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Abstract: Hume is aware that reason is useful for drawing conclusions about matters of fact: "Mathematics, indeed, are useful in all mechanical operations, and arithmetic in almost every art and profession" (T 2.3.3.2; SBN 413–14). But he offers no account of how relations of ideas direct our judgment concerning matters of fact. This is a pity, because the application of mathematics offers an excellent opportunity to observe the interplay between reason and experience, and thus it provides an interesting perspective on Hume's philosophy. This article aims to turn a handful of Hume's remarks into a Humean account of applied mathematics (§§1–3). The account is interesting on its own, but it reveals also an odd consequence for Hume's philosophy, viz., the existence of a species of probability, in which reason lends force and vivacity to inferences involving matters of fact (§4).

§0. Introduction

Hume describes the sciences as "noble entertainments" that are "proper food and nourishment" for reasonable beings (EHU 1.5–6; SBN 8).[1] But mathematics, in particular, is more than noble entertainment; for millennia, agriculture, building, commerce, and other sciences have depended upon applying mathematics.[2] In simpler cases, applied mathematics consists in inferring one matter of fact from another, say, the area of a floor from its length and width. In more sophisticated cases, applied mathematics consists in giving scientific theory a mathematical form and then explaining and predicting matters of fact by means of mathematics and

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the theory. Since Hume holds that, "All inferences from experience are . . . effects of custom, not of reasoning" (EHU 5.5; SBN 43, cf. T 1.4.1.8; SBN 149), he seems to be committed to saying that applied mathematics is founded upon custom or habit, rather than reason.[3]

Hume is aware, of course, that reason is useful for drawing conclusions about matters of fact:

> Mathematics, indeed, are useful in all mechanical operations, and arithmetic in almost every art and profession. Mechanics are the art of regulating the motions of bodies *to some design'd end or purpose*; and the reason why we employ arithmetic in fixing the proportions of number, is only that we may discover the proportions of their influence and operation. . . . Abstract or demonstrative reasoning, therefore, never influences any of our actions, but only as it directs our judgment concerning causes and effects. (T 2.3.3.2; SBN 413–14)

But he offers no account of how relations of ideas direct our judgment concerning causes and effects. This is a pity, because the application of mathematics offers an excellent opportunity to observe the interplay between reason and experience, and thus it provides an interesting perspective on Hume's philosophy.

This article aims to turn a handful of Hume's remarks into a Humean account of applied mathematics (§§1–3). The account is interesting on its own, but it reveals also an odd consequence for Hume's philosophy, viz., the existence of a species of probability, in which *reason* lends force and vivacity to inferences involving matters of fact (§4).

§1. Intermediate Ideas

The first commentator to call attention to Hume's philosophy of applied mathematics is Antony Flew.[4] He contends that Hume does not distinguish pure from applied mathematics in the *Treatise* but only later, in the *Enquiry* (*Belief*, 62). Flew designates EHU 4.13 as Hume's "seminal account of the nature of applied mathematics" (*Review*, 96).

> Every part of mixed mathematics proceeds upon the supposition that certain laws are established by nature in her operations; and abstract reasonings are employed, either to assist experience in the discovery of these laws, or to determine their influence in particular instances, where it depends upon any precise degree of distance and quantity. (EHU 4.13; SBN 31)

In this passage, Hume is concerned primarily to show that abstract reasoning won't reveal *causal* relations the way it reveals relations of proportion. But EHU 4.13 offers no *analysis* of applied or mixed[5] mathematics beyond describing the service it renders: assisting experience in discovery, prediction, and "giving us the just dimensions of all the parts and figures which can enter into any species of machine" (ibid.). There is no attempt to describe the connection between matters of fact and relations of ideas that obtains in applied mathematics.

Without offering further textual evidence, Flew proceeds to attribute to Hume a standard logical-empiricist position.

> Such bits of correct elementary arithmetic as the familiar 2+2=4 are logically necessary, logically certain, and can be known a priori. But consider what happens if you begin to apply them. You insert into the formula after each numeral some non-mathematical word. You replace the plus and equals signs by expressions for appropriate physical operations, such as putting together and counting. What you have then is no longer a snippet of pure mathematics, but a statement of the form: *'If you put two apples together with two apples and count what you then have you will find you have four apples.'* But this is not logically necessary. Nor could it be known a priori to be true. Indeed if you made it not apples but suitably sized lumps of uranium 235 anyone rash enough to test the truth of your resulting statement would produce a most spectacular falsifying experiment. (*Belief* 63, my italics)

This account is in some respects similar to the one I introduce in §3. Moreover, it is *prima facie* plausible, as Hume is often cited as the precursor of logical empiricism.[6] But it must ultimately fail as a reconstruction of Hume.

On Flew's reading, Hume treats mathematical truths as formally valid schemata, interpreted to yield empirical and so contingent propositions of applied mathematics like the statement italicized in the preceding.[7] Flew reinforces the logical-empiricist reading by comparing Hume's view favorably with Einstein's:

> As far as the laws of mathematics refer to reality, they are not certain, and as far as they are certain they do not refer to reality.[8]

Flew's comparison ignores, however, a fundamental difference between Hume's view and the logical-empiricists'. The difficulty is apparent in Einstein's remarks that follow the sentence that Flew quotes.

> It seems to me that complete clearness as to this state of things first became common property through that new departure in mathematics which is

> known by the name of mathematical logic or "Axiomatics." The progress achieved by axiomatics consists in its having neatly separated the logical-formal from its objective or intuitive content; according to axiomatics the logical-formal alone forms the subject-matter of mathematics, which is not concerned with the intuitive or other content associated with the logical-formal. (ibid.)

For several reasons, any explication of Hume that appeals to formal logic is unwarranted.

In the first place, doing so conflicts with the well-known antipathy of the British empiricists toward formal logic.[9] Like Euclid (and every early modern figure save perhaps Leibniz), Hume understands mathematical reasoning to be a function of content rather than logical form, and this ought to be reflected in his account of applied mathematics. Moreover, any explication of Hume that groups him with the logical-empiricists will be anachronistic. Axiomatics in Einstein's sense emerged only a century after Hume's death, as mathematicians developed the notion of abstract structure.[10] Abstract structure is *formal*; it's what remains in a theory when we ignore the reference of its non-logical terms ("one," "sum," "line," "triangle," and so on.). This is different from axiomatics in the sense of Euclid; for although Euclid's *Elements* proceeds from axioms, his inferences depend upon the content of his propositions, rather than their form.[11] As Ian Mueller puts it, "Euclid's 'formalism' is much more like formalism in literature, which focuses on stylistic niceties, than like formalism in mathematics, which is motivated by a philosophical conception of mathematics" (Mueller, 292). The difference to which Mueller refers is notoriously illustrated by the nineteenth-century "discovery" of gaps in Euclid's reasoning, gaps that were then filled by new axioms, like Pasch's and the axiom of continuity (Heath, I, 242). Further, the formal analysis of geometrical reasoning requires logical resources (quantifiers and variables), which appeared only in the late nineteenth century. Finally, neither of the passages in which Hume mentions applied mathematics (EHU 4.13; SBN 31, T 2.3.3.2; SBN 413–14) includes anything like the italicized statement in the quotation from Flew. There is good reason for their absence. In the logical-empiricist account, such propositions serve as premises that, together with observation statements, *formally* imply conclusions about real existence. But the need for such propositions disappears for a thinker, like Hume, who supposes that mathematical reasoning is intuitive rather than formal. In short, any explication of Hume's philosophy of applied mathematics must appeal to *relations between mathematical ideas* rather than formal relations that depend upon logical particles ("all," "not," "if . . . then," and so on).

Unfortunately, Hume's texts say little about relations among mathematical ideas. In this matter he simply follows Locke, for whom knowledge consists in "the perception of the connection and agreement or disagreement and repugnancy, of any of our ideas."[12] For Locke, reasoning increases knowledge by perceiving the agreement of ideas through the connection of the intermediate ideas that constitute its demonstration (Locke, 4.2.1–3, 530–32). Hume remarks only that mathematical propositions are known by intuition or demonstration (EHU 4.1; SNB 25) and that reasoning is nothing but a comparison of ideas (T 1.2.2.1, 1.3.2.2; SBN 29, 73). In order to develop a Humean account of applied mathematics it is necessary to investigate the intermediate ideas to which Locke refers. We must bring to light a particular subclass of intermediate ideas that is crucial to inferring (quantitative) matters of fact, one from another.

The investigation starts, of necessity, from Locke's account of the proof of *Elements* I.32.

> Thus the mind being willing to know the Agreement or Disagreement in bigness, between the three angles of a Triangle, and two right ones, cannot by an immediate view and comparing them, do it: because the three angles of a Triangle cannot be brought at once, and be compared with any other one, or two Angles; and so of this the Mind has no immediate, no intuitive Knowledge. In this Case the Mind is fain to find out some other angles, to which the three Angles of a Triangle have an Equality; and finding those equal to two right ones, comes to know their Equality to two right ones. (Locke, 4.2.2, 531)

The crux of Locke's account is the *intermediate idea*, in this case $\angle d + \angle b + \angle e$, which enables one to see the equality of $\angle a + \angle b + \angle c$ and $\angle f + \angle g$ (figure 1).

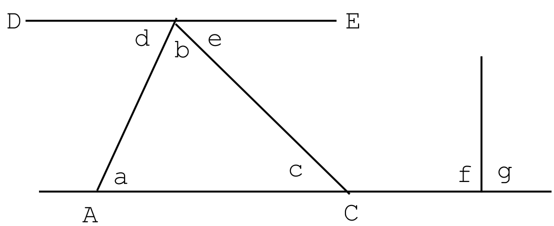

Figure 1

Here the intermediate idea is a clever construction, viz., DE parallel to AC. It takes talent to find intermediate ideas: "A quickness in the Mind to find out these intermediate *Ideas*, . . . and to apply them right, is, I suppose, that which is called *Sagacity*" (Locke, 4.2.3, 532).

David Owen formulates Locke's example "in propositional mode" as follows:

(1) Angle a + b + c is equal to angle d + b + e.

(2) Angle d + b + e is equal to angle f + g.

Therefore,

(3) Angle a + b + c is equal to angle f + g. (Owen, 37)

But he explains that this is *not* a formal argument, even though it is formally valid according to predicate logic (with identity).

> Although it can be reconstructed as a sequence of propositions, Locke sees the reasoning here as a sequence or chain of ideas: one starts with the idea of a + b + c, and ends with the idea of f + g. The idea of d + b + e is the intermediate idea that connects them in equality. . . . It counts as a demonstration because we intuitively perceive the relation between the ideas in the chain. Each link in the chain has to be intuitively known. . . . take away the intuitive certainty, and the demonstration disappears; there is no formal validity to fall back on. (Owen, 37–38)

As Owen sees it, the three ideas in the chain are linked by our intuitive grasp of (1) and (2). The equality of d + b + e and f + g, is, I grant, available to intuition. But there is no intuitive grasp of the equality of a + b + c and d + b + e; rather, it too needs demonstration. *One* intermediate idea in *that* demonstration is the construction of a line DE *through* one vertex of the triangle *and parallel to* the side opposite that vertex. Unless that line is parallel to the opposite side there is no equality to be demonstrated, let alone intuited. Moreover, even if it is known that DE is parallel to AC, the equality of *d + b + e constructed on the parallel DE* and *a + b + c* is not known intuitively. That too must be demonstrated by means of a previous theorem, viz., a straight line falling on parallel straight lines makes the alternate angles equal to one another (I.29). There is, in fact, a long chain of ideas connecting a + b + c and d + b + c, a chain that constitutes a proof of I.29. Owen's analysis of the reasoning behind I.32 requires that the reasoner take note of *each* of the intermediate ideas in *each* of the propositions presupposed in Euclid's proof of I.32. This can't be a satisfactory analysis simply because such a chain of intermediate ideas is too long to permit a surveyable proof.[13] It is doubtful, moreover, that the chain of ideas strategy applies to proofs like that of the Pythagorean theorem, which don't turn on the transitivity of identity. Fortunately, there is a more natural way to understand informal mathematical reasoning—one that fits within Hume's philosophy and avoids the difficulty of unsurveyable proofs.

I propose two sorts of intermediate ideas. In the first place there are auxiliary constructions like the parallel DE; Owen and other commentators appear to limit ideas to such constructions. But to avoid unsurveyable proofs, there must be another type of intermediate idea, viz., a rule in accordance with which relations of ideas are inferred. In the proof of I.32 the equality of ∠a and ∠d and the equality of ∠c and ∠e are both inferred in accordance with I.29. Thus, I.29 serves as an intermediate idea that enables us to compare two ideas whose relationship is not apparent. Here, too, sagacity is required to recognize what rule to apply in the course of demonstration.

To appreciate further the necessity of intermediate ideas that are rules rather than auxiliary constructions, consider a theorem that requires no such construction.

> *Bisector Theorem*. The bisector of the summit angle of an isosceles triangle is the perpendicular bisector of the base (figure 2).[14]

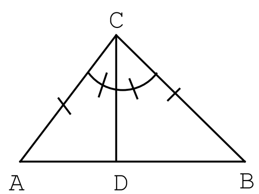

Figure 2

We seek to understand the relation between the bisector of the summit angle of an isosceles triangle and the perpendicular bisector of its base. No further construction is necessary to perceive that the two ideas denote the same line. Rather, in accordance with I.4, one infers from the original complex idea (specifically, from its component ideas ∠ACD = ∠DCB, AC = BC and CD = CD) that AD = DB and ∠ADC = ∠BDC. From the second equality, in accordance with the definition of "right angle," one infers that ∠ADC and ∠BDC are both right.[15]

Hume shouldn't balk at allowing intermediate ideas to include rules as well as auxiliary constructions. On the one hand, he understands propositions of mathematics as complex ideas: a demonstration is a process that forms a complex idea (T 1.3.7.5n; SBN 96–97n. Cf. Owen, 103–104). On the other, Hume refers explicitly to rules of demonstrative science, which "are certain and infallible" (T 1.4.1.1; SBN 180). Demonstrative science is, for practical purposes, a body of rules. It is by means of rules—rules in accordance with which inferences are drawn—that mathematics is applied to the world.

§2. Stipulation

The account of mathematical reasoning I have foisted upon Hume is thus far consistent with both the *Treatise* and the *Enquiry*. Yet it's well known that the two texts adopt different attitudes toward geometry—a difference connected to the fact that only in the latter text does Hume recognize applied mathematics. I shall defer to the *Enquiry* in filling out Hume's account of mathematical reasoning. However, it proves useful to understand how Hume was led away from the *Treatise* position. In the *Enquiry* Hume holds that the ideas of geometry

> being always sensible, are always clear and determinate, the smallest distinction between them is immediately perceptible, and the same terms are still expressive of the same ideas, without ambiguity or variation. An oval is never mistaken for a circle, nor an hyperbola for an ellipsis. The isosceles and scalenum are distinguished by boundaries more exact than vice and virtue, right and wrong. (EHU 7.1; SBN 60)

In contrast, the *Treatise* observes that we cannot give definitions which fix precise boundaries between equal and unequal, straight and curved, and so argues that there are no firm boundaries between geometrical ideas.

> [W]e form the loose idea of a perfect standard to these figures, without being able to explain or comprehend it. (T 1.2.4.25; SBN 49)

> It appears, then, that the ideas which are most essential to geometry, viz., those of equality and inequality, of a right line and a plain surface, are far from being exact and determinate, according to our common method of conceiving them. (T 1.2.4.29; SBN 50–51)

If we cannot fix precisely when two magnitudes are equal, we cannot, apparently, distinguish isosceles from scalene, and so on. Hence, the *Treatise* admits borderline cases among geometrical ideas, contrary to the doctrine of the *Enquiry*.

This difference between the *Treatise* and *Enquiry* is familiar to scholars;[16] Vadim Batitsky, as far as I know, is the only scholar who has made a whole-hearted effort to explain it. He maintains that Hume adopted the position of the *Enquiry* after discovering two problems in the *Treatise*: (1) He realized that he could not maintain *both* that our geometrical ideas are determined by our best measurement procedures *and* that our best measurements are inexact; for the inexactness of our measurement procedures presupposes "perfect geometrical properties" (Batitsky, 11). (2) Hume also realized that his reason for holding that no measuring technique can secure us from error, viz., "sound reason convinces us that there are bodies vastly more minute than those, which appear to the senses" (T 1.2.4.24; SBN 48), offends empiricist scruples; for it claims to know something about bodies which is not given to the senses (Batitsky, 11; cf. T 1.2.5.26; SBN 64). Neither claim is decisive against the position of the *Treatise*.

First, the inexactness of our best measuring procedures needn't presuppose perfect geometrical properties, even if geometrical ideas are determined by measuring procedures. Hume argues that our measuring procedures are inexact by appealing to our inability to say of two lines that "approach at the rate of an inch in twenty leagues" whether they share a common segment or not. Both their concurrence in a segment and their coincidence in a single point are consistent with any of our measuring techniques (T 1.2.4.30; SBN 51–52). But borderline cases show only that a concept is inexact; they needn't presuppose perfect geometrical properties. In general, the inexactness of an empirical concept does not presuppose an exact definition; so, for instance, our empirical concept "tree" does not presuppose an exact definition.

Second, accepting that bodies exist beneath the threshold of sensation need not offend empiricist scruples. Hume doesn't tell us what "sound reason" convinced him of this thesis, but the *Treatise* contains one.

> A microscope or telescope, which renders [minute parts of distant bodies] 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 28)

Past advances in optical technology support the idea that future advances will reveal bodies still more minute than those which now appear to the senses. Today's imperceptible will be tomorrow's minimum and today's minimum will have parts tomorrow. Even if this is not an a priori consideration, it is uncharitable to insist that "sound reason" rule out such an argument.

Batitsky's explanation may be unsatisfactory, but he is surely correct that Hume's new estimate of geometry needs explanation (Batitsky, 2). I suggest that Hume abandoned the inexact geometrical concepts of the *Treatise* for the exact geometrical concepts of the *Enquiry*, once he recognized that the absence of "any instrument or art of measuring, which can secure us from all error and uncertainty" (T 1.2.4.24; SBN 48) *makes no difference to pure geometry*. This realization explains the *Enquiry* thesis that "propositions of this kind are discoverable by the mere operation of thought" (EHU 4.1; SBN 25). Hume, unfortunately, never explicates "the mere operation of thought." We must fill in the details in order to develop Hume's account of pure mathematics, and subsequently his account of applied mathematics.

In the *Enquiry*, mathematical concepts are derived from sense impressions by the mind's creative power, "the faculty of compounding, transposing, augmenting, or diminishing the material afforded us by the senses and experience" (EHU 2.5; SBN19). Hume may have thought, for example, that the process of diminishing, without limit, the difference between two magnitudes can yield an exact equality. Yet, it would be a mistake to suppose that the mind creates its geometrical ideas in a manner analogous to an individual creating an artifact from raw materials. We perceive the end results of the artisan's work, but we can't be said to perceive exact geometrical ideas in the same sense. To see that this is so, consider the figures employed in geometrical reasoning. They are either physical diagrams or ideas in the imagination. Such figures are essential to (traditional) geometric proof, and yet they are *imperfect* representations of exact geometrical ideas. The particular figure is necessary to fill gaps that contemporary mathematics fills by means of additional axioms.[17] Furthermore, in order for the figure to aid in recognizing the agreement of ideas, we must literally be able to perform operations upon it, extending lines, constructing angles, and so on.[18] Imperfect representations would not be necessary if exact geometric ideas were available for the understanding to inspect and compare.

The *Enquiry* says little about the relation between imperfect representations and their exact counterparts. But "though there never were a circle or a triangle" seems to imply a distinction between the representation and the idea it conveys. How is such a distinction possible without appealing to abstract general ideas, which *do* offend empiricist scruples? Hume likely thought that Berkeley's treatment of general ideas sufficed as an account of the use of particular figures in geometric proof.[19] Hume notes, for instance, that a particular line "may be made to represent others, which have different degrees of [quantity and quality]" (T 1.1.7.3; SBN 19). Elsewhere he observes that the idea of an equilateral triangle of an inch perpendicular may be used to represent other ideas, like rectilinear figure or triangle (T 1.1.7.9; SBN 21). Berkeley explains this mode of representation as follows.

> [T]hough the idea I have in view whilst I make the demonstration be, for instance, that of an isosceles rectangular triangle whose sides are of a determinate length, I may nevertheless be certain it extends to all other rectilinear triangles, of what sort of bigness soever. And that because neither the right angle, nor the equality, nor determinate length of the sides are at all concerned in the demonstration. (Berkeley, §16)

Presumably Hume endorses the same strategy.

Berkeley's analysis explains satisfactorily *one* aspect of reasoning with particular figures, viz., their use in proving a theorem which denotes ideas which are *identical* with or *more general than* the "idea in view." But, there are cases of mathematical reasoning that fall *outside* the scope of *Principles*, §16. Not only are there cases like the bisector theorem, which used a distinctly scalene triangular diagram to prove a theorem about isosceles triangles, but there are also reductio proofs, which reason upon an object which is shown not even to be imaginable.[20]

For example, the proof of *Elements* I.6 requires reasoning about a triangle with equal base angles but unequal sides; this figure can neither be drawn nor imagined. A theory of mathematical reasoning must account for such cases, and an empiricist theory that lacks the resources of modern logic and mathematics must do so in terms of a careful analysis of the use of particular figures. An empiricist account of *stipulation* is necessary to make room for such cases. Robert Fogelin suggests that Hume failed to appreciate the role of stipulation in geometry (Fogelin, 57), and I'm inclined to agree. But even if this is so, Hume has at hand the resources to include stipulation within an empiricist theory of geometric reasoning.

A rarely noticed, though altogether commonplace feature of geometric reasoning is that the same diagram can be used in proving quite different theorems. For instance, besides its use in the bisector theorem, fig. 2 can *also* be used to show that if $\angle ACB$ is right and $CD \perp AB$, then $\Delta CBA \sim \Delta ACD \sim \Delta CDB$ (fig. 2').[21]

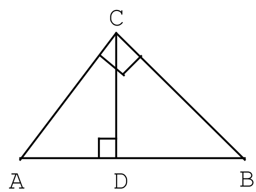

Figure 2'

The freedom that we enjoy in the use of a diagram arises from our capacity to *let* a diagram of, say, a triangle be a triangle of a certain sort, say, isosceles. This we accomplish by *treating* two sides of the triangle *as if* they are equal, that is, by stipulation. How do we manage that? In proving the bisector theorem, for example, one treats AC and BC in fig. 2 as if they are equal by being prepared to infer (among other things) that ΔACD is congruent with ΔBDC. In general one treats objects as if they're Φ by being prepared, regardless of appearance, to draw inferences warranted by their being Φ.

Jacquette contends that Hume would not need to be reminded that geometric reasoning proceeds by stipulation and argues that stipulation can't get a foothold without an exact concept of equality, a concept which Hume shows to be unavailable to anyone who accepts classical geometry.[22] We needn't accept this interpretation. Hume argued against classical geometry, which employs infinite divisibility, by observing that infinite divisibility implies that every segment has infinitely many sub-segments; if this is so, equality can't be determined exactly because all segments have the same number of sub-segments. While it is true that the classical geometer has no means of judging that two magnitudes are equal, his demonstrations do not require him to make such judgments. He judges, for example, only that *if* the base angles of a triangle are equal, *then* the corresponding sides are equal. He does not judge that the base angles are equal; rather he *stipulates* that they are. Likewise, he does not judge that the corresponding sides are equal; rather he infers that they are in light of the assumption.

Stipulation requires only that one *treat* objects *as if* they instantiate some idea (property or relation), and *pace* Jacquette, ideas adequate to that purpose are available. Equality, for instance, is available thanks to familiar rules like 'things which equal the same thing also equal one another.' Such rules prescribe for us how to treat magnitudes as if they are equal; theorems can serve the same purpose. The rules governing equality have, moreover, an empiricist foothold. Hume grants that arithmetic and algebra contain an exact standard of equality.

> We are possest of a precise standard, by which we can judge of the equality and proportion of numbers; and according as they correspond or not to that standard, we determine their relations, without any possibility of error. When two numbers are so combin'd as that the one has always an unite answering to every unite of the other, we pronounce them equal. (T 1.3.1.5; SBN 71)

The existence of *some* impression of exact equality is all that is needed for stipulations of equality to get an empiricist foothold. Stipulating that two magnitudes are equal amounts to no more than treating them as if they were quantities for which we have an exact idea of equality—drawing, for instance, the kinds of inferences warranted by Euclid's common notions. Since stipulation allows the geometer to avoid having to judge that two magnitudes are equal, concerns about the absence of a standard of exact equality for geometrical magnitude don't undermine traditional geometric reasoning. It is, of course, sometimes necessary to judge (rather than to stipulate or infer) that two magnitudes are equal, but never in the context of pure geometry.

Ideas of a right line and a plain surface, which are as essential to geometry as equality (T 1.2.4.29; SBN 50), can also get an empiricist foothold. To treat a magnitude AB as a right line one need only stipulate that no other path connecting A and B is to count as shorter than AB. Since the sense of a term like "shorter" can be traced back to impressions, stipulation allows the mathematician to reason about right lines in an empirically acceptable way. "Plain surface" can be handled similarly. And as before, concerns about whether a given line is right or a given surface plain are problems for the applied mathematician.

Stipulation obviously gives a better explanation of Hume's "though there never were a circle or triangle in nature, the truths demonstrated by Euclid would forever retain their certainty and evidence" than Platonism or formalism. It also explains the need to distinguish applied from pure mathematics in the *Enquiry*; for only the former must deal with the inexactness of the empirical world.

§3. Applied Mathematics

It is a surprisingly short step from this account of mathematical demonstration to an analysis of applied mathematical reasoning. Applied mathematics is simply a matter of inferring contingent propositions *in accordance with* mathematical rules, in close analogy to the model we've already employed for geometrical demonstration.

Imagine a landscape gardener faced with a roughly triangular park, two sides of which are, as far as he can tell, equal. Using pegs and a cord, and in accordance with *Elements* I.9, he draws a line which bisects the angle between the roughly equal sides and, again with pegs and cord, extends the line to the remaining side of the triangular area. In accordance with the bisector theorem he infers that his new line cuts that remaining side in half. This procedure involves some subtle conceptual maneuvers. To begin with, by eye-balling or by some measurement procedure the gardener determines that the park has various *empirical* characteristics, a triangular shape with a pair of roughly equal sides. Of course, such determinations are not infallible: the straightness of the sides may have been an illusion, the measurement may have been sloppy, or what looked at a distance to be a single angle may turn out to be a short side connecting two angles. Thus, neither I.9 nor the bisector theorem applies immediately to impressions, for they are not exact. The applicability of a theorem to one or more impressions depends on what can be called "identification."[23] The gardener *identifies* the park with an exact idea, say isosceles triangle, by choosing to *treat* the park *as if* it were a mathematical object of a certain sort.[24] In light of this choice, I.9 and the bisector theorem become applicable; and as a result, the gardener commits himself to treating the remaining side as though it has been cut in two. If subsequent measurement shows the remaining side not to have been cut in two, he faults not the rules in accordance with which his inference was drawn but either the decision to treat the park as if it were an isosceles triangle, or the sloppiness of the peg and cord construction.

Stipulation in pure mathematics and identification in applied mathematics both require one to bring conceptual resources to the objects about which one reasons. Whether through stipulation or identification, once we choose to treat an object as embodying such and such an idea, there is nothing to distinguish a pure from an applied mathematical inference *except* the use to which the conclusion is put.

Unfortunately, the account of mixed mathematics at EHU 4.13 shows no concern for the inexactness of the empirical world, let alone the role of identification in reasonings that determine the "influence of natural laws in particular instances."[25] When Hume writes, "Geometry assists us in the application of this law, by giving us the just dimensions of all the parts and figures which can enter into any species of machine" (EHU 4.13; SBN 31), he provides no insight into the relation between the exact ideas in terms of which the inference is carried out and the inexact ideas or impressions (that is, the results of measuring and counting) in terms of which the conclusion is to be understood. The dimensions are just only because the laws themselves involve exact ideas. The closest Hume comes to worrying about the mismatch between the inexact empirical realm and the exact mathematical realm is at T 1.3.1.4; SBN 71, where he suggests that the principles of geometry might not match "the prodigious minuteness of which nature is susceptible." But he responds to this worry by contending that as far as geometry is concerned, "its mistakes can never be of any consequence" (T 1.3.1.6; SBN 72). In defending the idea that the mistakes of Euclidean geometry can never be of any consequence, he appeals to a problem of *pure* rather than applied geometry—the number of right angles in a chiliagon (ibid.). He observes that the eye couldn't determine the number of right angles in a chiliagon as accurately as a demonstration from first principles. Even here, then, Hume is not concerned with accommodating the inexactness of the empirical realm to the exactness of the mathematical realm. Generally speaking, Hume fails to appreciate that mathematics enables us to connect impressions by *modeling* them with exact concepts. This failure is responsible for a gap in his philosophy of probability.

§4. Applied Mathematics and Probable Inference

In applying mathematics one infers a contingent conclusion from contingent premises, in accordance with a mathematical rule. Though such inferences are carried off with barely a thought, they involve the subtle conceptual maneuver of identifying objects of experience with exact mathematical ideas. It's safe to say that Hume's reflections on applied mathematics never progressed this far. Had they, he would have had to qualify his thesis that probable reasoning is exclusively the effect of custom or habit. Even though someone could assure himself of

> A weighs the same as B, and B weighs the same as C.
>
> ∴ A weighs the same as C.

by observing that the result of first two acts of weighing is constantly conjoined with the result of the third act of weighing, it would be more usual to justify the inference by appealing to a rule such as "things which equal the same thing also equal one another." These justifications are different in kind. The principle of the former is custom or habit, while the principle of the latter is understanding. Custom is subject to revision in the course of experience, but reason is not. Yet the inference each warrants is, in this case, undoubtedly probable. It could happen that the difference in weight between A and B and the difference in weight between B and C is *not* perceptually distinguishable, while the difference in weight between A and C is.

The probability of an inference based in constant conjunction is easily understood, since there is no presumption of conceptual connection between elements of experience. The probability of an inference in accordance with a mathematical rule arises in a more indirect manner as follows: The exact concepts in accordance with which the inference is drawn may be improperly identified with the inexact concepts in terms of which the premises and conclusion are stated; thus the connection between the exact concepts may fail to correspond to a connection between the inexact concepts. Hume's discussion of probable reasoning leaves no place for this kind of failure.

The *Treatise* distinguishes three sorts of probable reasoning: probability of chances, probability of causes and probability of analogy (T 1.3.11–13; SBN 124–42. Cf. EHU 6; SBN 56–59). All, according to Hume, are

> founded on two particulars, viz. the constant conjunction of any two objects in all past experience, and the resemblance of a present object to any one of them. The effect of these two particulars is, that the present object invigorates and inlivens the imagination; and the resemblance, *along with the constant union*, conveys this force and vivacity to the related idea; which we are therefore said to believe, or assent to. (T 1.3.12.25; SBN 142, my italics)

Of the species of probabilistic reasoning, applied mathematical reasoning is closest to reasoning by analogy. For, Hume writes,

> If you weaken either the union or the resemblance, you weaken the principle of transition and of consequence that belief, which arises from it. . . . In those probabilities of chance and causes above explain'd, 'tis the constancy of the union, which is diminished; and in the probability deriv'd from analogy, 'tis the resemblance only, which is affected. (ibid.)

Similarly, as just observed, the probability of applied mathematical inference depends on the degree to which inexact impressions resemble the exact ideas of the rule in accordance with which the inference is drawn.

But applied mathematical reasoning is not simply reasoning by analogy. For Hume maintains that

> [w]ithout some degree of resemblance, *as well as union*, 'tis impossible there can be any reasoning. (ibid., my italics)

Yet applied mathematical inference does not involve a union, that is, a constant conjunction, of two objects in experience. In place of the constant conjunction is a relation of ideas (a mathematical rule), which originates from the understanding; the ideas contained in the rule are connected in understanding rather than experience. Moreover, custom is responsible for neither the rule nor the resemblance between inexact and exact concepts on which applied mathematics depends. For neither the relation of ideas nor the resemblance is a result of custom; the mind is aware of them immediately.

Hume reinforces this claim. In arguing for the doctrine that we are determined by custom alone to expect one object from the appearance of another he writes,

> This hypothesis seems even the only one which explains the difficulty, why we draw from a thousand instances, an inference which we are not able to draw from one instance, that is, in no respect different from them. *Reason is incapable of any such variation.* (EHU 5.5; SBN 43, my italics)

An inference from the dimensions of a floor to its area is not strengthened by finding, repeatedly, that *these* dimensions are conjoined with *that* area. Even the idea of constant conjunction makes little sense here because it almost never happens that one determines area independently[26] of measuring linear dimensions and drawing an inference in accordance with mathematical rules.

One might object at this point that the experience of constant conjunction occurred in the dim past, when we first learned mathematics. No doubt the connection between length, width and area can be taught by counting, in various circumstances, the squares along two sides of a figure and the squares contained within the figure and noticing that the product of the first two results is equal to the second result. And no doubt the goal of such instruction is to impart a habit or custom. But it's a habit different from one engendered by constant conjunction. The habit that flows from recognizing, as Hume puts it, the agreement of two ideas needs no further reinforcement, since reason is *incapable of variation*. The habit associated with our seeing, for example, the agreement of area and the product of length and width shows itself in our attitude toward an empirical conclusion drawn in accordance with that agreement: Having obtained two numbers measuring the length and width of a floor, we refuse to accept any number different from the product of the first two as the area of that floor *unless* we have specific evidence that the measurements are inaccurate *or unless* we have specific evidence that the floor cannot be modeled by a rectangle. Acquiring such a habit is a matter of catching on to a practice, rather than a matter of strengthening, over time, a connection between impressions. Once one has caught on to inferring an empirical proposition in accordance with a mathematical rule, further experience no more strengthens the connection between premise and conclusion than looking at several editions of the same paper strengthens one's conviction in the headline.

Applied mathematical reasoning is a species of probable reasoning, but in place of a constant union is a rule of demonstrative science; in other words, a connection in understanding substitutes for a connection in experience. There is no doubt that applied mathematical inferences convey force and vivacity to their conclusions; but how is this possible? As Hume points out in discussing probability of analogy, resemblance alone—in this case between the (inexact) ideas that constitute the empirical premises and the exact ideas that constitute the mathematical rule—is insufficient to convey force to the conclusion. There must be some connection between the ideas constituting the premises and the ideas constituting the conclusion. That connection can come only from the connection embodied in the rule itself.

Hume apparently intends to exclude this line of thought in the *Treatise* when he writes, "Abstract or demonstrative reasoning, therefore, never influences any of our actions, but only as it directs our judgment concerning causes and effects" (T 2.3.3.2; SBN 413–14). But his arguments are hardly satisfactory. On the one hand, he argues that the proper province of the understanding is the world of ideas. This is persuasive only to the extent that one imagines that science is just noble entertainment, a sentiment that conflicts not only with the lives we lead, but also with nature's plea that science "have a direct reference to action and society" (EHU I.6; SBN 9). On the other hand, he argues that a merchant is desirous of knowing the sum total of his accounts only "that he may learn what sum will have the same *effects* in paying his debt, and going to market, as all the particular articles taken together." But the vivacity of the merchant's belief that the effect will indeed be the *same* has to arise from some source; and absent a relation of identity between a set of numbers and their sum, the merchant has no motivation to think that a certain number of pounds would have the same effect as the numbers of pounds it would take to settle several different accounts individually.

In sum, contrary to Hume's suggestion that inferences from experience are effects of custom, not of reasoning, it appears that reason must be able to invigorate and enliven some matters of fact. The degree of assurance depends, of course, on the degree of resemblance and so falls short of certainty. But a relation of ideas in accordance with which the inference is drawn can play a role strongly analogous to the constant conjunction of two impressions in past experience.

A recent treatment of Hume's discussion of mixed mathematics at EHU 4.13 appears to conflict with my claim that applied mathematics is a species of probable reasoning.[27] Peter Millican provides a full and interesting illustration of a sort of reasoning to which Hume refers in that passage:

> The argument sketched below is deductively valid in the modern informal sense, and would, I believe, undoubtedly be classed by Hume as "demonstrative":

> 1. The momentum of a body is equal to its mass multiplied by its velocity.
> 2. In any collision the total momentum of the colliding bodies (in any given direction) is conserved.
>
> ∴ If a spherical rigid body of mass 2kg moving directly eastward at 25,000 m/s collides head-on and instantly sticks fast to a second spherical rigid body of mass 10,000 kg which is moving directly westward at 4 m/s (without any breakage, any simultaneous interaction with other bodies, any change of mass, etc.), then the second body will no longer be moving westward immediately after the collision.
>
> This is precisely the kind of applied mathematics that Hume discusses at [EHU 4.13; SBN 31]. (Millican, 133–34)

Let us call Millican's illustration "the momentum argument." It helps to demonstrate Millican's thesis that Hume allows demonstrative reasoning from contingent premises, and it provides an excellent illustration of what Hume means by "mixed mathematics." I have no qualms about the demonstrative status of the momentum argument, provided one recognizes that its demonstrative character arises from its employing exact concepts. The numerical concepts must be exact, of course, as must the concept of equality. But the non-mathematical concepts, for instance, momentum, mass, and velocity (which includes the exact concepts 'length in meters' and 'duration in seconds') must be exact too. Their exactness is a function of, among other things, their being continuous quantities in a specific sense: The levels of the quantity are *dense*, that is, between any two levels there is a third. Unless it is *presumed* that mass, length, and so on, have this kind of structure, there are no grounds for using real numbers as their values and for applying the mathematics of real numbers in drawing inferences about relations among these values. That is, without this presumption there is no assurance that the properties of the real number system will correspond to properties of the objects to which they are applied.[28] Owing to our limited perceptual capacities, the empirical concepts of momentum, mass, and so on *lack* this rich structure; we can, for example, discern only finitely many values of momentum, mass, and so on, and between two levels of mass, we may not be able to discern a third. Moreover, empirical concepts of the levels of physical quantities admit of borderline cases, and hence we may not be able to say whether two objects have the same mass, or whether one object has 5,000 (rather than 5000.1, say) times the mass of another.

To appreciate further that the momentum argument runs on exact concepts, consider an alternative version, one in which the values of the quantities are different and the hypothesis of the conclusion is instead asserted as one of the premises.

1. The momentum of a body is equal to its mass multiplied by its velocity.
2. In any collision the total momentum of the colliding bodies (in any given direction) is conserved.
3. A spherical rigid body of mass 2kg moving directly eastward at 25,000 m/s collides head-on and instantly sticks fast to a second spherical rigid body of mass 9,999.9 kg which is moving directly westward at 5m/s (without any breakage, any simultaneous interaction with other bodies, any change of mass, etc.).

∴ The second body will no longer be moving westward immediately after the collision.

If we treat these concepts as exact, then the conclusion will follow demonstratively, in accordance with the mathematical rules:

i) $2 \times 25{,}000 = 50{,}000$

ii) $9{,}999.9 \times 5 = 49{,}999.5$, and

iii) $50{,}000 > 49{,}999.5$.

But if the concepts in the third premise (for example, "body of mass 9,999.9 kg") are *inexact*, as they are bound to be when their application is the result of measurement rather than stipulation, then it is entirely possible that the conclusion turns out to be false. If we had either barely over-estimated the velocity of the eastward moving object or barely under-estimated the mass of the westward moving object, the westward object might have continued on its course.

Millican's version of the momentum argument is reminiscent of a problem from a physics text, in which it is presumed (by the instructor, if not the unwary student) that all the concepts are exact.[29] In other words, the quantitative attributes of the objects are *stipulated* to be such and such rather than *determined empirically* to be such and such by measurement. The prediction involved in Millican's version of the argument is a *hypothetical* prediction, because the initial conditions are stipulated rather than asserted on the basis of observation; hence, the conditional form of the conclusion. The appearance of the open-ended *ceteris paribus* clause ("without any breakage . . . mass, etc.") underscores this point, because outside the context of teaching computational techniques and proving theorems, such assumptions are unwarranted. Unlike Millican's version, the alternative version of the momentum argument involves a *categorical* prediction, because the third premise asserts the contents of an observation. Thus, whereas Millican's version of the argument is demonstrative, the alternative version is not. The alternate version falls, indeed, under the species of probabilistic reasoning we've just uncovered.

The different momentum arguments point to a difference between what Hume apparently means by "mixed mathematics" at EHU 4.13, and what I have been calling "applied mathematics." Mixed mathematics consists of demonstrative reasoning, while applied mathematics is probable reasoning in accordance with mathematical rules. Paradigm cases of the former appear in the first two books of Newton's *Principia*, which contain theorems like "if several bodies revolve about one common centre, and the centripetal force is inversely as the square of the distance of places from the centre, I say, that the periodic times in ellipses are as the 3/2th power of their greater axes."[30] Newton derives this theorem, a generalization of Kepler's third law, from his laws and definitions of motion, all of which are constituted by exact concepts. Paradigm cases of applied mathematics include the reasoning of the landscape gardener, discussed above, as well as inferences of shopkeepers, builders, and farmers, none of which involve scientific laws. There are also hybrid cases, such as those that appear only in the *third* book of *Principia*, in which Newton's system of the world is confirmed by successful predictions and explanations. In the third book, Newton confirms the just quoted theorem by appealing to the observations of Borelli, Townly, and Cassini (Newton, 407). Likewise, in accordance with the principles of the first two books, he explains various phenomena, e.g., that tides are greatest when the luminaries are nearest the earth (Newton, 582), and predicts novel phenomena, like the flattening of the earth at the poles (Newton, 428–33). The hybrid cases are all probable, rather than demonstrative reasoning; for the distances, times, masses, and forces are measured rather than stipulated. Thus they leave open the possibility that the model and the phenomenon under investigation lack a sufficient degree of resemblance.

While the distinction I've drawn between different senses of applied mathematics is a subtle one, it is one that Hume was probably capable of drawing for himself. The discussion of mixed mathematics at EHU 4.13 distinguishes uses of mathematics that "assist experience in the discovery of these laws" from uses which "determine their influence in particular instances." The former are illustrated by Millican's momentum argument, whose conclusion is a law. The latter are illustrated by the alternative argument, provided "particular instances" refers to the particular measurements from which the conclusion is drawn in accordance with a mathematical rule. Furthermore, the same distinction was current in the eighteenth century, as in the following passage from Chambers's *Cyclopaedia*:

> Mathematics are distinguished with regard to their end, into speculative mathematics, which rest in the bare contemplation of the properties of things; and practical mathematics, which apply the knowledge of those properties to some uses in life. With regard to their object, mathematics are divided into pure or abstract; and mixed. Pure mathematics consider

> quantity, abstractly; and without any relation to matter or bodies. Mixed mathematics, consider quantity as subsisting in material being: e. gr. length in a road, breadth in a river, height in a star, or the like.[31]

Without a doubt, practical mathematics is probable reasoning, but it is not as obvious that mixed mathematics is always demonstrative. It is hard to say whether hybrid cases, which require both scientific laws and actual measurements, count as applications of mathematical knowledge to "some uses in life." In Hume's day, especially, the hybrid cases dealt with abstract matters, far from the concerns of shopkeepers and builders. It would have been easy for either Chambers or Hume to imagine that the scope of mixed mathematics was well indicated by the first two books of Newton's *Principia*, and that could have led either of them to suppose that the inferences of mixed mathematics were always demonstrative.[32] Be that as it may, Hume was still in position to recognize the probabilistic character of some practical mathematics, in particular less lofty applications of mathematics, which are useful "in almost every art and profession" (T 2.3.3.2; SBN 413). Such inferences were around long before the formulation of physical laws. They proceed, as we have suggested, from an observational premise to a factual conclusion in accordance with a mathematical rule. Thus, they are probable inferences that draw their force from the understanding as much as from experience. Had Hume paid attention to such cases, they could have had an enormous impact on the doctrine of the *Enquiry*.

Conclusion

Inferences *from* matters of fact *in accordance with* mathematical rules are still probable inferences; and so they are liable to deliver a surprise now and then. But there are crucial differences between applied mathematical inferences and the kind Hume typically uses to illustrate reasonings concerning matter of fact:

> When I see, for instance, a Billiard-ball moving in a straight line towards another; even suppose motion in the second ball should by accident be suggested to me, as the result of their contact or impulse; may I not conceive, that a hundred different events might as well follow from that cause? May not both these balls remain at absolute rest? May not the first ball return in a straight line, or leap off from the second in any line or direction? All these suppositions are consistent and conceivable. Why then should we give the preference to one, which is no more consistent or conceivable than the rest? All our reasonings a priori will never be able to show us any foundation for this preference. (EHU 4.10; SBN 29–30)

Probable inferences in accordance with a mathematical rule are at odds with Hume's claim that there are countless other conclusions which "to reason, must seem fully as consistent and natural."

Suppose we have erected two posts of equal length, 10 feet apart and at right angles to the floor. If it is required to cut a length to fit between the two posts, level with both their tops, mathematics tells us to cut a 10-foot length. It is imaginable, of course, that the 10-foot length is too long or too short just to bridge that gap. We can conceive easily enough being an inch or two off. And because reason dictates that this result is inconsistent and unnatural, we would infer that either an earlier measurement must have been mistaken or that the geometrical figure on which the inference was based must not have resembled the empirical situation closely enough (perhaps the floor was not flat). In either case we would re-measure, with greater care, before cutting another length. But is it conceivable, as Hume suggests, that a 6-foot length or a 14-foot length does the job? If it turned out that the 6-foot length just fits the space, we would not write this off as an illustration of the thesis that all events are entirely loose and separate (cf. EHU 7.26; SBN 74). Instead we would complain that something very peculiar is happening. Most likely someone is playing a trick, or the measurements have been made under bizarre conditions. Perhaps the tape measure has been tampered with, or perhaps the floor is subject to abrupt changes in dimension. We may not be able to explain the anomaly, but we would not relinquish our belief that there is an intelligible explanation, viz. one that doesn't conflict with our disposition to let mathematical rules be our guide.

It won't do to object that the judgments of inconsistency or unnaturalness simply reflect a history of successful applications of mathematics. For, expectations generated by applied mathematics are often subverted. When the amount in my check register doesn't match the amount in the bank, I search diligently until I find the mistake. I don't inform the bank officer that relations of ideas provide no assurance concerning matters of fact. When a prediction from a scientific hypothesis is contradicted by observation, the scientist knows that either one of the measurements is wrong or that the theory (often a mathematical model) needs to be reconfigured. This remains substantially true when the prediction is based on a probabilistic model (see appendix). A prediction that is not borne out by the course of experience is indicative of a biased sample, a mistaken measurement or the selection of a very unusual sample. In all these cases the mathematics in accordance with which the inference is drawn *informs* the reasoner where he needs to look in order to correct and better control his inference. Thus he is directed to act in a particular way, and he finds little consolation in Hume's dictum that only custom can assure us that the future will resemble the past.

Hume should allow that reason and observation contribute equally to applied mathematical inference. Moreover, he should recognize that the vivacity imparted to the conclusions of such inferences is not always due to past or present experience with the objects involved in the inference. Experience can, indeed, be at odds with the dictates of applied mathematics, as the discussion of two ideas of the sun in Descartes' third meditation reminds us: One is due to "natural impulse" while the other, based in applied mathematics, is due to "the natural light."[33] The vivacity of the latter overrides, and rightfully overrides, the vivacity of the former.

APPENDIX: PROBABILISTIC MODELS

In Hume's day mathematical probability was not developed sufficiently to provide rules in accordance with which inductive inferences could be drawn. Owing to ideas of Laplace (early nineteenth century) and their subsequent development, such models are now familiar fare. For example, whenever one hears the outcome of an opinion poll expressed in the form "the survey is accurate within ±n%" the conclusion is drawn in accordance with a rule of mathematics, the Central Limit Theorem.[34] Discussion of such inferences will clarify and strengthen the significance of treating applied mathematics as a further type of probable inference. Since the mathematical tools gained currency only in the twentieth century, these remarks do not bear directly on a Humean philosophy of applied mathematics. They are relevant, however, to philosophical assertions that developments in probability make no difference to Hume's position on induction. For example,

> It deserves mention that David Hume, who was the first to see that general synthetical propositions cannot be proved *a priori*, also clearly apprehended that this result of the impossibility of foretelling the future cannot be 'evaded' or 'minimised' by reference to probability.[35]

and more recently,

> To conclude: by invoking only points that he made in his discussion of probability or in his own original argument, Hume would have been able to defend his argument against attempts to defuse it by appeal to those conceptions of probability which have been developed since his time.[36]

On the contrary, some inductions have a rational basis insofar as they are instances of applied mathematics. That some inductions have a rational basis is not a new insight.[37] But what hasn't been appreciated before is that the arguments of Williams, Stove, and others concern only a special case of applied mathematics.

To understand the type of inference with which we are concerned, it is useful to compare it to an inference from sample to sample:

> Of voters questioned over a long period of time, 66% disapprove of Mr. Shrub.
>
> ∴ Of 100 voters questioned in the future, 66 will disapprove of Mr. Shrub.

It's reminiscent of an example from Hume's "Probability of Causes":

> Suppose . . . I have found by long observation, that of twenty ships, which go to sea, only nineteen return. Suppose I see at present twenty ships that leave the port: I transfer my past experience to the future, and represent to myself nineteen of these ships as returning in safety, and one as perishing. (T 1.3.12.11; SBN 134)

Likewise, the "long observation" that 66% questioned disapprove of Shrub generates a habit of expecting that the same proportion will disapprove of Shrub in the future. The warrant for this inference is the supposition that the future resembles the past, which has no basis in reason. The situation is quite different for a related inference from sample to population:

> 66% of 1600 randomly questioned voters disapprove of Shrub.
>
> ∴ 66 ± 2.5% of all voters disapprove of Shrub.

For, this inference can be drawn in accordance with a mathematical rule. Laplace articulated the rule ca. 1812, but it is easier to understand it in contemporary garb. The rule states that the set of large ($n \geq 30$) n-fold random samples of a parent population is normally distributed with mean proportion π(where π is the proportion of the parent population with the characteristic in question) and standard deviation $\sqrt{\frac{\pi(1-\pi)}{n}}$.[38] Since more than 95% of a normal distribution lies within two standard deviations of the mean, it follows that more than 95% of the samples of size 1600 lie within 2.5% of the true proportion.[39] In accordance with this rule, it follows from the observation that 66% of voters questioned disapprove of Shrub that 66 ± 2.5% of all voters disapprove of Shrub. This conclusion holds *unless* the sample is not random *or unless* a very unusual random sample has been drawn.

There is an interesting analogy but also an enlightening disanalogy between the second Shrub example and Hume's example of a die marked with one figure on four sides and another figure on the remaining two. According to Hume, we expect more strongly that the figure that appears on one of the four sides will show uppermost because

> 'Tis evident that where several sides have the same figure inscrib'd on them, they must concur in their influence on the mind, and must unite upon one image or idea of a figure all those divided impulses. (T 1.3.11.13; SBN 129. Cf. EHU 6.3; SBN 57)

Here the degree of belief in the conclusion depends upon the weight of experience, which arises from reviewing (visually or in imagination) *every* side of the die. On the one hand, the situation is analogous for our degree of belief in the proposition that 66 ± 2.5% of all voters disapprove of Shrub. For we understand that there are two groups of samples, one consists of samples that *do* resemble the original population, and the other of samples that *don't*. The first group comprises 95% of the samples, and so it is analogous to the four faces sharing the same figure on Hume's die. Just as we expect more strongly that one of *these* sides will appear uppermost, so we expect even *more* strongly that a sample from the resembling group will be selected. On the other hand, there is an important disanalogy between the die and a set of possible samples, for there is no *experience* of reviewing all the samples. The resembling samples concur, apparently, in their influence on the mind, but in a fashion Hume did not anticipate. There is no such experience, because we are not perceptually acquainted with this population (or hyper-population).[40] Nevertheless, once a random sample is actually drawn from the population of voters, the mind is influenced by the superiority of chances—that is, by the mathematical proposition that the vast majority of large random samples resemble the parent population—to expect strongly that the proportion of sampled voters who disapprove of Shrub is close to the proportion of all voters who disapprove of Shrub.

Expecting that the population proportion resembles the sample proportion is a natural reaction of someone who understands the mathematics and believes the sample is random. Likewise, when Hume discusses the die with the same figure on four sides, he, too, observes a natural reaction. Both cases involve a *census* of possible outcomes (that is, a review of *all* the possibilities). But one census is accomplished through observation while the other is accomplished through reason. The crucial point is that the census influences the mind, whether it is established as a matter of fact or as a relation of ideas. Thus, the examples of the die and the random sample of voters differ from Hume's example of ships leaving port. The observation of the ships is neither a census nor a random sample from a hyper-population of samples for which a mathematical census is available. For this reason, the habit generated by long observation that of twenty ships which go to sea only nineteen return is more subject to bias. If *that* inference is a natural reaction, it is an overly hasty one.

It ignores the possibility that the twenty that just left port differ systematically from the long observed population of ships leaving port.

NOTES

1 The reference is to David Hume, *Enquiries concerning the Human Understanding and concerning the Principles of Morals*, ed. Tom Beauchamp (Oxford: Oxford University Press, 1999), or "EHU," with references to sections and paragraph numbers, followed by page numbers in *Enquiries Concerning the Principles of Human Understanding and Concerning the Principles of Morals*, ed. L. A.Selby-Bigge, revised by P. H. Nidditch (Oxford: Clarendon Press, 1975), abbreviated "SBN" in the text.

2 Curiously, the oldest records we have of applied mathematics are in the context of religious practice. See Abraham Seidenberg, "The Origin of Mathematics," *Archive for History of Exact Science* 18 (1977): 301–42.

3 The reference is to David Hume, *A Treatise of Human Nature*, ed. David Fate Norton and Mary Norton (Oxford: Oxford University Press, 2000), or "T," with references to Book, part, section, and paragraph numbers, followed by page numbers in *A Treatise of Human Nature*, ed. L. A.Selby-Bigge, 2nd ed., revised by P. H. Nidditch (Oxford: Clarendon Press, 1978), abbreviated "SBN" in the text.

4 See Antony Flew's *Hume's Philosophy of Belief* (London: Routledge and Kegan Paul, 1961), review of *Hume: Precursor of Modern Empiricism* by Farhang Zabeeh, *Ratio* 5 (1963): 226–27, and "Did Hume Distinguish Pure from Applied Geometry?" *Ratio* 8 (1966): 96–100. Zabeeh denies that the *Enquiry* includes the pure/applied distinction. See Farhang Zabeeh, *Hume: Precursor of Modern Empiricism* (The Hague: Martinus Nijhoff, 1960), 142–43, and "Hume on Pure and Applied Geometry," *Ratio* 6 (1964): 185–91. But Zabeeh struggles with Hume's assertion that the truths of geometry would hold even if there were no circles or triangles in the universe (EHU 4.1; SBN 25).

5 A distinction between applied and mixed mathematics will be drawn and discussed below.

6 See, for example, Alfred Jules Ayer, *Language, Truth, and Logic* (New York: Dover Publications, 1952), 31.

7 A formally valid schema is a schema that yields truths no matter what predicates are substituted for the schematic letters. For example, "Every FG is F," which yields sentences like "Every white horse is a horse" or "Every red ball is a ball," is such a schema.

8 See Albert Einstein, *Ideas and Opinions* (New York: Crown Publishers, 1954), 233. Cf. Flew, *Hume's Philosophy of Belief*, 63.

9 See John Passmore, "Descartes, the British Empiricists, and Formal Logic," *Philosophical Review* 62 (1953): 545–53; and David Owen, *Hume's Reason* (Oxford: Oxford University Press, 1999), chaps. 2–3. See also EHU 12.27 (SBN 163).

10 See Ian Mueller, "Euclid's Elements and the Axiomatic Method," *British Journal for Philosophy of Science* 20 (1969): 289–309, 298–99.

11 In fact, Euclid avoids formal inference even when it would shorten his proof. See Thomas L. Heath, *The Thirteen Books of Euclid's Elements* (New York: Dover, 1956), I: 256.

12 John Locke, *An Essay concerning Human Understanding* (Oxford: Clarendon Press, 1975), 4.1.2, 525. The reference is to book, part, and section numbers, followed by page numbers in that edition.

13 The same difficulty exists, in exaggerated form, with Owen's proposed demonstration that $3467 = 2895 + 572$; it takes a chain with 572 links. See Owen, *Hume's Reason*, 95ff. Locke seems to be aware of this problem; see Locke, 4.1.9, 528–29.

14 For reasons that will emerge in the next section, figure 2 is purposely inaccurate.

15 The triangles ADC and BDC are, perhaps, intermediate ideas in Locke's original sense. Yet if one were interested only in showing their congruence, it would still be necessary to infer it in accordance with the side-angle-side rule.

16 See, for instance, Robert Fogelin, "Hume and Berkeley on the Proof of Infinite Divisibility," *Philosophical Review* 97 (1988): 47–69; and Vadim Batitsky, "From Inexactness to Certainty: the Change in Hume's Conception of Geometry," *Journal for General Philosophy of Science* 29 (1998): 1–20.

17 For example, instead of assuming an axiom of continuity (if an area is separated into two regions by a line, then any path between the regions must intersect that line) Euclid simply relies on his diagram to assure himself that such a point of intersection exists.

18 This is true even when we perform the constructions "in our heads," for such constructions are but imaginations of constructions performed on paper or a plot of ground.

19 See George Berkeley, *A Treatise concerning the Principles of Human Knowledge*, ed. Jonathan Dancy (Oxford: Oxford University Press, 1998), §16; and EHU 12.20n (SBN 158n).

20 See David Sherry, "Don't Take Me Half the Way: On Berkeley on Mathematical Reasoning," *Studies in History and Philosophy of Science* 24 (1993): 207–25, and "Construction and Reductio Proof," *Kant-Studien* 90 (1999): 23–39.

21 By hypothesis, $\angle CDA = \angle BCA$. So, $\angle CDA + \angle CAD = \angle BCA + \angle CAD$. Therefore, the remaining angles, $\angle CBA$ and $\angle ACD$, in ΔCBA and ΔACD, respectively, are equal. But then $\Delta CBA \sim \Delta ACD$ (angle-angle). Likewise, since $\angle CBA = \angle ACD$, $\Delta ACD \sim \Delta CDB$ (angle-angle). Annotations indicating equality or right angularity are inessential, being used only to keep track of which stipulations are used in a particular proof.

22 Dale Jacquette, *David Hume's Critique of Infinity* (Leiden: Brill, 2001), 177–78.

23 This idea is articulated at length in Stephan Körner, *Philosophy of Mathematics* (New York: Dover Publications, 1960) and *Experience and Theory* (New York: Humanities Press, 1966). In the earlier work Körner uses "idealization" in place of "identification." Since Körner has a reputation as an interpreter of Kant, the reader may suspect that I am

interpreting Hume as a proto-Kantian. But Körner argues that Kant's philosophy of applied mathematics is no longer plausible (*Mathematics*, 143–44). Moreover, he explicitly denies Kant's distinction between pure and empirical intuition (ibid., 170–71). Körner introduces identification to deal with a problem that infects many philosophies of applied mathematics, including Kant's, viz., the failure to bridge the divide between the exact concepts of mathematics and the inexact concepts that we apply to the empirical world (ibid., 176ff.). In a move that would appeal to Hume, Körner argues that there is no *logical* connection between the exact and the inexact. Thus, he rejects entirely the Kantian doctrine that the objective validity of mathematics can be established a priori (ibid., 180).

24 This account is the reverse of Flew's, which proposes that mathematical objects and operations be replaced by physical objects and operations.

25 As Hume sees matters, the inexactness of the empirical world makes no difference to applications of arithmetic. In both the *Enquiry* and the *Treatise* arithmetic is an exact science owing to an exact idea of equality, viz., one-to-one correspondence (T 1.3.1.5; SBN 71), which can be established as easily between impressions as between ideas. Thus an inference of applied arithmetic is no different from an inference of pure arithmetic because there is no difference (with respect to numerical equality) among the objects of the inferences. This position is not without difficulty, however, because many instances of counting require us to rule out borderline cases. Suppose, for example, that you are counting people in several rooms with an eye to determining a total by addition. How will you treat individuals who come only part way into the room? In order to apply arithmetic you will have to rule out these borderline cases by excluding or including them; this amounts to treating "person in the room" as an exact concept, even though it is normally an inexact, empirical concept.

26 Covering an area with congruent square tiles and counting them could accomplish this, of course.

27 See Peter Millican, "Hume's Sceptical Doubts concerning Induction," in *Reading Hume on Human Understanding*, ed. Peter Millican (Oxford: Oxford University Press, 2002).

28 It is the province of measurement theory to study rigorously the relations between quantitative concepts and the mathematical structures used to represent them. To show rigorously that an empirical relational structure, say the structure consisting of the set of physical objects and the relations "x is at least as massive as y" and "x and y together are at least as massive as z," can be represented by a particular mathematical structure, say $\langle \Re^+, x \geq y, x + y \geq z \rangle$, one proves a representation theorem. My claim here is that the conditions of the representation theorem amount to an idealization (or exactification) of the quantitative concepts involved in the momentum argument. For a popular exposition of measurement theory, see Joel Michell, *Measurement in Psychology* (Cambridge: Cambridge University Press, 1999), chap. 3.

29 It is not just the quantitative concepts that are presumed to be exact. Rigidity, too, must be treated as exact or ideal in order to avoid complications arising from deformation of the bodies.

30 See Isaac Newton, *Newton's Principia*, trans. Andrew Motte, rev. Florian Cajori (Berkeley and Los Angeles: University of California Press, 1934), 62.

31 Ephraim Chambers, *Cyclopædia: or, an universal dictionary of the arts and sciences*, 2nd ed. (London: printed for J. and J. Knapton and 18 others, 1728), II: 509.

32 As noted earlier, Millican, too, neglects to distinguish mixed from applied mathematics. His primary concern in this discussion is to show that Hume recognizes demonstrative arguments from a posteriori premises, and I agree with that point. However, both Hume and Millican are guilty of overlooking the step of idealization that makes possible that sort of demonstrative reasoning. By conflating mixed and applied mathematics Millican can claim that *every* applied mathematical inference is founded upon the experience of constant conjunction rather than reason alone, since applied mathematics presupposes physical laws, which are founded upon the experience of constant conjunction (Millican, "Hume's Sceptical Doubts," 126). This claim is problematic, not only because mixed mathematics depends on idealizing the components of physical laws, but more importantly because any inference that uses measurements (instead of stipulations) cannot guarantee its conclusion.

33 René Descartes, *Discourse on Method and Meditations on First Philosophy*, ed. Donald Cress, 4th ed., (Indianapolis: Hackett, 1998), 72–73.

34 Jay Devore and Roxy Peck, *Statistics*, 5th ed. (Belmont, CA: Brooks-Cole Publishing, 2005), 345–47.

35 Georg H. von Wright, *The Logical Problem of Induction* (New York: Macmillan, 1957), 153.

36 Dugald Murdoch, "Induction, Hume, and Probability," *Journal of Philosophy* 99 (2002): 185–99, 199.

37 See Donald Williams, *The Ground of Induction* (Cambridge, MA: Harvard University Press, 1947). According to Stove, Laplace himself defends that thesis. See David Stove, *The Rationality of Induction* (Oxford: Clarendon Press, 1986), 55.

38 The theorem holds as long as $n < 10\%$ of the entire population, $n\pi \geq 10$, and $n(1-\pi) \geq 10$. See Devore and Peck, *Statistics*, 355.

39 It is not a problem that π is unknown, since σ will be a maximum for $\pi = .5$. Thus, $\sigma \leq \sqrt{\frac{(.5)(.5)}{1600}} = .0125$, and $2\sigma \leq .025 = 2.5\%$.

40 See Williams, *Ground of Induction*, 93ff. The hyper-population is a population of random *samples* of voters; it is not a population of voters. There are far more members in the hyperpopulation of samples of voters than there are members in the original population of voters.