction should not be interpreted as *eliminativist* in part on the grounds that Hume takes himself to be giving an account *of* certain phenomena, which would not be possible if he took these *explicanda* to be "nothing but" the behaviors of their *explanans*. While I agree that explanatory reductionism need not be eliminavist *per se*, in Landy, "A Puzzle about Hume's Theory," I argue that the specific form that Hume's scientific realism takes does commit him to manifest phenomena's being mere appearance.

9 That said, while there is not space enough to defend the view here, I argue elsewhere that Hume is an anti-realist about the physical sciences insofar as he holds that the theoretical posit of *matter* is illegitimate (because it amounts to a mere *via negativa*). See Landy, *Hume's Science of Human Nature*. Thus, I count Hume as an *idealist*—all that exists are perceptions—but also a realist—it is the perceptions posited by the science of human nature rather than, for example, those observed through direct introspection, that are what *really* exist. Schliesser, "Hume's Attack on Newton's Philosophy," also argues that Hume rejects the conclusions of Newton's physical science, but on the grounds that, unlike the explanations of Hume's science of human nature, Newton's explanations move beyond the firm epistemic ground of "common life," what is "provable," to conjectures regarding what is merely "probable." Elsewhere, I argue that Hume's own distinctions are as conjectural as Newton's (if not more so), and so Hume's rejection of Newton cannot proceed in this way. See Landy, "A Puzzle about Hume's Theory."

10 The caveat limiting Hume's realism to successful science is important. Schliesser, "Hume's Attack," provides one way of bringing this out. Schliesser argues that Hume distinguishes between three epistemic categories: demonstrations, proofs, and probabilities. Demonstrations are limited to relations of ideas; proofs and probabilities concern matters of fact. A matter of fact that is proved thereby becomes subjectively certain, whereas one that merely has evidence in its favor, no matter how much evidence, is only probable and therefore not certain. Schliesser argues that, "at least four kinds of 'matters of fact' are susceptible to 'proofs': (1) claims about objects immediately present to senses and memory; (2) common sense (causal) claims; (3) results of some experiments in natural philosophy, especially if immediately present to eyes; (4) causal claims in moral sciences (*e.g.*, economics and politics)" (Schliesser, "Hume's Attack," 84). I would suggest that what these four types of matters of fact have in common is that there appears to be no immediate need to *explain* them. For example, Schliesser cites Hume's claim that if a friend were to throw himself out of the window, he could know with certainly that he will fall (EHU 8.20; SBN 91) as an example of (2). The reason that we can be subjectively certain of that fact, that it admits of proof is that we do not, at least in the moment, require any explanation of it. It appears, again in the moment, to be self-evident. The same is true of the objects that appear to the senses and memory (with their high degree of force and vivacity), experiments whose results

are "immediately present," and causal claims in the moral sciences (e.g., Schliesser's example of fairly-priced goods always finding a buyer). If it is true, though, that it is the lack of a demand for explanation of a matter of fact that makes it "proved" or subjectively certain, then whereas apparent self-evidence would be *one* way of achieving proof, the explanatory *completeness* at which science aims would be another—recall that Hume proposes "a compleat system of the sciences" (T Intro. 6; SBN xvi). Of course, in the end, Hume does not take even his own pursuit of the science of human nature to have been *entirely* successful (T 1.4.7.14; SBN 272–73), and so not even it will achieve the status of proof. Still, what scientific realism requires is not that any *actual* science be provable, but only that science, *when successful*, be such. If the goal of science is explanatory completeness, and a matter of fact is proved when it needs no explanation, then successful science achieves proof. (It is worth noting that Hume does appear to think that such a complete system of science will be forthcoming in due time (T 1.4.7.14; SBN 273).)

11 As an anonymous reviewer at *Hume Studies* points out, De Pierris's use of "induction" is limited to what we would now call "enumerative induction," whereas there is a broader-scope use of "induction" that includes any form of reasoning that moves from observed experience to invisible principles or causes. (By contrast, deduction moves from a general principle or cause to its effect.) Conceived broadly, enumerative induction is a form of induction because it makes implicit use of the principle of the uniformity of nature as its invisible principle, but so is inference to the best explanation, and the positing of unobservable theoretical entities. That said, I will continue to follow De Pierris in reserving "induction" and "inductivist" for enumerative induction, and contrasting these other forms of reasoning from the particular to the general with it.

12 That explanation must ultimately be answerable to experience is what Garrett calls Methodological Empiricism. Garrett correctly reads this as aimed at rationalist explanations that *directly conflict* with experience, not as a blanket prohibition on theoretical explanatory activity (Garrett, *Cognition and Commitment*, 30–31).

13 De Pierris, *Ideas, Evidence, and Method*, chapters 4 and 5 argue that Hume's radical skepticism is a second-order skepticism concerning only the epistemological status of the principle of the uniformity of nature, the ultimate ungroundedness of which nonetheless leaves in tact the norms governing common life and science. Nonetheless, as the quotation above brings out, even if Hume's meta-level skepticism about the principle of the uniformity of nature does not conflict with the norms of science, there is another kind of radical skepticism waiting in the wings that does. Namely, since De Pierris's Hume holds that observation is the only means of justifying scientific theorizing, he is also led to a radical skepticism regarding theoretical-explanatory posits. That might seem to be a relatively benign form of skepticism, but its consequences reach farther than I believe Hume would allow, a claim for which I will present evidence in the following two sections.

14 For example, see T 1.4.7.14 (SBN 272).

15 References to the *Treatise* are to Hume, *A Treatise of Human Nature*, ed. Norton and Norton, hereafter cited in the text as "T" followed by Book, part, section, and paragraph number, and to Hume, *A Treatise of Human Nature*, ed. Selby-Bigge, rev. by Nidditch, cited in the text as "SBN" followed by the page number.

16 One important feature that this example also exhibits is that a theoretical posit (as I understand it and attribute its use to Hume) represents the substance underlying the manifest phenomena that it explains. Thus, for example, an object's center of gravity or the mean solar time are not theoretical posits in this sense, but rather mere mathematical idealizations. These do not represent what massive bodies or the sun *are*, but only how they can be treated in certain equations describing them. On the other hand, a theoretical posit must specify not only what the underlying substance is like, but also what it is unlike. It must consist in some novel representational content which content is what does the work of *explaining* the manifest phenomena, rather than merely repeating it. In representing that which *underlies* manifest phenomena, a theoretical posit necessarily represents more than just what is familiar from ordinary life. I discuss this requirement in more depth in the final section on representing theoretical entities.

17 Of course, one might respond that Hume's call to explain such exceptionless generalizations amounts to nothing more than subsuming one such generalization under a second that is wider in scope. For example, if universal gravitation were the result of the differing densities of the ether surrounding massive bodies, then one could cast it as an instance of the laws of motion. This way of proceeding, however, runs dangerously close to becoming the Deductive-Nomological account of scientific explanation of the Logical Positivists, and there is good reason to reject on Hume's behalf any such interpretation (even if the Positivist's mistakenly took themselves to be inspired by Hume). Salmon, "Four Decades of Scientific Explanation," give an excellent overview of some of the most prominent refutations of their account.

18 Schliesser, "Hume's Newtonianism and Anti-Newtonianism."

19 One final note on this passage: as an anonymous reviewer at *Hume Studies* points out, this passage occurs as part of a note in which Hume insists that terms such a "*vis inertiae*" or "gravity" refer to only observed phenomena. That fact itself might appear to support the interpretation that Hume endorses Newton's casting of the ether as "mere hypothesis" because it appears to indicate that Hume holds that the content of theoretical terms is exhausted by a description of manifest phenomena. The context of this note is important, however. It comes at the conclusion of Hume's discussion of those philosophers who defend a, "theory of the universal energy and operation of the Supreme Being." Hume's objections to that theory are that (a) the posited deity is unlike anything found in experience and so is incomprehensible, and (b) since we attribute precisely those powers to the posited deity that we already observe in manifest phenomena, these powers can serve no explanatory purpose. The point of the note, then, is to present two corresponding defenses of empirical science. The first emphasizes the close tie between science and experience; this is the point about the descriptive content of "*vis intertiae*" and "gravity." That these terms refer only to observed phenomena, though, does not imply that such phenomena do not themselves stand in need of explanation. Thus, the second point presents the form of a genuinely explanatory theory: Newton himself appeals to the ether to explain gravity. (Finally Hume cautions that in both the divine and the scientific case acolytes can be too quick to accept mere hypotheses as established fact.)

20 Hazony, "Netwonian Explanatory Reduction."

21 See, for example, Strawson, *Secret Connexion*, and Wilson, "Is Hume a Skeptic."

22 Winkler, "The New Hume," and Flage, "Relative Ideas Revisited," are early and persuasive critics of the New Humeans, and their objections would not apply to my view. I also critique Strawson's interpretation of relative ideas along the lines mentioned in Landy, *Hume's Science of Human Nature*.

23 For example, Owen, *Hume's Reason*, 65–66.

24 Of course, this depends on what exactly Hume means by "essence," "nature," and "powers," including how his understanding of those terms relates to that of his predecessors, for example, Locke, who took these to be terms for an object's primary qualities, an understanding that Hume explicitly rejects. The question, then, is what exactly about Locke's understanding does Hume find wanting. Note that a full treatment of this issue would require a close examination of Hume's discussion of *substance* in T 1.1.6, 1.4.3, and 1.4.4 (SBN 15–17, 219–25, and 225–31). Suffice it to say for now that in those sections, Hume does not reject the notion of substance *simpliciter*, but only the *empty* understanding of substance as a *via negativa* that his Ancient and Modern predecessors employ. Hume himself gives what he takes to be the correct notion of substance in T 1.1.6: "the particular qualities, which form a substance . . . are at least suppos'd to be closely and inseparably connected by the relations of contiguity and causation" (T 1.1.6.2; SBN 16). The idea of substance, properly conceived, is the idea of some particular qualities, assigned the same name, that are "suppos'd to be closely and inseparably connected by the relations of contiguity and causation." The relation of contiguity that Hume refers to here is the contiguous appearance of each of the qualities at hand, but that turns out to be a non-starter because, as Hume notes a moment later, it is easy enough to imagine a scenario in which a mode displays this same relation. "The simple ideas of which modes are form'd, either represent qualities, which are not united by contiguity and causation, but are disperd'd in different subjects; or if they be all united together, the uniting principle is not regarded as the foundation of the complex idea" (T 1.1.6.3; SBN 17). So, the proper notion of substance is that of particular qualities connected via causal relations. *That*, it turns out, is exactly what the science of human nature aims to discover by offering causal explanations of the regularities found in experience. That is, in making use of theoretical-explanatory posits regarding the "nature," "powers," and "essence" of the human mind, what Hume thereby discovers are the causal relations that underlie the observable (introspectable) phenomena. (E.g., that the human mind consists of two kinds of perceptions, impressions and ideas, the former of which cause the latter, or that our complex phenomenology is the result of the recombination of certain simple components, and so on.)

25 As the examples in the next section will bring out, "reason" here includes any operation of the inferential faculty, including, I will suggest, inference to the best explanation.

26 An example of the first transgression would be the practice of those mechanical philosophers who took the conclusions of their *a priori* arguments about the nature of substance to be strong enough to override empirical evidence to the contrary. An example of the second would be the Peripatetic philosophers that Hume scolds in T 1.4.5 for their attempts to "feign something unknown and invisible" as that which underlies perceptual flux, the problem being not that such philosophers attempt to explain the behavior of our perceptions, but rather the "unintelligible something" to which they appeal to do so.

27 To be clear, merely *forming* a perceptible model ("a mixture of reason and impressions") or *drawing* some analogy is not sufficient for taking that which is thereby represented to exist. Belief in the existence of that which is pictured in this way can only be justified by the *explanatory force* of such a theoretical posit. So, merely picturing an atom as having a structure like the solar system is not what warrants believing in atoms, but rather that doing so *explains*, for example, the Rydberg formula for the spectral emission lines of atomic hydrogen.

28 At this point, a reader might well wonder how the scientific realism that I have attributed to Hume can be compatible with his repeated professed *scepticism*. Notice, though, that while Hume is skeptical about a great many subjects, he expresses almost no skepticism about the science of human nature itself, and even when he does express some mild skepticism about it—e.g., at T 1.4.7.3 (SBN 265)—he consistently refutes this skepticism in favor of ringing endorsements of his own conclusions (e.g., T 1.4.7.14; SBN 272–73). Furthermore, note that Hume's professed skepticism is of a very particular form: that reason alone cannot provide a justification for some thesis. In T 1.4.1, he concludes that reason cannot justify its own use; in T 1.4.2, he concludes that it cannot justify the use of the idea of body. In T 1.4.7, he likewise appears to concede that reason alone cannot provide a justification for pursuit of the science of human nature (T 1.4.7.9; SBN 269), but, like in the former two cases, also holds that *reason's* inability to provide this justification does not entail that no such justification exists. In the case of the science of human nature, it is that science's explanatory successes, and consequent ability to provide, "what is naturally satisfactory to the mind of man" that justifies its use. Cf. Loeb, *Stability and Justification*, 22 and Garrett, *Cognition and Commitment*, 234–37.

29 Again, space here does not allow a consideration of the most important examples of Hume's theoretical-explanatory activities, which I have discussed elsewhere. See note 12. Additionally, many of the examples in this section are drawn not from the science of human nature, but from Hume's consideration of the physical sciences, which, as previously noted, gives them a merely provisional status. Still, I believe that the more general *methodological* points demonstrated by these examples is sound.

30 In a note to a paragraph soon after this one (T 1.2.4.31 n12; SBN 638–39) Hume considers the question of "whether or not the invisible and intangible distance be always full of *body*, or of something that by an improvement of our organs might become visible or tangible," and appears, in his answer to that question, to endorse a proscription against theoretical-explanatory hypotheses. "If *the Newtonian* philosophy be rightly understood it will be found to mean no more. A vacuum is asserted: That is, bodies are said to be plac'd after such a manner, as to receive bodies betwixt them, without impulsion or penetration. The real nature of this position of bodies is unknown. We are only acquainted with its effects on the senses, and its power of receiving body. Nothing is more suitable to that philosophy, than a modest scepticism to a certain degree, and a fair confession of ignorance in subjects, that exceed all human capacity" (T 1.2.4.31 n12; SBN 639).There is not space enough here to argue the point, but I would suggest that this passage is not a *wholesale* rejection of theoretical-explanatory posits, but only of *Newton's* use of them to explain the regularities among perceptions *by appealing to external existence*. That is, I would argue that one conclusion at which Hume arrives in pursuing the science of human nature is that the very idea of body as it is understood by the vulgar and false philosophers is nonsense (precisely because it is an attempt to

deploy a theoretical posit without a suitable perceptible model). Thus, Hume politely chastises the Newtonian here not for deploying a theoretical explanatory model *per se*, but rather for deploying one that is not up to its task. I discuss this at more length and detail in Landy, *Hume's Science of Human Nature*.

31 Owen, *Hume's Reason*, repeatedly and vehemently denies that Hume countenances inference to the best explanation. He does so partly on the grounds that, as Owen reads him, Hume is committed to a "refusal to go beyond experience." That is, as Owen understands Hume, he limits his scientific-explanatory project to the kind of descriptive phenomenology and tentative extension thereof that we have already seen De Pierris advocate. Owen supports that latter claim, in turn, on readings of Intro. 8 and 9 contrary to the ones presented in the previous section.

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