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Analogy

John D. Norton
Department of History and Philosophy of Science
University of Pittsburgh
http://www.pitt.edu/~jdnorton

1. Introduction

Reasoning by analogy is a venerable form of inductive inference and was recognized already millennia ago by Aristotle. Over these millennia it has been the subject of persistent analysis from the perspective of formal approaches to inductive inference. The goal has been to find the formal criteria that distinguish good from bad analogical inference. These efforts have met with mixed success, at best.

As we shall see below, the difficulties these efforts have faced are similar to those facing the formal explication of other sorts of inductive inference. If analogical reasoning is required to conform only to a simple formal schema, the restriction is too permissive. Inferences are authorized that clearly should not pass muster. This familiar problem is illustrated below in the case of a generic account of analogical inference, drawn from the older literature and described in Section 2. This is Joyce's (1936, p. 260) account, which I label "bare analogy" to reflect its simplicity. It has long been recognized that bare analogy authorizes too many inferences. This failure and its long-standing recognition is recounted in Section 3.

The natural response has been to develop more elaborate formal templates that are able to discriminate more finely since they capture more details of various test cases. Two elaborations are recounted here. Section 4 reviews Hesse's two-dimensional account, which is in turn derived from an analysis by Keynes. Section 5 reviews Bartha's articulation model. It was designed to remedy the shortcomings of Hesse's account by still further elaborations. Section 6 describes how these elaborations cannot escape the inevitable difficulty. Their embellished schema are never quite embellished enough. There is always some part of the analysis that must be handled intuitively without guidance from strict formal rules.

Section 7 turns to the material approach. According to it, the continuing expansion of the schema of the formal approach is inevitable since, according to the material approach, there is no single formal schema that can embrace all cases. As one tries to find schema that fit a growing body of cases better, the schema must introduce further distinctions and elaborations; and it must do so without end. For there are always new instances to be accommodated and a need for schema that fit more closely.

That the material approach is a better way to understand analogies and analogical inference in science is indicated by a curious divergence between the philosophical literature and the scientific literature. The philosophical literature categorizes analogy as a *form of inference* to be analyzed using some version of the formal methods of logical theory. The scientific literature approaches analogies as *factual* matters to be explored empirically; or at least it does so for the important analogies that figure centrally in the sciences. For the scientists, there are many inferences associated with the analogy. But the analogy itself is a factual matter.

This gap between the philosopher and the scientist is hard to close if we approach inductive inference formally. If, however, we take a material approach to inductive inference, the gap closes automatically and the difficulties faced by the formal approach evaporate. We no longer need to display some universal schema that separates the good from the bad analogical inferences. Rather an analogical inference is good just in so far as there is a warranting fact to authorize it. Each warranting fact can be identified on a case by case basis without the need for it to conform with some elaborate template. That warranting fact is the factual analogy that scientists pursue empirically.

Sections 8, 9 and 10 illustrate the material approach with three cases of analogies in science: Galileo's discovery of mountains on the moon, the Reynolds analogy in fluid flow and the liquid drop model of the atomic nucleus. Section 11 presents general conclusions. An appendix provides technical details of the Reynolds analogy and a little of its history.

2. Bare Analogy

Argument by analogy has long been a standard in the inventory of topics of logic texts in the older tradition. It is specified formally in terms drawn ultimately from syllogistic logic. Joyce (1936, p. 260) states it as:

> S_1 is P.
>
> S_2 resembles S_1 in being M.
>
> [therefore] S_2 is P.

Mill (1904), Book III, Ch. XX, §2) gives an equivalent characterization in words:

> Two things resemble each other in one or more respects; a certain proposition is true of the one, therefore it is true of the other.

This simple argument form has proven quite fertile in the history of science. Galileo observed shadows on the moon that resembled the shadows of mountains on the Earth in both their shape and motion. He pursued the resemblance to posit that there are mountains on the moon and to determine their height. Darwin's celebrated argument in the early chapters of *Origin of Species* exploits an analogy between domestic selection by breeders and the selective processes arising in nature. Gravity and electricity resemble one another in being forces that act between bodies or charges, diminishing in strength with distance. So in the eighteenth century, it was natural to expect that the analytic methods Newton developed for gravity might apply to electricity as well, even issuing in an inverse square law. Two more fertile analogies will be developed in more detail below: analogies among transport phenomena, notably the Reynolds analogy; and the analogy between an atomic nucleus and a liquid drop.

3. Its Failure

In spite of this record of success, descriptions of the argument form also routinely concede its inadequacy. Joyce (1936, p. 260) insists that the scheme he had just described has further hidden conditions.

> The value of the inference here depends altogether on the supposition that there is a causal connexion between M and P. If this be the case, the inference is legitimate. If they are not causally related, it is fallacious; for the mere fact that S_2 is M, would then give us no reason for supposing that was also P.

This amounts to a gentle concession that the formal scheme laid out is not able to separate the good from the bad analogical inferences. The addition, the fact of a causal connection, lies well outside the vocabulary of syllogistic logic in which this argument form is defined. That vocabulary is limited to individuals and properties and assertions about them using "not," "Some…" and "All…" For example: "Some As are not B."

Recalling classic examples of the failure of analogical reasoning shows us that this pessimistic appraisal is still too optimistic. The depressions Galileo found in the moon's surface resemble terrestrial seas. But there are no water filled seas on the moon's surface. Lines on the surface of Mars resemble terrestrial canals. But there are no such canals on Mars. Fish and whales resemble one another in many of their features. But one extends the resemblance at one's peril. Whales are mammals, not fish, and do not breathe through gills or lay eggs. In the eighteenth and early nineteenth century, heat was found to flow like a fluid from regions of higher heat density (that is, higher temperature) to those of lower heat density. Pursuit of the resemblance leads one to conclude that heat is a conserved substance. That heat is not conserved, but is convertible with work, was shown by the mid 19th century by Joule and others. Studies by Clausius, Maxwell and Boltzmann showed that heat is not even a substance in its own right. It is really a disorganized distribution of energy over the very many components of other substances. In the nineteenth century, the wave character of light was reaffirmed. In this aspect it resembles the wave motions of sound or water waves. Since both these waves are carried by a medium, the air or water, analogical reasoning leads to the positing of a corresponding medium for light, the ether. The positing of this medium fared poorly after Einstein introduced relativity theory.

We see through these examples that formally correct analogical inferences frequently yield false conclusions. Joyce's added requirement of a causal connection is not sufficient to reveal the problems of the analogical failures just listed. Water on the moon or Mars would be causally connected with seas and canals. The property of surviving underwater is causally connected with having gills. The passage of heat from regions of higher to lower temperature is causally connected with the heat as a substance and temperature measuring its concentration. The wave motion of light is causally connected with the supposed medium that carries the waves.

We may want to discount these sorts of failure as a familiar artifact of inductive inference in general. When one infers inductively one always takes an inductive risk and inevitably, sometimes, we lose the gamble. The frequency with which we lose the gamble has supported a more pessimistic conclusion on analogical inference in science (Thouless, 1953, Ch. 12):

> Even the most successful analogies in the history of science break down at some point. Analogies are a valuable guide as to what facts we may expect, but are never final evidence as to what we shall discover. A guide whose reliability is certain to give out at some point must obviously be accepted with caution. We can never feel certain of a conclusion which rests only on analogy, and we must always look for more direct proof. Also we must examine all our methods of thought carefully, because thinking by analogy is much more extensive than many of us are inclined to suppose.

This unreliability of analogical reasoning is a fixture of handbooks of logic. They commonly have sections warning sagely of the fallacy of "false analogy." The reader is entertained with numerous examples of conclusions mistakenly supported by analogies too weak to carry their weight. The difficulty with these accounts is that the falsity of the analogy is only apparent to us because we have an independent understanding of the case at hand. There is little beyond banal truism to guide us away from false analogies when the difficulty was not already obvious at the outset.[1] Merely being warned to watch for weak analogies is unlikely to have helped an early nineteenth century scientist who infers that light waves must be carried by a medium, as are other waves; or that heat is a fluid since it resembles one in so many features. Until further empirically discovered facts are considered, these analogies seem quite strong.

After reviewing many examples of successful and unsuccessful analogies, Jevons (1879, p. 110) comes to a sober and cautious conclusion:

> There is no way in which we can really assure ourselves that we are arguing safely by analogy. The only rule that can be given is this, that the more closely two things resemble each other, the more likely it is that they are the same in other respects,

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[1] Bartha (2010, p. 19) has performed the useful service of collecting a list of eight "commonsense guidelines." They include: "(CS1) The more similarities (between the two domains), the stronger the analogy." "(CS3) The greater the extent of our ignorance about the two domains, the weaker the analogy." "(CS5) Analogies involving causal relations are more plausible than those not involving causal relations."

> especially in points closely connected with those observed . … In order to be clear about our conclusions, we ought in fact never to rest satisfied with mere analogy, but ought to try to discover the general laws governing the case.

Once one has been steeped in the literature on analogical reasoning and has sensed both its power and resistance to simple systematization, it is easy to feel that Jevons' rule is not such a bad outcome, in spite of its vagueness. It is a good tonic, therefore, to recall what successful rules look like in deductive logic. Modus ponens[2] is a valid inference, always. Affirming the consequent[3] is a deductive fallacy, always. We should take this as a warning. That our rules need to be protected by vagueness and ambiguity may be an alert that there is no precise rule to be found.

4. Two-Dimensional Analogy: Hesse's Account

If a formal account of analogical inference is to succeed, it will need to be significantly richer than the schema of bare analogy just discussed. There have been important efforts in this direction. The most successful and the most promising of these richer accounts is due to Mary Hesse and, more recently, Paul Bartha. First I will sketch the central, common idea of the account and then give a few more details of Hesse's and Bartha's versions.

An analogical inference passes from one system to another. Following Bartha (2010, p. 15), I will call the first the "source" and the second the "target." A successful analogical inference, in this richer account, does not just pass a property from the source to the target. It passes a relation over the properties of the source to the analogous relation over the properties of the target. The source may carry properties P and Q where P and Q stand in some causal, explanatory or other relationship. If the target carries a property P* analogous to P, the analogical inference authorizes us to carry over the relation to the target system, where we now infer to a property Q* that stands in the same causal or explanatory relation to P*. This is the crucial enhancement. This relation makes it reasonable to expect that, if the target system carries P*, then it also carries Q*. I call this approach "two dimensional" because we have relations

[2] If A then B; A; therefore B.

[3] If A then B; B; therefore A.

extending in two dimensions: there are relations contained within each of the source and the target systems; and there are the relations of similarity between the two systems.

Hesse's (1966) study of models and analogies in science provided a fertile tabular picture in which the two dimensions are arrayed vertically and horizontally. Hesse gave tables illustrating particular examples. Bartha (2010, p.15) extracts the general schema as

Source	Target	
P	P*	(positive analogy)
A	~A*	(negative
~B	B*	analogy)
Q	Q* (plausibly)	

The first column indicates the properties carried by the source and the second indicates those carried by the target. Properties corresponding under the analogy are indicated by adding an asterisk. The property P* in the target corresponds to P in the source.

The table indicates the introduction of the terms "positive analogy" and "negative analogy," drawn originally from Keynes (1921, Ch. XIX). The positive analogy is the properties on which the source and target agree; the negative analogy is the properties on which they disagree. Establishing possession of the as yet unaffirmed property Q* by the target is the goal of the analogical inference. The table does not indicate the relations obtaining in the two dimensions, the vertical and the horizontal. They are specified by Hesse (1966, p. 59) as: "…horizontal relations will be concerned with identity and difference… or in general with *similarity* and vertical relations will, in most cases, be *causal*."

The general sense is that the strength of support for this conclusion depends on a trade-off between the positive and negative analogy. The stronger the positive analogy, the more the conclusion is favored; but the stronger the negative analogy, the more the conclusion is disfavored. However I have found no simple formula or simple synoptic statement in Hesse's text for how this balance is to be effected. In discussing a particular example, however, Hesse (1966, pp. 58-59) gives guidelines for a particular case. These guidelines can be generalized by the simple expedient of suppressing the particulars of the case by ellipses and the substitution of symbols in order to simulate a general schema.[4] We recover:

> The validity of such an argument will depend, first, on the extent of the positive analogy compared with the negative … and, second, on the relation between the new property and the properties already known to be parts of the positive or negative analogy, respectively. If we have reason to think that the properties in the positive analogy are causally related, in a favorable sense, to [Q], the argument will be strong. If, on the other hand, the properties of the [target] which are parts of the negative analogy tend causally to prevent [Q*] the argument will be weak or invalid.

If any general schema is intended by Hesse, it must be this or something close to it. There is considerably more discussion in Hesse's text, but I find it mostly inconclusive. The chapter "Logic of Analogy" (p.101) is devoted to the question of whether the presence of an analogy makes it reasonable to infer to some new property of the target system. "Reasonable" is given a weak reading only; it amounts only to the comparative notion of one hypothesis being more reasonable than another. Grounding for the comparative judgment is then sought in several then extant approaches to evidence, with largely negative results.

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[4] The unedited quote reads:

> Under what circumstances can we argue from, for example, the presence of human beings on the earth to their presence on the moon? The validity of such an argument will depend, first, on the extent of the positive analogy compared with the negative (for example, it is stronger for Venus than for the moon, since Venus is more similar to the earth) and, second, on the relation between the new property and the properties already known to be parts of the positive or negative analogy, respectively. If we have reason to think that the proper- ties in the positive analogy are causally related, in a favorable sense, to the presence of humans on the earth, the argument will be strong. If, on the other hand, the properties of the moon which are parts of the negative analogy tend causally to prevent the presence of humans on the moon the argument will be weak or invalid.

5. Bartha's Articulation Model

Bartha (2010, pp. 40-46) mounts a careful critical dissection of Hesse's theory that reveals its problems and short-comings. Bartha's own theory is the best-developed account of analogy I have found in the philosophical literature. It sets out to resolve the problems of Hesse's account and is based on an extension of Hesse's two-dimensional approach (p. 35). The goal of Bartha's (2010, Ch. 4) "articulation model" is to enable a judgment of the plausibility of an analogical inference. The term "plausibility" is itself employed as a term of art and is given two explications, probabilistic and modal (pp. 15-19). The articulation model proceeds with the vertical and horizontal relations of Hesse's two-dimensional model. However the bulk of Bartha's analysis is devoted to the vertical relations and it greatly extend those of Hesse. Instead of merely requiring that the properties of the source system are causally related, Bartha allows four different sorts of vertical relations among these properties: they may be predictive, explanatory, functional or correlative. The first two come in deductive and inductive forms. The final two come only in inductive forms. Analogical inference carries these relations from the source to the target system.

The conditions for a successful analogical inference in the articulation model are elaborate. There are two general principles (p. 25): "prior association," which requires the existence of an explicit vertical relation that is to be extended by the analogical inference; and "potential for generalization," which requires "no compelling reason" that precludes extension of the prior associations to the target system. The formal specification of the model then approaches the judgment of plausibility in two stages. The first, "prima facie plausibility," requires the positive analogy to be relevant to the prior association and the absence of critically relevant factors in the negative analogy. The second stage assesses qualitative plausibility on the basis of three criteria: strength of prior association, extent of positive analogy and presence of multiple analogies.

The implementation of these two stages seems to differ according to the type of prior association. Further conditions become more clearly articulated, as the implementation proceeds. For example, in the discussion of "predictive/probabilistic analogies," (pp. 120-21) it turns out that there are five important determinants of plausibility: strength of prior association, extent of correspondence, the existence of multiple favorable analogs, only non-defeating completing analogs and only non-defeating counteracting causes. Perhaps the most difficult case is that of multiple analogies. Its treatment requires a formal extension of the original theory. A ranking relation "is superior than" is introduced as a partial ordering on the set of analogical arguments at issue. There is much more to explore in Bartha's richly elaborated account. However, sufficient of both accounts has been developed here for me to indicate why I think a different approach is preferable.

6. Problems of the Two-Dimensional Approach

Hesse's and especially Bartha's analyses of analogy are impressive for their care and detail; they significantly enrich the original formal notion of bare analogy. In particular, Bartha is surely correct to refocus attention on the vertical relations within each of the source and target, as opposed to the horizontal similarity relations between them. For these vertical relations matter more—or so I shall argue below. If a formal analysis of analogical inference can succeed, this is likely the right direction. However, my view is that they are proceeding in the wrong direction. What was wrong with the bare notion of analogy was precisely that it tried to treat some inductive inferences formally rather than materially, and the resulting simple schema fitted poorly. The two-dimensional approach seeks to tighten the poor fit by including more formal apparatus. Yet each new formal notion brings with it further problems, compounding the difficulties and threatening an unending regress. Here are some of the problems.

Hesse strains to explicate in general terms even the simple notion of similarity that constitutes the horizontal relations. She does not favor "formal analogy," which refers to "the one-to-one correspondence between different interpretations of the same formal theory." (1966, p.68) The simple example is the analogy of a father to the state. The scientific example (whose details are not elaborated) is "the formal analogy between elliptic membranes and the acrobat's equilibrium, both of which are described by Mathieu's Equation." She continues: "This analogy is useless for prediction precisely because there is no similarity between corresponding terms." (p. 69) Instead she favors "material analogy," which are "pretheoretic analogies between observables." (1966, p.68) Examples of the favored material analogy are the analogy of the pitch of sound with the color of light; and the sphericity of the Earth with the sphericity of the Moon. These material analogies reduce the similarity relation to sameness of properties. The Earth and Moon are analogous in their sphericity since they carry the same property, sphericity.

While one can see the appeal of a limit to more secure material analogies, it is clearly overly restrictive. It disparages the fertile analogy between Newtonian gravity and Coulomb electrostatics, for example. It is a formal analogy in that it connects gravitational and electrostatic fields by virtue of their both satisfying the same field law (up to signs in the source term). There are other problems. A formal test that checks whether an analogy is material requires clear guides for when some term is "pretheoretic" and an "observable." There are many traps here. The analogy between pitch and color can be implemented only if we have numerical measures of pitch and color. Since these measures depend on a wave theory for both, are they still pretheoretic? Since they are inferred from measurements, are they observables?

Hesse's vertical relation is causality and it is similarly troubled. If we are to recover a serviceable, formal account of analogy, we must in turn have access to a serviceable formal account of causation. We must be able to confront each instance of a vertical relation with some formal criterion that tells us whether the relation is causal. Hesse's (1966, p. 87) summary is vague on just what is meant by causal relations. The vertical relations are "causal relations in some acceptable scientific sense…," which seems to suggest that discerning them is unproblematic. In this regard, Hesse seems unfazed by the plethora of candidate explications of causation that she lists. They include (1966, p.79) a Humean relative frequency account in which causation is co-occurrence; a hypothetico-deductive account, in which causal relations are delivered by some higher level law; a modal account in which causes are necessities; and an ontological account in which causes are productive. We can hardly expect each of these theories to agree in every application. We have to know which is the right theory and then how to apply it in a formal account. The length of Hesse's list already indicates the difficulty in clarifying causation. Some half century after her list was formulated, we are now even farther from the goal of a general, formal account of causation. For my own quite pessimistic appraisal, see Norton (2003).

Bartha's articulation model is designed to free Hesse's more limited model from arbitrary restrictions. However, if an account this complicated is what is needed for a successful formal treatment of analogy, we surely have reason to wonder if a formal analysis is the right approach. Our starting point was a simple and familiar idea. If systems share some properties, they may share others. This idea has been used repeatedly to good effect in science. As we pass through the various efforts to explicate the idea formally, we have arrived at a multi-stage procedure with many specializing components and trade-offs. Yet we are still not in possession of a fully elaborated formal schema. The trading off of many of the competing factors still seems to be effected at crucial moments by our inspection and intuitive judgment.

Rather than examining these problems in detail, I want to indicate one aspect of the articulation model that is directly relevant to the decision between a formal and a material approach to analogical inference. The vertical relations of the articulation model are characterized in inferential terms. When P and Q are related predictively, P entails Q. When P and Q are related through explanation, Q entails P so that P explains Q. The third and fourth functional and correlative relations are explicated similarly as inductive relations. Hence, in this model, an analogical inference passes a property, expressed in inferential terms, from the source to the target. That means the analysis is meta-logical, since the analogical inferences are performed at a higher, that is a "meta," level on lower level structures that are in turn characterized by inferential properties. This meta-logical character places a rather extraordinary burden on the articulation model. If it is to give a formal schema for analogical inference, it must provide a schema for the analogical parts of the inference at the meta-level, and also schemas for each of the lower level forms of inductive inference. In short, it must solve the formal problems of analogical inference and also every other form of inference it invokes.

The simple solution to the last problem is to approach inductive inferences materially. Then to note that one may infer inductively from P to Q requires that there is some factual relation between P and Q that authorizes the inference. That is all it requires, for there is no supposition of a universal schema. This factual relation is what is passed by the analogical inference, so that the amended model would lose its meta-logical character. Rather than pursuing this hybrid material/formal model, let us return to the full material approach.

7. Analogy in the Material Theory of Induction

In the material theory of induction, that there is an analogy between two systems is captured in a fact that may be merely conjectured or, better, may be explored empirically. This fact of an analogy then warrants an analogical inference, which is the passing of particular properties of the source system to the target. The precise character of the fact of analogy and precisely which properties may be passed will vary from case to case. There will be at best a loose similarity only between different analogical inferences in that, in all of them, we are authorized to pass properties from one system to another. There is no universal schema that can specify just which properties can be passed in which circumstances.

Hence, we should expect efforts to find a formal schema to face precisely the difficulties sketched in the last three sections. A simple formal schema will at best fit a range of cases imperfectly. Efforts to narrow the gap between the schema and the cases will require the proposal of more elaborate, more fragmented schemas. In an effort to capture a diversity not governed by any formal rule, they will need to divide the cases into a growing number of categories and subcategories. These refinements will allow a better fit, but the fit will never succeed perfectly for every case. We may eventually arrive at a formal system as elaborate as the articulation model, which, I have argued above, still falls short of the final, fully elaborated formal schema. No matter how complicated the successive proposals become, they will still never be adequate to all the cases. Gaps will remain.

There are two notions in the material analysis. The first is the fact of an analogy or just *fact of analogy*. This is a factual state of affairs that arises when two systems' properties are similar, with the exact mode of correspondence expressed as part of the fact. The fact is a local matter, differing from case to case. There is no universal, factual "principle of the uniformity of nature" that powers all inductive inference. Correspondingly, there is no universal, factual "principle of similarity" that powers analogical inference by asserting that things that share some properties must share others.[5] The fact of an analogy will require no general, abstract theory of similarity. The fact of analogy will simply be some fact that embraces both systems. There is no general template to which the fact must conform.

The second notion is an *analogical inference warranted by a fact of analogy*. Such an inference may arise if we know the properties of one system but not the other. We may then conjecture that there is a fact of analogy obtaining between the first system and the other system.

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[5] If one is tempted by a principle of similarity, note that every failure of an analogy is a counterexample to a simple statement of the principle. The real principle would separate the projectable similarities from the unprojectable, even if only statistically. Formulating such a principle amounts to the same problem as finding a formal theory of analogy, which, this chapter urges, is an insoluble problem.

This conjectured fact then becomes the fact that warrants the inference. If the conjectured fact is unequivocal and held unconditionally, the analogical inference from one system to another may simply be deductive, with all the inductive risk associated with the acceptance of the fact of analogy. In other cases, there will be some uncertainty or vagueness in the conjectured fact of analogy. The analogy is asserted as likely; or even merely possible; or that the particular way the analogy is set up might not be correct, but something like it might be. These hesitations confer an inductive character onto the inference warranted by the fact of analogy.

The fact of analogy must be able to power this inference. Since there is no "principle of similarity," the fact of analogy cannot merely assert some similarity between the two systems. It must assert a factual property of the second system that is sufficient to warrant the inference to its properties. For this reason, it will turn out that similarities between the two systems will be less important in the material analysis. Rather the similarities will appear more as conveniences of expression. It is cumbersome to specify how dark shapes on the moon appear as shadows of tall prominences when they obstruct linearly propagating sunlight. It is easy for Galileo to say that they are just like the shadows of mountains on the earth.

The material approach reorients our focus in two ways:

First, the focus will be on the fact of an analogy, for that controls the inferential connection between source and target systems. Moreover, it will turn out in the examples below that the fact of an analogy will tend less to express a brute similarity between source and target systems. It will tend to express a property that they share. The fact of possession of this property by the target system will drive the resulting inference, rather than similarity with the source.

Second, there will be no general formal principles sought to assess the strength or weakness of an analogical inference. Its strength will be assessed by examining the fact of analogy that warrants the inference. If we doubt the strength of the inference and wish to refine our assessment, we would not seek to refine and elaborate formal principles. We would not, for example, seek better guides on just how, as a matter of general principle, we should balance the competition of positive and negative analogies. We would instead engage in empirical investigations of the fact of analogy. Knowing more, the material theory asserts, enables us to infer better.

In the following, I will show these ideas are implemented in three cases of analogy. The first is Galileo's discovery of the mountains of the moon. The second and third are analogies that have played an important role in recent science: the Reynolds analogy for fluid flow and the liquid drop model of the atomic nucleus.

8. Galileo and the Mountains of the Moon

Galileo's (1610) *Siderius Nuncius*—the Starry Messenger—is an extraordinary document. In it Galileo reports the discoveries he made when he turned his telescope onto the heavens and observed systematically. One of the most striking was that the surface of the moon has mountains and valleys analogous to those on earth. The announcement of that discovery provided strong support to a major shift in scientific thinking then underway. The heavens, it was coming to be realized, were not the realm of immutable perfection but rather more like the earth. Here was observational evidence that the moon was not a perfect heavenly sphere after all, but resembled the craggy, pockmarked earth.

Galileo did not directly *see* mountains on the moon. Their presence was inferred from what he saw. He tracked the advancing division between light and dark on a waxing moon. His telescope showed him that its edge was not a smooth curve but an "uneven, rough and very wavy line." More important was the way it changed over time. As it slowly advanced, bright points of light would appear ahead of it. They would grow and soon join up with the advancing edge. Galileo finds the analogy to the illumination of mountains on earth irresistible. He exclaims (1610, p. 33):

> And on the earth, before the rising of the sun, are not the highest peaks of the mountains illuminated by the sun's rays while the plains below remain in shadow? Does not the light go on spreading while the larger central parts of these mountains are becoming illuminated? And when the sun has finally risen, does not the illumination of plains and hills finally become one?

Galileo is careful to exempt certain darker areas on the moon whose shading does not change with time. In so doing, he provides a positive summary of his conclusion concerning the shadows of the mountains (pp. 37-38):

> They [these other markings] cannot be attributed merely to irregularity of shape, wherein shadows move in consequence of varied illuminations from the sun, as indeed is the case with the other, smaller spots which occupy the brighter part of

> the moon and which change, grow, shrink, or disappear from one day to the next, as owing their origin only to shadows of prominences.

There is a similar analysis that identifies the depressions in the moon's surface that we now know as "seas."

Once secure in the conclusion that the moving dark shapes seen on the surface of the moon are shadows of mountains and valleys, Galileo proceeds to the most striking result (pp. 40-41). The higher the mountain, the farther ahead of the advancing edge that its peak will be illuminated. In some cases, Galileo noted, the peaks first appeared sometimes at more than one twentieth of the moon's diameter. This illumination, Galileo presumed, came from a ray of sunlight grazing tangent to the moon's surface at the edge of light and dark and then proceeding in a straight line to the mountain peak. These presumptions reduced computing the height of the mountain to the simple geometry of triangles. The result was a height of four miles for the largest mountain, which fares well against modern assessments.

Galileo's presentation of the analogy of earth and moon is compelling. However, from the perspective of the logic, the arguments are presented in fragments only and the reader is left to fill in the details. No doubt, once we undertake this exercise, different reconstructions of the logic will emerge. Here is one way of reconstructing it from the material perspective.

The controlling fact of the analogy is just this:

> The mode of creation of shadows on earth and of the moving dark patterns on the moon is the same: they are shadows formed by straight rays of sunlight.

This fact then authorizes two inferences. They both start with the same premise:

> There are points of light in the dark that grow (as Galileo described) ahead of the advancing bright edge on the moon.

They proceed to two conclusions:

> The bright points are high, opaque prominences.
>
> The higher ones are as much as 4 miles high.

Both inferences proceed deductively if the fact of analogy is as stated. The details are tedious, so I will not rehearse them. It is simply a matter of inferring from a shadow to the shape that produced it. For example, the moment a bright spot first appears ahead of the advancing edge, we know that the bright spot lies on a straight line, tangent to the moon at the edge of the advancing brightness. It now follows that that bright spot is elevated above the spherical surface of the moon, and by an amount recoverable by simple geometric analysis of triangles.

It is worth noting two features of the inferences. First, the analysis looks initially like a textbook instance of a simple analogical inference. Loosely, the earth and moon are similar in their shadows; the earth has mountains causing them; therefore the moon does too. However closer inspection shows that notions of analogy and similarity play a small role. The earth functions as a convenient surrogate for any uneven body turning under unidirectional light. Galileo could equally have called to mind a person's head turning in a room lit by a lantern. As the person's face turns to the light, the tip of the nose would first be lit, before the full nose. What matters is the posit that the moon and its changing pattern of light and dark result from shadows cast. The inference is not driven as much by analogy as by subsumption of the moon into a larger class of illuminated bodies.

Second, the above reconstruction contains deductive arguments only. Galileo's full analysis is inductive. The inductive elements have been confined above by the selection of the fact of analogy. It comes after the inductive part of the analysis is complete. In that inductive part Galileo infers that the moving dark patches are shadows formed by straight rays of sunlight. The basis for his conclusion is the way the bright and dark spots change; they move just like shadows so cast. However that does not entail deductively that they are shadows. The inference is inductive, albeit a fairly safe one. To see that it is inductive, we need only recall that the inference requires also the assumption that no other mechanism could produce patterns of light and dark that move as Galileo observed.

Galileo is taking the inductive risk of accepting this assumption. Other mechanisms are possible and further analysis would be needed to rule them out conclusively. One lies close at hand. In the middle of his discussion, Galileo seeks to assure us that the mountains and valleys need not be visible to us in the periphery of the moon, where we are aligned to see them in elevation. As an addendum to his discussion, he conjectures that the moon's surface may be covered by a layer of "some substance denser than the rest of the ether." (p. 39) This substance may obstruct our view of the lunar terrain at the moon's periphery, for then our gaze passes through a great thickness of the material. Noting that the illuminated portion of the moon appears larger, Galileo conjectures that some interaction between this material and sunlight may be deflecting our gaze outward. Finally, puzzled that "the larger spots are nowhere seen to reach the very edge," Galileo conjectures: "Possibly they are invisible by being hidden under a thicker and more luminous mass of vapours." (p. 40)

The illumination of the mountain tops ahead of the advancing edge employs light that grazes the moon's surface and thus passes through a great thickness of this optically active, denser material. Galileo needs to assume that this optical activity is insufficient to create illuminated mountain tops as something like mirages, that is, by the bending of light towards us by this denser medium.

9. Reynolds Analogy

The explicit identification of analogies has played a prominent role in the analysis of transport phenomena. These are processes in fluids in which momentum, heat and matter are transported. Analogies within these processes form a standard chapter or more in the textbooks. The earliest of these analogies is the "Reynolds analogy," named for Osborne Reynolds, the nineteenth century scientist-engineer who founded the field. Its central idea is of an identity of the processes that transport momentum and heat. Hot gases flowing through a tube, for example, are slowed by friction with the tube's walls. This friction transfers momentum out of the gas and that loss is manifested as a need to maintain a pressure difference to keep the gas flowing. The gas will also transfer heat to the cool tube walls. In the analogy, the two processes operate with identical mechanisms. For more discussion see the account of the Reynolds' analogy below in Appendix A.

This textbook attention to an analogy is quite revealing, since it shows directly how a particular science conceives an analogy. It conceives the analogy as an empirical fact. The fact has two modes of expression, as reported in the Appendix. In the looser mode, the analogy asserts that the mechanisms or laws governing momentum and heat transfer are the same. That version is somewhat ambiguous. Since heat and momentum are different quantities with different properties, just how can the mechanisms or laws be the same? If we construe the sameness to mean that the rates of momentum and heat transfer are numerically proportional under the same conditions, then there is a simple quantitative expression of this sameness in terms of two dimensionless numbers. The friction factor f measures the frictional losses of momentum from a moving fluid; the Stanton number St measures the rate of heat transfer. This second, more precise form of the analogy sets these two numbers equal, up to a constant factor: $f/8 = St$.

In material terms, this literature is equating the analogy with the fact of analogy. The associated analogical inferences are present, but draw only subsidiary attention. The most common is to use the analogy to authorize an inference from momentum transfer to heat transfer. That is, if we know the friction factor f for some system, we use the fact of analogy to infer to the Stanton number St. From the Stanton number we can infer rates of heat transfer. This inference has great practical utility. Friction factors are relatively easy to determine from pressure differences. The corresponding rates of heat transfer are a great deal harder to measure.

This practical utility of the Reynolds' analogy means that there is some premium on determining just how good an analogy it is. When faced with this problem, the literature does not seek guidance from a formal theory of analogical reasoning. It does not ask for rules on how to trade off the competition of positive and negative analogy. The refinement of the analogy is regarded as an empirical question to be settled by measurement. The equation to be tested is just that $f/8 = St$. It was evident already quite early that the analogy obtains only in special cases. It fails for fluids in laminar flow and even liquids in turbulent flow, but succeeds as a relatively poor approximation for gases in turbulent flow. Since the fundamental analysis of fluids in turbulent flow is difficult, the exploration of the analogy and refined versions that replace it, has remained largely a matter of brute-force empirical measurement.

10. Liquid Drop Model

In the 1930s, after the discovery of the neutron, the new field of the nuclear physics was born. The nucleus of an atom was recognized as consisting of many particles. The most common isotope of Uranium, U^{238}, consists of 92 protons and 146 neutrons, which sums to an overall nucleon number of 238. The nucleus was found to exhibit energetically excited states, somewhat like the excitations of an electron in a hydrogen atom. However the single particle methods that had worked so well for electrons in atoms were inapplicable to the many-body problem posed by the atomic nucleus. The many particles of the nucleus, all clustered together, seemed something like the many molecules clustered together in a liquid drop. The liquid drop model of the nucleus was based on this analogy. The hope was that the physics of drops might also coincide with at least some of the physics of nuclei.

The liquid drop model was already an established element of nuclear theory[6] in the 1930s, before it found its most popular application. In 1939, Lise Meitner and Otto Frisch (1939) sent their celebrated letter to Nature in which they proposed that certain processes were dividing the nuclei of Uranium atoms. This "fission" process, they suggested, could be understood using the liquid drop model. The capture of neutrons by Uranium nuclei may be sufficient stimulus to break them apart, much as an unstable liquid drop is easily broken up by a slight tap. The idea was taken up by Bohr and Wheeler (1939), who extended the liquid drop model quantitatively to encompass fission.

A liquid drop is held together because its constituent molecules are attracted to each other. For molecules deep within the drop, these attractions do not pull markedly in any direction and thus, by themselves, do not contribute to the drop's cohesion. Molecules near the surface, however, are attracted towards the drop by those deeper in the drop. A drop may have many shapes. However the larger the surface area, the more it has molecules on its surface seeking to move towards the center. Hence the drop naturally adopts a shape with the smallest surface area, a sphere, as its lowest energy state. This tendency to spherical form is commonly described as arising from a tension in the surface driving the drop to its smallest area. The general theory assigns a surface tension energy to the drop, proportional to its surface area. If the drop is energized by tapping, for example, it oscillates, somewhat like the ringing of a struck bell. As the drop deforms and increases its surface, it excites to higher energy states and absorbs the added energy of the tap. Finding the spectrum of these oscillations was an already solved problem of classical physics.

The motivation for the liquid drop model of the nucleus is the idea that the stability of the nucleus arises is some analogous way. It leads to the assumption that there is a nuclear energy corresponding to the surface tension energy of the drop. The volume of a nucleus is proportional to A, the number of nucleons. Volume varies with radius^3 and surface area with radius^2.

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[6] For an early review before fission, see Bethe (1937, §53). For a history of the origin of the liquid drop model, see Stuewer (1992). I thank Michel Janssen for drawing Roger Stuewer's history of the liquid drop model to my attention.

Therefore the surface area of the nucleus varies as $A^{2/3}$ and the liquid drop model posits an energy proportional to $A^{2/3}$. Further, the various excitation modes of the nucleus were assumed to correspond to those of a liquid drop with suitably adjusted parameters.

Finally, the instability of a nucleus that results in fission could be analyzed quantitatively. The surface tension effect tends to hold the nucleus together. However a nucleus is positively charged, carrying Z protons. This positive charge creates forces that drive the nucleus apart. They come to be favored as the nucleus grows larger. The point at which they overcome surface tension is computed in the model by finding the state in which the slightest energizing of the nucleus will lead to such violent oscillations that the nucleus must split. The computation yields a stability condition expressed in terms of the number of protons Z and the number of nucleons A. The ratio Z^2/A must be less than 42.2 (as quoted by Blatt and Weisskopf, 1979, p. 304). U^{238} is perilously close to this figure, so it is expected to be prone to fissioning. For it, $Z^2/A = 92^2/238 = 35.5$. This result is traditionally quoted as a great success for the model.

The model appears, initially, to be a textbook case of analogical inference. In their synoptic treatise on nuclear physics, Blatt and Weisskopf (1979, p. 300) give what amounts to an inventory of the positive and negative analogies. “We find the following points of analogy,” they remark and then proceed to list three elements of the positive analogy. They can be stated in simplified form, writing “A” for both the number of molecules in the drop and the number of nucleons in the nucleus. They are:

- The volume of a liquid drop and the volume of a nucleus are both approximately proportional to A.
- The energy to evaporate a drop and the binding energy of nucleus are both approximately proportional to A, subject to correction by a surface tension term.
- Surface tension corrects this energy for a liquid drop by an additive term in $A^{2/3}$; and a semi-empirical formula for the binding energy of a nucleus also has an additive term in $A^{2/3}$.

However Blatt and Weisskopf harbor considerable doubts about the analogy. “It is probable that this analogy is only very superficial,” they continued. What followed amounted to an inventory of the negative analogy, consisting of:

- The stability of a liquid drop derives from repulsive forces that preclude molecules approaching one another by less than a minimum distance of the order of the size of electron orbits. There is no similar limit known for the approach of nucleons.
- Molecules in a drop follow the classical dynamics of localized particles. Nucleons have de Broglie wavelengths of the order of inter-nucleon distances and are governed by quantum mechanics.

At this point in the narrative, what is needed is some assessment of how good the analogy is. What Blatt and Weisskopf do *not* do is to try to assess the competition between these rivaling factors by appeal to general rules such as one might expect from a formal approach to analogical inference. Rather, they derive the formula for the energy levels of a nucleus as indicated by the model and subject it to experimental test. They decide (p. 305) that the energy levels fit observation poorly. " The liquid drop model of the nucleus is not very successful in describing the actual excited states. It gives too large level distances." However the liquid drop model works better when it comes to fission: "The limit for stability against fission is well reproduced…"

This mode of assessment is just what the material theory calls for. The fact of analogy, as revealed through this assessment, is a rather bare one. It is:

> The energy of a nucleus has an additive surface term proportional to $A^{2/3}$; and the nucleus' oscillatory modes match those of a liquid drop with corresponding parameters.

This fact is sufficient to support the inferences made under the model; and this fact is what Blatt and Weisskopf are actually putting to test.[7]

We also see once again that the similarity of the source and target is a subsidiary matter. What matters to the analogy is what is expressed in the fact of analogy, that the liquid drop and nucleus share just the properties listed.

11. Conclusion

The material theory of induction succeeds in simplifying our understanding of analogical reasoning in its acceptance of the dual role of facts: they may be premises in arguments and they

[7] For a more recent assessment with similar import see Wagemans (1991), pp. 8-12.

may also serve as warrants of inference. Crucially, the material theory allows that displaying such facts provides the justification of the analogical inference and is the endpoint of analysis that seeks to determine the validity of the analogical inference. While there will be similarities among different analogical inferences, there will be no overarching similarity of sufficient power to allow the separation of good and bad inductive inference by purely formal means.

A formal approach faces a more elaborate challenge. It can allow that a fact of analogy can somehow play a role in justifying an analogical inference. But this recognition cannot terminate a successful formal analysis. The validity of an analogical inference must be established ultimately by displaying conformity with a universal schema. The enduring difficulty is that, no matter how elaborate these schemas may have become, none proves to be final and complete. That this difficulty is irremediable is predicted by the material theory of induction.

Appendix A. Reynolds Analogy

The General Idea

In the dynamic analysis of systems with moving fluids, analogies have been found between three of the most important types of processes. The three processes are often called the "unit operations" of chemical engineering. They are momentum transfer, heat transfer and mass transfer.

The simplest and most studied instance is a fluid (gas or liquid) flowing in a cylindrical tube. As the fluid flows through the tube, its passage is resisted by friction with the wall of the tube. At the center of the tube, the fluid moves with the greatest velocity and therefore has the highest momentum density. At the wall of the tube, friction has brought the fluid to a halt, so that the outermost layer of fluid has no momentum. This frictional slowing is understood as a momentum transfer process. Momentum from the inner part of the fluid is passed to its outer surface, where it is lost to friction. This loss of momentum must be compensated by an applied force if the fluid is to continue flowing. That applied force creates a pressure difference along the length of the tube.

Heat transfer can arise in same system. The tubes might be in the boiler of a steam engine. Hot flue gases from the fire pass through a bundle of tubes that are surrounded by a jacket of boiling water. Heat is transferred from the gases in the tubes, through the tube walls into the water. To illustrate mass transfer, we might imagine that the gases contain some contaminant that is to be scrubbed out. The inner surface of the tube carries some absorbing solution. In the mass transfer operation, the contaminant passes from the gas into the solution.

The analogies arise from the idea that the mechanisms of three processes are the same, so that they are governed by the same quantitative laws. That simple idea has proven to be difficult to verify in all generality. The earliest proposals for implementing the analogies proved to work only under very restrictive conditions. In spite of the early failures, the idea of the analogy has proven appealing and has generated a literature of many different and more complicated implementations.

Our interest is the underlying logic used with these analogies. We can recover that well enough merely by looking at the first and best known analogy, the "Reynolds analogy." It is the proposition that the mechanisms of momentum and heat transfer are the same. Texts differ in their statements. Here are a few selected at random:

> Reynolds postulated that the mechanism for transfer of momentum and heat are identical. (Foust et al., 1960, p. 173.)
>
> …Reynolds suggested that momentum and heat in a fluid are transferred in the same way. He concluded that in geometrically similar systems, a simple proportionality relation must exist between fluid friction and heat transfer. (Kakaç and Yener, 1995. p. 203)
>
> Reynolds proposed that the laws governing momentum and heat transfer were the same. (Glascow, p. 156)

These statements are strong and it is not entirely clear how they are grounded.

The Original Reynolds Analogy

Reynolds' authority is routinely invoked. Reynolds' (1874) original note certainly proposes some connection between the rate of heat transfer and internal motions in a fluid. However it is unclear that he intends a complete identity of both mechanism and law as asserted above. His analysis was not conducted in the context of the modern theory of transport phenomena and his paper does not give the quantitative expression now attached to the analogy. There are none of the dimensionless numbers we shall see shortly: no friction factors or Stanton numbers. Reynolds' own celebrated analysis of fluid flow in pipes was published nine years later. Reynolds' synopsis of his 1874 paper from his later collected papers reads:

> The heat carried off by a fluid from a surface proportional to the internal diffusion of the fluid near the surface—the two causes natural diffusion of the fluid at rest, and the mixing due to the eddies caused by visible motion—the combined effect expressed by: H = At + Bρvt—this affording an explanation of results attained in Locomotive Boilers—experimental verification. (Reynolds, 1900, p.xi)[8]

For later reference, this equation is numbered by Reynolds as (I):

$$H = At + B\rho vt \qquad \text{(I)}$$

The closest Reynolds comes to a direct assertion of analogy arises in connection with a second equation he numbers as (II)

$$R = A'v + B'\rho v^2 \qquad \text{(II)}$$

where R is the resistance to fluid flow in the pipe. The essential quantitative assumption of Reynolds' (1874, p. 83) analysis was:

> And various considerations lead to the supposition that A and B in (I) are proportional to A' and B' in (II).

This analogy asserts less than the sameness of laws. In drawing an analogy between momentum and heat transfer, the temperature difference t is analogous to the velocity v, for each magnitude drives the transport. Heat transport arises from a temperature difference and momentum transport arises from the velocity differences of a velocity gradient. Under this association, the "B" term of equation (I) would need to be $B\rho t^2$, which it is not.

There is a way that the equations (I) and (II) can be fully analogous, however Reynolds does not make these details explicit, so we cannot know if he intended them. We assign dual roles to the velocity v. In its first role, it measures the fluid flow, so that the term ρv measures fluid flux. In its second, it drives momentum transport and is analogous to temperature difference t. We would then suppose that the first appearance of v in the v^2 term of (II) represents fluid flux and the second v in the v^2 term of (II) represents driving force. Then both B terms of (I) and (II) would have the analogous form "B (fluid flux) (driving force)."

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[8] H is the time rate of heat passed per unit surface area, t is the temperature difference between the surface and fluid, ρ is the fluid density, v its velocity and A and B are constants.

Reynolds' explicit use of a more limited analogy that determines how large the velocity v needs to be for the "B" term of (I) to dominate. The proportionality of the constants enabled Reynolds to argue that this arose under the same conditions for which the B' term of equation (II) dominates. That, he reported, arose for "very small" v.[9]

There was an immediate practical application of the dominance of the B term for commonly arising velocities. When it dominates, the temperature of the discharged fluid is independent of the velocity v.[10] That means that a locomotive boiler operating with larger flue velocities would be equally efficient at withdrawing heat from the flue gases no matter how great the flow of flue gases. This result, Reynolds could report with obvious satisfaction, explained an otherwise surprising fact about boilers: they are "as economical when working with a high blast as with a low." (p. 84)

The Modern Reynolds Analogy

If we cannot ground the analogy of the modern textbooks in Reynolds' original work, there are informal justifications available. There are two regimes for fluid flowing in tubes. If the flow is slow or the fluid very viscous, then the flow is laminar. It has the perfectly regular streamlines of slowly flowing honey. When the velocity is high, however, these perfect lines are disturbed by tumultuous eddies, readily visible if smoke or a tracing dye is injected into the fluid. These eddies mix the fluid quite efficiently. They will carry the fluid in bulk from the center of

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[9] Reacting to Reynolds' name, modern readers will likely find it irresistible to associate the conditions in which the A and B term dominate as regimes of laminar and turbulent flow respectively. However, Reynolds' (1883) celebrated study of laminar and turbulent flow was published nine years later and supports different relations. In it, Reynolds (p. 975) reports that previous experiments had adhered to laws $i = v^2$ or $i = Av + Bv^2$, where i is a pressure term. He now corrects these laws by setting the pressure term proportional v in the laminar regime and to $v^{1.723}$ in the turbulent regime.

[10] When the B term dominates, it follows from (I) that the heat withdrawn H is proportional to the mass flux ρv. So doubling the mass flux will just double the heat withdrawn, which entails that there is no change in the temperature reduction of each unit of mass of the flue gases passing through the boiler.

the tube to the wall and back. In this process, they transport both the momentum and heat of the fluid, making it plausible that the same law governs both transports. It is at best a weak grounding, for we proceed with little more than a caricature of turbulence and ignore a laminar region in the fluid that will be at the tube's inner surface. Since the plausibility argument can be given at best for turbulent flow, some authors limit assertion of the Reynolds analogy to turbulent flow. This is so with Kay and Nedderman (1974, pp. 143-44), who also sketch the above grounding.

Whether well-grounded or not, the goal is to generate a quantitative relation from the analogy. To do that, we need to find quantitative measures of both momentum and heat transfer. In the case of fluid flow in tubes, the pressure difference, ΔP, is an easy-to-measure manifestation of the momentum transfer process within the tube. This pressure difference will depend upon many other factors. It will change with many variables: the average speed of the fluid v, the length of the tube L, its diameter D, as well as the physical properties of the fluid, such as its density ρ. If we seek general regularities that govern this pressure difference, it turns out that we can accommodate many of these variables by considering a dimensionless number formed from these variables. The most commonly used is a dimensionless number, the friction factor[11]

$$f = (D/L)\Delta P/(\rho v^2/2)$$

We need not linger over why this particular combination of variables is introduced. It will be sufficient for our purposes to treat f as generalized measure of pressure difference and thus a measure of momentum transport.

In the case of heat transport, we are interested in the time rate q that heat is transmitted to the tube walls. The total rate will vary with the area of the walls A and the temperature difference ΔT between the tube wall and the fluid mean temperature that is driving the transport. To accommodate these variables, the goal of analysis is usually a heat transfer coefficient h, where

$$h = q/A\Delta T$$

[11] The definitions of these dimensionless numbers can sometimes differ in constant factors. I follow the conventions of Foust et al. (1960).

Since the heat capacity at constant pressure C_P, mean velocity v and fluid density ρ can also affect the process, it turns out to be most convenient to embed the heat transfer coefficient in the dimensionless Stanton number

$$St = h/C_P\rho v$$

Once again, we need not linger now over just why the number is assembled as it is. We need only treat it as a generalized measure of the rate of heat transport.

Determining just how much momentum and just how much heat are transported out of the tube under nominated conditions is not easy. If the flow is turbulent, it cannot be done from first principles. However if we assume with the modern Reynolds analogy that the same process transports both, then, whatever the amounts may be, they are closely connected. A fairly straightforward if tedious computation (given in the next section) finds that connection to be expressed as an equality between the two dimensionless numbers that measure momentum transport and heat transport:

$$f/8 = St$$

This is the quantitative statement of the Reynolds analogy. It is an empirical claim that can be tested quite readily. It turns out only to hold under quite limited conditions. It holds as a relatively poor approximation for gases in turbulent flow, but fails for liquids and fluids in laminar flow. See Glasgow (2010, pp. 156-57) for a brief historical sketch of the discovery of limits to the analogy and of efforts to improve it.

Generating the Quantitative Relation

Now we will linger over why the two numbers St and f are chosen to be as they are. Following Foust et al, 1960, p. 173, we may generate the quantitative expression for the Reynolds analogy, f/8=St, as follows. The context is a fluid of density ρ flowing with mean velocity v in a tube of diameter D and length L. Momentum, heat and, in general, other quantities are transferred to the tube wall. It is assumed that this transport of an unspecified quantity is governed by the relation

flux at wall = -K (concentration at wall – mean concentration)

The "flux at wall," is the time rate of transport of the quantity per unit wall area. The two concentrations are just the amount per unit volume of the quantity, respectively at the wall and averaged over the whole fluid. The real point of the equation is to define the general transport coefficient K, whose values will vary with any change in the physical properties of the fluid and the geometry of the tube.

The supposition is that this equation holds for both heat and momentum transport, so that we can define a coefficient K_{heat} and K_{mom} for each. The quantitative expression of the Reynolds analogy arises from setting the two coefficients equal.

For the case of heat, the "flux at wall" is q/A, where q is the total rate of heat transport from the fluid and A is the tube wall area. The concentration of heat is just $\rho C_P T$. Hence we can write

$$q/A = -K_{heat} (\rho C_P T_{wall} - \rho C_P T_{mean}) = -K_{heat} \rho C_P (T_{wall} - T_{mean})$$

The second equality obtains if both ρ and C_P vary negligibly over the system. In general this assumption fails. However, for common engineering applications, it holds quite well in a wide range of cases. If we compare this last equation with the definition of the heat transfer coefficient h

$$q/A = h\Delta T = -h (T_{wall} - T_{mean})$$

we can then identify

$$K_{heat} = h/\rho C_P = (h/\rho C_P v)\, v = St\, v$$

where $St = h/\rho C_P v$ is the Stanton number defined earlier.

For the case of momentum, we proceed as follows. The total pressure force acting on the fluid is (pressure drop) x (flow area) = $\Delta P\, \pi D^2/4$. By Newton's second law, this quantity is the total rate of loss of momentum from the fluid. All this momentum is lost through transport to the tube wall, since friction from the wall surface is the only other force acting on the fluid. The tube wall has area $L\pi D$. Hence

$$\text{momentum flux at wall} = (\Delta P \pi D^2/4) / (L\pi D) = (\Delta P/4)(D/L)$$

The momentum concentration is (mass density) x velocity. At the wall, the velocity is zero, since the fluid is halted by friction with the tube wall. Thus the momentum density at the wall is zero. The mean momentum density is just ρv. Combining and substituting into the general transport equation used to define K we recover

$$(\Delta P/4)(D/L) = -K_{mom} (0 - \rho v)$$

so that

$$K_{mom} = (D/L)\ (\Delta P/4\rho v) = (1/8)\ v\ (D/L)\ \Delta P/(\rho v^2/2) = v\ f/8$$

where $f = (D/L)\Delta P/(\rho v^2/2)$ is the friction factor defined earlier.

We now express the Reynolds analogy in the setting equal of the two coefficients[12]

$$K_{heat} = St\ v = v\ f/8 = K_{mom}$$

from which we recover the quantitative expression for the Reynolds analogy

$$St = f/8$$

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[12] It may seem odd at first to set K_{heat} and K_{mom} equal, rather than merely proportional. For they pertain to the transport of different quantities, heat and momentum, where each is measured by its own system of units. Just this reason would preclude us setting *rates* of heat and momentum transport equal, for the equality would fracture if we merely changed our units for measuring heat from calories to BTU. However this will not affect the coefficients K. For they are insensitive to unit changes in the quantity transported. If we change the numerical value of the heat flux by moving our units from calories to BTU, there will be a corresponding change in the heat concentrations, so that the value of K_{heat} remains unchanged.

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