Chapter 1: Productive Ambiguity: Galileo *contra* Carnap

Argument that employs controlled and highly structured ambiguity can play a central role in mathematical discovery and justification. The exposition of projectile motion given on the Fourth Day, following preliminaries on the Third Day, of Galileo's *Discourses and Mathematical Demonstrations Concerning Two New Sciences* {1} is a good illustration of my central claim. I propose to begin this book *in medias res*, with a case study, because Galileo's proofs are canonical, paragons of mathematical and scientific demonstration that set the stage for the scientific revolution. If productive ambiguity is central to these proofs, we can expect it to be both commonplace and rational in many other settings. In the rest of the book, I will show that this is indeed the case; and I will draw out the philosophical consequences of this insight for epistemology, including a theory of mathematical knowledge.

In the analysis of free fall on the Third Day, the use of proportions is polyvalent, because Galileo asks us to read their terms both as finite and as infinitesimal. When we read them as finite, they allow for the application of Euclidean results and also exhibit patterns among whole numbers; and their configurations stand iconically for geometrical figures. When we read them as infinitesimal, they allow for the elaboration of the beginnings of a dynamical theory of motion, leading to the work of Torricelli and Newton; and their configurations stand symbolically for dynamical, temporal processes. In the exposition of projectile motion, the curve of the semi-parabola, read iconically, stands for a temporal, dynamical process that we 'see' whenever a projectile leaves a trail behind it; read symbolically, it stands for an infinite-sided polygon that articulates the rational relations among an infinite array of instances of uniform motion that compose the accelerated, curvilinear motion of the projectile. And the rationality of that reduction is justified by results involving proportions and the similarity of geometric figures. {2}

Galileo's use of ambiguous modes of representation is typical of reasoning in mathematics, even though the pattern has not been sufficiently noted and studied by philosophers of mathematics. Under the influence of the Vienna School, Anglophone philosophers often write as if the language of mathematics and science is, or ought to be, univocal and transparent; the second section of this chapter examines this thesis, which I contest, in the writings of Carnap. The terms of an ideal language, Carnap argues, should refer one-to-one to all and only those things that exist, and its predicates and relations should follow suit. Thus, its locutions should not refer to more than one state of affairs at a time, and should not add anything to the situation: there should be no linguistic 'artifacts.'

In the third section, I locate my own position in the context of a general tendency in Anglophone philosophy of science and mathematics to move from a syntactic approach to a semantic and indeed pragmatic approach, which studies the use of language in terms of its representational role in an historical context of problem-solving. The 'pragmatic' philosophers find that problem-solving typically requires the juxtaposition of a variety of modes of representation; I emphasize that in such contexts a single mode of representation, used iconically for one purpose and symbolically for another, may be called upon to mean more than one thing. The resultant polysemy generates not confusion but insight. In the fourth section, and as a bridge to the second chapter, where I propose an account of *analysis* and mathematical *experience* that emerges from my reflection on Leibniz, Hume, Peirce, and the work of various contemporary philosophers and historians, I discuss a case study from the history of chemistry developed by Ursula Klein. This episode prefigures my own chemical and mathematical case studies, where I argue that the role of representation must be understood in its semantic and pragmatic as well as syntactic dimensions. Keeping all three dimensions of mathematical reasoning in view together, I can better address the issues of reduction, explanation, representation, and rationality.

1. Constructive Ambiguity in Galileo's Demonstration of projectile Motion

Mathematics often requires the combination of different modes of representation in the same argument: equations, diagrams, matrices, tables, proportions, schemata, natural language. Arguments in mathematics do many things. They defend definitions, constitute problems, explain problem solutions, deploy and exhibit procedures or methods, and formally or informally present proofs. When modes of representation are combined in mathematical arguments, they may be juxtaposed or superimposed, or carefully segregated to exhibit certain features of the situation. Some arguments—and I claim this is true of Galileo's reasoning examined in this section—may require that one and the same representation be used ambiguously in order for the mathematician to exhibit a novel organization and exploration of things, and for the reader to follow the reasoning.

Galileo's treatment of free fall and projectile motion occurs in the Third Day and Fourth Day of his *Discourses and Mathematical Demonstrations Concerning Two New Sciences* (referred to hereafter as the *Discorsi*). The Third Day of the *Discorsi* is entitled 'Change of Position,' and its first section is 'Uniform Motion.' Galileo defines uniform motion—straight line motion at a constant speed—as 'one in which distances traversed by the moving particle during any equal intervals of time, are themselves equal,' and adds that the equal intervals must be thought of as being arbitrarily chosen; he thus includes the possibility that they may be chosen to be arbitrarily small. The first diagram he offers, which accompanies Theorem I, Proposition I of 'Uniform Motion,' consists of two horizontal lines, the line IK representing time and the line GH representing distance that is re-conceptualized to mean displacement, since we are instructed to suppose that a moving particle is traversing it. {3} [Figure 1.1] The two lines therefore have a different status, since no particle traverses the time-line. Both lines are, however, measured off in intervals: the left-hand half of line IK is measured in intervals of length DE and the right-hand half in intervals of length EF, while the left-hand half of line GH is measured in intervals of length AB and the right-hand half in intervals of length BC.

Theorem I, Proposition I states, 'If a moving particle, carried uniformly at a constant speed, traverses two distances the time-intervals required are to each other in the ratio of these distances.' {4}

This theorem asserts that a (non-continuous, that is, without a shared middle term between the two ratios) proportionality AB: BC :: DE : EF holds between any two displacement intervals and any two corresponding time-intervals in uniform motion. Galileo has designed the diagrams and the reasoning to allow for a direct application of the Euclidean / Eudoxian axiom, which states that proportions between non-continuous ratios, Q : R :: S : T, can be formed if and only if for all positive integers m, n, when $nQ \leq mR$, then correspondingly $nS \leq mT$. Its intent is to allow for the comparison of ratios when Q and R are one kind of thing, and S and T are another kind of thing, while holding to the precept that ratios themselves may only compare things of the same kind. In Greek mathematics, ratios cannot hold between lines and numbers, between finite and infinitesimal magnitudes, or between curved lines and straight lines. The Euclidean tradition treats ratios as relations, different from the things related, and proportions as assertions of similitude (not equality) between ratios.

There is, however, a second, medieval tradition of handling ratios and proportions that originates with Theon, a commentator on Ptolemy's *Almagest*, and is transmitted by Jordanus Nemorarius, Campanus, and Roger Bacon. It associates with each ratio a 'denomination,' that is, a number which gives its size, and in general treats the terms occurring in ratios as well as the ratios themselves uniformly as numbers. Thus ratios are just quotients and the distinction between ratio and term is abolished in so far as they are all numbers. {5} The proportion Q : R :: S : T becomes Q / R = S / T, the equation of two numbers, and so automatically Q x T = R x S. The *first* tradition governing proportions is invoked here and proves just what Galileo requires; indeed, the second tradition would be unhelpful because in the assertion Q x T = R x S, the product [(time interval) x (displacement interval)] is physically pointless. The really interesting product is [(time interval) x (mean velocity during that time interval)], as will appear below.

In the sequel, Theorem II, Proposition II, and Theorem III, Proposition III, Galileo examines cases involving two particles in uniform motion, and concludes in Theorem IV, Proposition IV: 'If two particles are carried with uniform motion, but each with a different speed, the distances covered by them during unequal intervals of time bear to each other the compound ratio of the speeds and time intervals.' {6} In other words, a precise relationship can be established between any two cases of uniform motion; Galileo formulates it as a proportion: D_1: D_2 :: [S_1 : S_2 compounded with T_1 : T_2]. The problem is that 'compounding' or finding a product of ratios can only be carried out with continuous ratios, according to the first tradition of handling proportions: to compound the ratios A : B and B : C is rewrite their combination as A : C. However, S_1 : S_2 and T_1 : T_2 are not continuous, and Galileo is not willing to treat S_1: S_2 and T_1 : T_2 as fractions that could simply be multiplied and thus compounded according to the second tradition. Galileo solves the problem by finding the middle term I between D_1 and D_2, which must satisfy the proportions D_1 : I :: S_1: S_2 and I : D_2 :: T_1 : T_2 : it is the distance the second particle would traverse in the time interval allotted to the first particle. Since we can always find such an I, we can always bring S_1 : S_2 and T_1 : T_2, and D_1 and D_2 into rational relation. The accompanying diagram [Figure 1.2] is just a collection of line segments, one each for the speed, time, and distance traversed of body E (resp. A, C and G) and the speed, time, and distance traversed of body F (resp. B, D and L), as well as the seventh line segment, I, which links the two sets of proportions.

The reason why Galileo goes to the trouble of showing that two separate cases of uniform motion can be rationally linked in this manner is because he is going to reduce the uniformly accelerated motion of free fall to a series of cases of uniform motion which then must be brought into rational relation. This is a nice instance of problem reduction, leading a problem about a more complex thing (uniformly accelerated motion) back to a problem about a simpler thing (uniform motion). However, we will see that the reduction only works if the intervals in question may be made "as small as one wishes," which of course leads to a highly non-Euclidean employment of the theory of proportions as well as highly non-Euclidean geometric diagrams. Galileo's treatment of the proportions and diagrams later on becomes carefully ambiguous; and therein lies the innovation.

Theorem I, Proposition I in the section 'Naturally Accelerated Motion' states: 'The time in which any space is traversed by a body starting from rest and uniformly accelerated is equal to the time in which that same space would be traversed by the same body moving at a uniform speed whose value is the mean of the highest speed and the speed just before acceleration began.' {7} The accompanying figure has two components, a vertical line CD on the right representing space traversed (again, not just distance but displacement), and a two-dimensional figure AGIEFB on the left, in which AB represents time. [Figure 1.3]. The two-dimensional figure reproduces Oresme's diagram that applies the important theorem reached by the logicians at Merton College, Oxford concerning the mean value of a 'uniformly difform form' to uniformly accelerated motion. However, Galileo rotates it by 90° because he is going to apply it even more specifically to the case of free fall, and wants to emphasize its pertinence to the vertical trajectory CD. Koyré points out that the genius of this set of figures is that AB represents not the distance traversed but time, for Galileo (like Oresme) had wrested geometry from the geometer's preoccupation with extension and put it in the service of the temporal processes of mechanics. {8} The left-hand figure represents a process like integration with respect to time: the parallels of the triangle AEB perpendicular to AB stand for velocities, and the area of the triangle as a whole, taken to be a summation of instantaneous velocities, therefore represents distance traversed. Distance is then represented in two different ways, as the line segment CD and as the area of the triangle AEB; because the second representation is a two-dimensional figure, it can exhibit the way that uniformly increasing velocity and time are related in the determination of a distance.

A two-fold representation of distance also occurs in the analysis of free fall given immediately afterwards in Theorem II, Proposition II, but here the right-hand line gains articulation and the left-hand two-dimensional figure loses some: this theorem is about distances, or rather, displacements.[Figure 1.4] The theorem states: 'The spaces described by a body falling from rest with uniformly accelerated motion are to each other as the squares of the time-intervals employed in traversing these distances.' {9} The right-hand figure, the line HI, stands for the spatial trajectory of the falling body, but it is articulated into a sort of ruler, where the intervals representing distances traversed during equal stretches of time, HL, LM, MN, etc., are indicated in terms of unit intervals (by a shorter cross-bar) and in terms of intervals whose lengths form the sequence of odd numbers, 1, 3, 5, 7… (by a longer cross-bar). The unit intervals are intended to be counted as well as measured. In the left-hand figure, AB represents time (divided into equal intervals AD, DE, EF, etc.) with perpendicular instantaneous velocities raised upon it—EP, for example, represents the greatest velocity attained by the falling body in the time interval AE—generating a series of areas which are also a series of similar triangles.

Galileo then considers two cases of uniform motion, and brings them into rational relation, which proves the theorem. He instructs us to draw the line AC at any angle whatsoever to AB and then, given any two equal time intervals AD and DE, to draw parallel lines DO and EP intersecting AC at O and P. He uses the result just proved to show that the distance traversed by a particle falling from rest with uniformly accelerated motion during the time interval AD (resp. AE) is the same as the distance traversed by a particle moving with speed ½ DO (resp. ½ EP) during the time interval AD (resp. AE). Thus we know that the ratio D_1: D_2 is the same as the ratio (distance traversed during AD at speed ½ DO): (distance traversed during AE at speed ½ EP). But what is the latter ratio—how can we bring these two cases of uniform motion into rational relation? The answer is given in Theorem IV, Proposition IV from the section on uniform motion: 'the spaces traversed by two particles in uniform motion bear to one another a ratio which is equal to the product of the ratio of the velocities by the ratio of the times.' {10} And in this case, because ΔADO is similar to ΔAEP, we know that the ratio of AD:AE is equal to the ratio of ½ DO : ½ EP, which is just the same as the ratio DO : EP; so [V_1 : V_1 compounded with T_1 : T_2] is just [T_1 : T_2 compounded with T_1 : T_2]. Since the ratios only involve the single parameter time, Galileo doesn't mind treating them as numbers and calls the product $[T_1 : T_2]^2$. Thus D_1: D_2 is equal to $[T_1 : T_2]^2$. By the same token, D_1: D_2 is equal to $[V_1: V_2]^2$, the square of the ratio of the final velocities.

Galileo gains his insight here by combining numerical patterns with geometry in the service of mechanics, as this summary from the immediately following Corollary I indicates: 'Hence it is clear that if we take any equal intervals of time whatever, counting from the beginning of the motion, such as AD, DE, EF, FG, in which the spaces HL, LM, MN, NI are traversed, these spaces will bear to one another the same ratio as the series of odd numbers 1, 3, 5, 7; for this is the ratio of the differences of the squares of the lines [which represent time]…While, therefore, during equal intervals of time the velocities increase as the natural numbers, the increments in the distances traversed during these equal time-intervals are to one another as the odd numbers beginning with unity.' {11} Since $1 + 3 = 2^2$, $1 + 3 + 5 = 3^2$, 1 + 3 + 5 + 7

$= 4^2$, and so forth, these sums representing distances will be proportional to the square of the time intervals; and since the time elapsed is proportional to the final velocity, as the similar triangles in the diagram to the left makes clear, the distance fallen will be proportional to the square of the final velocity.

Galileo is now using at least four modes of representation to express his argument: proportions, geometrical figures, numbers, and natural language. He also employs a systematic ambiguity to carry his argument further. By adding Corollary I to Theorem II, Proposition II, he insists on the pertinence of the number theoretical facts just discussed to the analysis of free fall. The reader is thus forced to read the intervals depicted (AD, DE, EF…, and then HL, the three intervals of LM, the five intervals of MN…) sometimes as units, sometimes as infinitesimals. *There is only one set of diagrams, but the set must be read in two ways*. Reading the intervals as finite allows both for the application of Euclidean results, and for the pertinence of the arithmetical pattern just noted. Reading the intervals as infinitesimal allows for the analysis of accelerated motion. The accompanying text in natural language guides and exploits this double meaning.

Note that in Corollary I, Galileo does not compare the interval-terms directly, but is careful to refer to them in ratios. Even if infinitesimal intervals (instants and points, to use Galileo's vocabulary) are mathematically suspect—as they surely were in the early 17th century—the geometry of the diagrams supports the rationality of holding that ratios between them are 'like' the ratios between their finite counterparts. That is, AD:DE::AO:OP no matter what size the configuration is; or, to use the other diagram, HL:LM::1:3 no matter what the size of the configuration. Theorem IV, Proposition IV from the section on uniform motion, which is so carefully Euclidean in its reasoning, is here put to highly non-Euclidean use because of its juxtaposition with the systematically ambiguous diagram. When the time intervals AD and AE are read as finite, the application of the theorem that brings disparate cases of uniform motion into relation is direct; when AD and AE are read as infinitesimal (because we may take 'any equal intervals of time whatsoever') the application of the theorem is non-Euclidean because Euclid does not allow infinitesimal terms. But it is *this* application that allows cases of uniform motion to be brought into rational relation with uniformly accelerated motion in a way that Newton can employ when, a generation later, he uses geometry to represent the dynamical processes of the solar system. Read as finite, the triangles are the iconic representations of geometrical figures; read as infinitesimal, the triangles are the symbolic representation of a dynamical process, free fall. What lies before us in Figure 1.4 is a diagram that must be read in two different ways, as both icon and symbol, and natural language that explains the ambiguous configuration. The same point can be made a propos the left-hand diagram that accompanies Theorem I, Proposition I [Fig. 1.3] and the diagram borrowed from the Oxford Calculators that adumbrates Corollary I in the section 'Naturally Accelerated Motion'.

The centrally important diagram of projectile motion from Theorem I, Proposition I of the *Fourth Day* [Fig. 1.5] also enjoys an ambiguity rich in consequences. The diagram of projectile motion must be compared to the diagram accompanying Theorem I, Proposition I from the section 'Uniform Motion' [Figure 1.1], the diagram accompanying Theorem II, Proposition II from the section 'Naturally Accelerated Motion' of the *Third Day* [Fig. 1.4], and the diagram of a parabola borrowed from Apollonius just preceding. Significantly, the diagram of projectile motion refers to all of them and conflates and superimposes certain of their elements in instructive ways. {12}

The first thing to note about Figure 1.5 is that the line *abcde* conflates the two lines in Fig. 1.1, IK representing time and GH representing distance understood as displacement in uniform motion. {13}The intervening theorems have taught us that it is precisely because line GH represents displacement in uniform motion that it can be merely a line; non-uniform motion requires either a two-dimensional figure or a ruler-with-commentary for its representation. However, in the case of uniform motion, a line suffices and moreover can also serve to represent time, since (as the theorem accompanying Fig. 1.1 states) in such motion the intervals of time elapsed are proportional to the intervals of distance traversed. The line *bogln* is the line HLMNI from Fig. 1.4 divided in just the same proportions. The genius of the diagram is the perpendicular juxtaposition of line *bogln* with *abcde*, which represents the insight that projectile motion is 'compounded of two other motions, namely, one uniform and one naturally accelerated.' {14}The proof of Theorem I, Proposition I in the section 'The Motion of Projectiles' shows that the rest of the diagram stems from the superposition of Apollonius' construction of the parabola as the pathway of the moving body: 'A projectile which is carried by a uniform horizontal motion compounded with a naturally accelerated vertical motion describes a path which is a semi-parabola.'{15}

In order for the reasoning in the proof of Theorem I, Proposition I of the *Fourth Day* to proceed, the line *abcde* must mean both time and distance; it must represent time symbolically in order for the application of the results achieved in the *Third Day* and it must represent distance qua displacement in order for the diagram to make sense as the icon of a trajectory, the movement of a body across a plane in space. The line segments HL, HM, HN from Figure 1.4, re-labeled *ci*, *df*, *eh* in figure 1.5, are drawn to represent the vertical displacement at equal intervals of time / displacement at *c*, *d*, and *e*; *b* is the point taken to represent the beginning of the projectile motion, *cb* is chosen as the 'unit' and *cb* = *dc* = *de* and so on. Galileo's exposition of the diagram claims that no matter how *cb* is chosen ("if we take equal time-intervals of any size whatsoever") the curve described is always the same, and it is the semi-parabola, as the results from Apollonius that precede the proof in Theorem I, Proposition I have made clear. Thus the best way to understand projectile motion is as 'uniform horizontal motion compounded with a naturally accelerated vertical motion,' which produces a parabolic downward trajectory. Reading *cb* as a finite interval allows for the application of results of Euclid and Apollonius; reading *cb* as an infinitesimal allows the diagram to stand as an analysis of accelerated motion. The great diagram that presents projectile motion thus succeeds because of Galileo's inspired handling of controlled ambiguity.

2. Presuppositions about Language and Thought in Mathematics that stem from Carnap and the Vienna School

The reductionist program set out in Rudolf Carnap's *The Logical Structure [Aufbau] of the World* can only conclude that Galileo's text should be re-written. {16} Though the program has long been regarded as inconclusive and ultimately unsuccessful by most Anglophone philosophers of science and mathematics, many of its presuppositions nonetheless remain unnoticed in our discourse and thinking. I now return briefly to Carnap's classic work, in order to uncover some of these assumptions and question them more closely. Then I will give a brief sketch of what became of Carnap and Hempel's view of scientific and mathematical knowledge in the work of their academic children (Bas van Fraassen, Nancy Cartwright, Margaret Morrison, Ian Hacking) and grandchildren (Robin Hendry, Ursula Klein, myself), a development that will bring us back to the project of this essay in an unexpected way.

Carnap begins the book with a description of his ideal, a 'constructional system' that begins with certain fundamental objects / concepts and constructs all other objects / concepts from them. Trying to avoid the antinomy between rationalism and empiricism, he argues that to every concept there belongs one and only one object: 'the object and its concept are one and the same.' {17} An object / concept is said to be reducible to one or more other objects / concepts if all statements about it can be transformed into statements about the other object / concepts via a constructional definition. This is a 'rule of translation which gives a general indication how any propositional function in which *a* occurs may be transformed into a coextensive propositional function in which *a* no longer occurs, but only *b* and *c*.' {18} Then we say that *a* is logically reducible to *b* and *c*. The example that Carnap gives to illustrate his point is pertinent to our discussion:

EXAMPLE. The reducibility of fractions to natural numbers is easily understood, and a given statement about certain fractions can easily be transformed into a statement about natural numbers. On the other hand, the construction, for example, of the fraction 2/7, i. e., the indication of a rule through which all statements about 2/7 can be transformed into statements about 2 and 7, is much more complicated. Whitehead and Russell have solved this problem for all mathematical concepts [Princ. Math.]; thus they have produced a "constructional system" of the mathematical concepts. {19}

Earlier in the book, he tells us, 'all real numbers, even the irrationals, can be reduced to fractions. Finally, all entities of arithmetic and analysis are reducible to natural numbers.' {20} And in the *Preface* to the Second Edition, he adds that Frege, Russell, and Whitehead had shown, 'through the definition of numbers and numerical functions on the basis of purely logical concepts, the entire conceptual structure of mathematics […] to be part of logic.' {21} This highly controversial last statement was, significantly, written in 1961; the last section of my book revisits the controversy.

The alleged success of Russell and Whitehead at reducing all of mathematics to logic inspires Carnap: 'Logistics (symbolic logic) has been advanced by Russell and Whitehead to a point where it provides a theory of relations which allows almost all problems of the pure theory of ordering to be treated without great difficulty.' His book is thus designed to 'apply the theory of relations to the task of analyzing reality…in order to formulate the logical requirements which must be fulfilled by a constructional system of concepts, to bring into clearer focus the basis of the system, and to demonstrate by actually producing such a system (though part of it is only an outline) that it can be constructed on the indicated basis and within the indicated logical framework.' {22} He adds, if this project is successful it will show 'that there is only one domain of objects and therefore only one science.' {23}

My aim in this chapter is not to evaluate Carnap's overall project, but to note its reductionism (and its optimism about reductionism, even in the face of much evidence to the contrary), and to focus especially on its theory of language. Even philosophers of science and mathematics who reject the constructionist *Aufbau* of the world have accepted many of Carnap's claims about language. For example, he writes that a constructional definition must be "pure," that is, free of unnoticed conceptual elements; and it must be 'formally accurate,' that is, 'it must be neither ambiguous nor empty… it must not designate more than one, but it must designate at least one, object.' He notes that in natural language this requirement is difficult to fulfill, but by contrast 'this requirement is easily and almost automatically fulfilled when we apply an appropriate symbolism, for example, when we apply the logistic forms for the introduction of classes or relation extensions and for definite descriptions of individuals. It is a fact of logistics that these forms guarantee unequivocalness and logical existence, for they have been created with these desired properties in view.' {24}

In sum, the ideal language for philosophy of science and mathematics, the language in which science and mathematics are to be re-constructed in order to exhibit the real structure of the world, is 'the symbolic language of logistics.' {25} It does the best job of demonstrating that all objects are reducible to the basic objects: 'It is obvious that the value of a constructional system stands or falls with the purity of this reduction, just as the value of an axiomatic exposition of a theory depends upon the purity of the derivation of theorems from axioms. We can best insure the purity of this reduction through the application of an appropriate symbolism.' {26} The symbolic language of logistics is allegedly an ideal mode of representation that makes all content explicit; it stands in isomorphic relation to the objects it describes, and that one-one correspondence insures that its definitions are 'neither ambiguous nor empty.' It is, obviously, symbolic and not iconic; in the ideal limit, it will replace—not merely supplement—natural language. And its successful use in the *Aufbau* of the world will show that there is only one kind of thing: Carnap's choice of basic object is the sense datum, but he also believes that in the end mathematics has no subject matter, having been reduced to the pure formalism of logic.

3. From the Syntactic to the Semantic to the Pragmatic Approach

Galileo's account of projectile motion in the *Discorsi* thus appears to fall woefully short of Carnap's ideal. It involves icons and natural language as well as symbols; and many of the modes of representation that it employs, refer ambiguously. Should we commit it to the flames? Carnap's judgment would probably not be so severe; he might suggest we review it as a curiosity, admirable in its time but philosophically inert for us. This is one reason why he was not interested by the history of mathematics. By contrast, I find in my historical case study evidence that is philosophically pertinent in its own right, and that weighs strongly against many of the assumptions made by logical positivists such as Carnap, and in favor of a very different philosophical view of mathematics and indeed of language and logic.

One way to make clear the nature of my quarrel with Carnap and mid-twentieth-century logical positivism is to sketch a philosophical development that links us, which I understand to be driven by problems articulated but unsolved by his program. It is not unfair to characterize Carnap's project in the *Aufbau* as essentially syntactical, for it reduces content to the sense datum (which really has no content) and tries to build everything else into the form. As Robin Hendry sums up: 'The logical positivists bequeathed to philosophy of science a characterization of theories as linguistic structures whose content was to be identified in terms of the notion of logical consequence, a notion intimately related to structural features of their formulations in canonical formal languages.'{27} If we recall that logical positivists like Carnap and Hempel gave an account of the relations between theory and evidence, between explanation and the *explanandum*, and between the reducing and reduced theory in terms of deductive (only sometimes inductive) relations among sets of sentences in a formal language, the approach appears unrelentingly syntactic.

The philosophical offspring of these mid-twentieth century philosophers concluded that the study of formal languages would not in itself provide a deeper epistemological account of how scientists and mathematicians represent reality. I would put it this way: logical inference can be formalized, though we would do well to keep in mind that formalization is a kind of representation, and that formalization *qua* representation tends to precipitate novel ideal items like sets, well formed fomulas, and recursive functions, which must then be studied. But the things of mathematics and science must be represented in order to be studied, and we cannot understand the problems or procedures they occur in and give rise to without looking closely at how they are represented. Representation is a much broader notion than formalization; and formalization suits *inference*, which is indifferent (up to a point) to the things it treats.

Thus, the semantic philosophers turned their attention to 'models,' and a new school of Anglophone philosophy of science that characterized its approach as semantic began to dominate philosophical debates. Some of these philosophers thought of models in the sense used by logicians, as a structure that satisfies some set of sentences in a meta-language, where what is meant by 'structure' is a set of sentences in an object-language; Bas van Fraassen, inter alia, had a rather more conservative view of what constitutes a model. But others urged a broader and richer account. As Robin Hendry observes, 'This logical notion is quite different, however, from the sense of 'model' that is at work when we speak of (for instance) the molecular model that we can construct from one of Linus Pauling's kits: here the important relation is not satisfaction but representation.'{28} Once the notion of model is investigated along these lines, it becomes clear that different models bring out different aspects of the real systems they model with different degrees of precision and explanatory power. Philosophers who pursue a more broadly semantical philosophy of science, like Nancy Cartwright, Margaret Morrison, Kenneth Schaffner, and Ian Hacking, tend to be interested in the activity of contemporary scientists, not only as they justify their results in journals but as they discover them in the laboratory and the field.

All the same, as we see in recent works by van Fraassen and Schaffner especially, the adequacy of a scientific theory is characterized in terms of a relation of isomorphism between theory and model. {29} That is, a theory is said to be empirically adequate when a model of the appearances (the quantitative results of experiment) is isomorphic to a (mathematical) sub-model of one of the theory's models. {30} It is often assumed that the best way to think of representation in mathematics is in terms of isomorphism between structures, and this habit can then be simply and appropriately transferred to science. But the philosophical children of the semanticians (and grandchildren of the logical positivists) have concluded otherwise, finding the semantic view of representation problematic not only for philosophy of science but also for philosophy of mathematics. They propose instead a view that encompasses pragmatic as well as syntactic and semantic considerations, focusing on the successful posing and solution of problems in a context of use that is, in the end, historical. Thus the program of Carnap—which, like the program of Kant, eschews history—has been transformed into a program that finds it cannot escape history after all.

Ursula Klein concedes that the semantic notion of isomorphism might capture the notion of 'representation of,' or denotation, but 'must be supplemented by 'representation as' or meaning. A representation *A* of an entity *B* is not merely a denotation of it, but also creatively describes and classifies it as such-and-such. Representation… is not a matter of passive reporting… Rather, representation involves organization, invention, and other kinds of activity.'{31} When isomorphism is the central term in the analysis of scientific language, the philosopher assumes that the objects so related are already available and organized in a definitive way. But representation itself may have a role in constituting and organizing the things represented. Klein argues, for example, that the use of Berzelian formulas (the familiar formulas like H_2O used by chemists) was crucial to the swift advance of organic chemistry in the mid-nineteenth century, not because it was 'more isomorphic' to experimental patterns recorded in the laboratory than the notations preceding it, but because it enjoyed a useful, meaningful iconicity, ambiguity, and algebraicity. {32} I discuss her case study in more detail at the end of this chapter.

Hendry reminds us that the notion of isomorphism in fact does not even account very well for denotation in science. One notorious difficulty with first-order predicate logic is that, in most non-trivial cases, a first order theory describes correctly a whole range of models that satisfy it, and cannot by itself pick out the intended model. Hendry argues that the context of natural language in which symbolic language is used makes its reference a determinate, not merely stipulative relation.

'The particular historical and material context of a language within which a theoretical discourse is pursued is what endows it with reference, and reference can be passed on to other media (like equations) which become entwined in that discourse.' {33} Whereas isomorphism is reflexive and symmetric, representation is irreflexive and asymmetric due to its intentionality.

Moreover, Hendry observes, supposing we find a way to single out the intended model, there are always uninteresting ways of constructing a theory that will stand in the relation of isomorphism to it: the question is then, how to articulate and explain the selection of an *interesting* theory. Klein and Hendry both argue that interesting modes of representation contribute to the advance of scientific knowledge, that is, to success in posing and solving problems. And when we look at the details of their case studies, the representations in those interesting theories turn out to be iconic as well as symbolic, often ambiguous, embedded in natural language, and partially constitutive of what they stand for. Thus the semantic reliance on the notion of isomorphism appears misplaced.

My own work in the philosophy of mathematics over the years has led me to the same conclusion about mathematics, an insight missed even by Hendry and Klein who sometimes write as if the semantic approach might work well for mathematics but not for the empirical sciences. Van Fraassen and Schaffner emphatically assume that the relation between theory and thing in mathematics is successfully captured by isomorphism between symbolic meta-language and symbolic object-language, and for the same kind of reason Carnap assumes the syntactic reduction of mathematics to logic is successful. They all suppose that their epistemological account (syntactic or semantic) works well for mathematics and, since mathematics is the language of science, it can be transferred without much difficulty to science itself. My purpose in this book, by contrast, is to move towards an epistemology that works properly for mathematics by taking into account the pragmatic as well as the syntactic and semantic features of representation in mathematics. Focusing on the pragmatic dimension of mathematical language allows us to see the philosophical interest of useful ambiguity in mathematics, as well as the limits of formalization.

Carnap wanted to rewrite science (and mathematics) in the language of logic, in order to exhibit the logical structure of reality. Thus for him the role of language is to purify and correct. It should render every inferential step explicit; stand in isomorphic relation with the objects / concepts it refers to; and either reduce content to form or show that there is only one kind of thing. Recall that Carnap's preferred 'thing' is the sense datum, which does not in itself have any content. (The preferred 'thing' of mathematical structuralists is either the point or the empty set, for the same reason, that each lacks content.) Carnap is an enthusiastic champion of the Russellian project of reducing geometry and analysis to arithmetic and arithmetic to logic. He is also interested in the reductionist project of reducing the things of biology and chemistry to the electron (and, he reluctantly adds, the proton). Like that other reductionist champion of conceptual purity, Descartes, Carnap downplays the difficulty of arriving at only one kind of thing. Descartes was left with the *cogito*, the line segment, the particle in inertial motion, and the mechanism; Carnap is left with the sense datum; the proposition; and the electron (and—darn it!—the proton). But the ideal remains.

By contrast, I (along with my pragmatist cohort) regard the role of representation to be the successful discovery and solution of problems about problematic things, heterogeneous things of many different kinds. My exposition of Galileo's analysis of projectile motion in the *Discorsi* is designed to show the fruitful employment of consortia of modes of representation at work in his argument, as well as their inescapable ambiguity; in a later chapter I will show how his procedures lend themselves to further generalization in Newton's use of Galileo's results in the *Principia*. The logicist account of mathematical language makes this useful ambiguity impossible to see, because it tries to eliminate modes of representation that are 'different'—that is, only one mode of representation at a time is countenanced—and it insists that all referring be univocal. To provide a more adequate approach, I now turn to the notion of analysis and its allied understanding of mathematical experience, illustrated with a chemical example developed by Ursula Klein.

4. A pragmatic Account of Berzelian Formulas

Mathematics invites us to study intelligible but problematic things that are hard to investigate because they cannot be directly perceived and because they involve the infinite. We search for the conditions of intelligibility of these things (which are themselves conditions of the intelligibility of other things) as we search for the conditions of solvability of the problems that characterize them and the reach of procedures we use in the solutions. I discuss this process in the next chapter, under the general term *analysis*. Our search depends on representations that make the invisible visible and the infinite finite; I argue throughout this book that we study the things of mathematics by a kind of triangulation, where we combine different modes of representation in order to improve our access to them. Formalization in terms of predicate logic is one kind of representation among many, useful for some purposes but limited in important ways. Different representations reveal different aspects of intelligible things, the problems in which they occur, and the procedures that successfully solve problems. When distinct representations are juxtaposed and superimposed, the result is often a productive ambiguity that expresses and generates new knowledge. Mathematical *experience* emerges from traditions of representation and problem-solving, as it explores the 'combinatorial spaces' (to use Jean Cavaillès's phrase) produced in polyvalent mathematical discourse. It cannot be summed up by the formalism of predicate logic or abstract algebra or any single mode of representation, nor by the 'intuition' variously invoked by Descartes, Kant or Brouwer in an attempt to escape the traps of formalism. Formalism, structuralism, and intuitionism are all intolerant of ambiguity, as a consequence of exaggerated epistemological ambitions.

Some mathematical representations are iconic, that is, they picture and resemble what they represent; some are symbolic and represent by convention, without much resemblance; and some are indexical, representing for the sake of organization and ordered display. The diagram that accompanies Euclid's proof of the Pythagorean Theorem is an example of the first; the polynomial equation that represents a circle in Descartes' *Geometry* is an example of the second; and the Gödel numbers that name well formed formulas of a certain specified formal language are an example of the last. But all icons have a symbolic dimension, as all symbols have an iconic dimension; and all representations to a certain extent organize, order, and display. Which representations we have at our disposal and how we combine them determines how we can formulate and solve problems, discern items and articulate procedures, supply evidence in arguments and offer explanations. And how the representations should be understood, their import and meaning, must be referred to their use in a given tradition of problem-solving. Thus, I believe that mathematical rationality must be studied in historical context. To underline this point, I turn to an example from Ursula Klein's book *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century*. {34} I find this example so instructive, along with Robin Hendry's and Robert Bishop's studies of the applications of quantum mechanics to chemistry and the insights of Roald Hoffmann into organic chemistry set out in a co-authored paper that forms the basis of Chapter 3, that I have begun my book on philosophy of mathematics with an excursus into chemistry and geometry.

Klein's case study concerns the impact of Berzelian formulas, introduced by the Swedish chemist Jacob Berzelius in 1813, on the emergence of organic chemistry between 1827 and 1840. During that period, plant and animal chemistry—an experimental practice concerned with the extraction and description of organic substances—changed into carbon chemistry, in which quantitative analysis and the experimental study of chemical reactions led to the identification, classification and construction of a wealth of new chemical objects in the late 19th century. Her main thesis is that the catalyst of this change was the wide-spread adoption of Berzelian formulas by organic chemists, which exhibit the concatenation of chemical components in a simple and perspicuous way. (Berzelius wrote the numerical subscripts as superscripts.) Its algebraicity is striking. Klein makes two interesting observations in an initial chapter entitled 'The Semiotics of

Berzelian Chemical Formulas,' one about the ambiguity of the notation and another about its abstractness. The former is that the letters in Berzelian formulas are signs that convey a plurality of information simultaneously: depending on context, they may refer to macroscopic chemical compounds and their composition from chemical elements; to stoichiometric or volumetric quantitative relations; and to atomic weights or 'atoms' in the sense of sub-microscopically small particles. In the new theoretical context, a chemical proportion meant a bit or portion of a substance defined by its unique and invariant relative combining weight. Thus, Berzelian formulas became signs for *scale-independent* chemical portions. The ambiguity of the notation allowed chemists to move back and forth between the macroscopic and microscopic worlds as needed.

The abstractness of the notation also freed those who used Berzelius' formulas from a commitment to an elaborate foundational theory, like that of Dalton. Dalton proposed a (prematurely) iconic notation intended to be read as unequivocal signs for sub-microscopically small atoms that have a certain shape, size, orientation in space, and additional chemical properties; he seemed to view his diagrams as realistic images of atoms. By contrast, Berzelius's formulas are noncommittal. In Chapter 1 of his book *Force and Geometry in Newton's* Principia, François De Gandt makes a similar point about Newton's abstract definition of a center of force as that point with respect to which equal areas are swept out in equal times by a body subject to the action of the centripetal force. {35} The definition is metaphysically noncommittal, which meant it could be used in succeeding generations by a gamut of physicists even as they argued over the metaphysics of force. So too Berzelius's formulas served both anti-atomists like Berthelot and atomists like Wurtz and Grimaux, even as they argued over the reality and nature of atoms, and the relation between physics and chemistry. In Klein's words, Berzelian formulas 'had different layers of meaning and conveyed a building-block image of chemical portions without simultaneously requiring an investment in atomic theories.' {36}

However, Klein disputes the way that historians of chemistry have emphasized the merely symbolic nature of Berzelian formulas, as if his notation were just convenient shorthand that allowed chemists to evade certain issues of explanation. In particular, she challenges François Dagognet, whose books *Tableaux et Languages de la Chimie* {37} and *Ecriture et Iconographie* {38} deal with many of the same issues as her *Experiments, Models, Paper Tools.* Dagognet insists upon a strong distinction between symbols and icons, and in particular contrasts the 'logical,' symbolic notation of Berzelius with the graphical, iconic structural formulas of Archibald Scott Couper and the stereochemical formulas of Jabobus van't Hoff. On Dagognet's account, only the latter are paper tools with a 'generative function' above and beyond the mere conveyance and storing of information. Klein contests this dismissal, first by questioning a rigid dichotomy between iconic and symbolic modes of representation, and second by marshalling the details of her case study, which show convincingly (to my mind) the generative function of Berzelian formulas.

Invoking and then thinking beyond Nelson Goodman's criticism of the icon / symbol distinction in his *Languages of Art*, Klein points out, quite rightly, that any iconic notation is incompletely iconic and must involve certain symbolic conventions, and that any symbolic notation is iconic in a rudimentary way: typographical isolation, for example, is iconic in intent. 'The fact that a letter is a visible, discrete, and indivisible thing (unlike a written name) constitutes a minimal isomorphy with the postulated object it stands for, namely, the indivisible unit or portion of chemical elements.' {39} I came to similar conclusions about the iconic dimensions of symbols and the symbolic dimensions of icons in mathematics, as will be evident in the chapters that follow, so I could not agree more with this point. Klein goes on to explain briefly (borrowing Goodman's terms) that Berzelian formulas have both 'graphic suggestiveness' and 'maneuverability.' They exhibit clearly the constitution of compounds in ways that verbal descriptions and Daltonian diagrams did not; and, given the goal of constructing models of compounds and chemical reactions, Berzelian formulas were much easier to use. Leibniz, who always insisted on the generative capability of a good 'characteristic,' would have been delighted by the new notation of Berzelius.

Klein contrasts the old mode of organic chemistry with the new mode that emerged around 1830 by showing that what counted as a scientific object changed drastically, along with the concept of 'organic substance' and its material referents in the laboratory; and so too did the system of classification, the type of experiments performed, and the kinds of problems to be solved, the goals and beliefs of scientists. Thus, she argues, pre-1830 plant and animal chemistry and post-1840 carbon chemistry should be considered two different scientific cultures; and her subsequent exposition makes a good case that Berzelian formulae were a centrally significant agent of change in that metamorphosis. Examining the interaction between improved laboratory methods of quantitative analysis in the nineteenth century, improvements that nonetheless remained limited, and the recording of laboratory events in terms of Berzelian formulas, Klein writes, 'Berzelian formulas… presupposed quantitative chemical analyses of the substances at stake. They were products of the transformation of the analytical results into small integral numbers of "portions" (or "atoms") of elements in that the elemental composition of the substances expressed in weight percentages was divided by the theoretical combining weight (also "atomic weight") of each element.' {40} In 1827, Jean Dumas and Polydore Boullay revisited an earlier experiment involving the discovery of 'sulfovinic acid,' a reaction product that appeared at the beginning of the process of distilling alcohol. The problem of interpreting the reactions in question was the multiplicity of reaction products. Using Berzelian formulas, the two chemists demonstrated that there were two simultaneously occurring but fully distinct reactions between alcohol and sulfuric acid; that is, they brought order to the chaotic cascade of reaction products by distinguishing independent parallel reactions as well as a successive reaction.

In the detail of her exposition, Klein shows that the modeling of the reaction depended on the use of Berzelius's notation, because it exhibited so well the balancing of the masses involved in the interpretive model. The balance of the number of element symbols (C, H, O, and S) on the left and right sides of the chemical equations must exhibit the equality of the masses of the initial substances and the reaction products. Thus in the two parallel reactions, and the reaction subsequent to them, the chemists could show formally that all reaction products had been accounted for. As Ursula Klein argues, in summary, ‘balancing schemata of organic reactions could not be constructed based on the measurement of the masses of the reacting substances, but required the acceptance and application of theoretical combining weights such as those represented by Berzelian formulas. {41} In the early nineteenth century, it was difficult and often impossible to isolate and measure all the pertinent masses of the substances entering into and resulting from reactions; Berzelian formulas permitted the construction of exact models in the partial absence of experimental data.

Around 1840, Jean Dumas and Auguste Laurent developed a new chemical concept that was incompatible with the theory of the binary constitution of organic substances (a theory that held sway for a couple of decades before 1840), the concept of substitution. Klein shows that the new concept depended just as heavily on ‘work on paper’ with Berzelian formulas as the predecessor theory had. The example given is Dumas’ interpretive models for the formation of ‘chloral,’ a reaction product discovered by Justus Liebig when chlorine gas was introduced into alcohol and heated up. The last of these models, offered around 1835, involved a rule that one ‘atom’ of hydrogen of an organic substance can be substituted by one ‘atom’ of chlorine, bromine, or iodine, or half an ‘atom’ of oxygen; this was the basis of what Dumas called his theory of substitution. Moreover, Dumas claimed that the formation of chloral proceeded step by step creating a series of intermediate compounds by a step-wise substitution of one portion of hydrogen by one portion of chlorine. His model, expressed in Berzelian formulas, visually demonstrates the step-wise substitution process and shows that none of the compounds demanded by the theory of binary constitution are involved.

Klein defends her bold claim that Dumas and his assistant Laurent owed this theory first and foremost to Berzelian formulas considered as a paper tool on the following grounds. The concept of substitution could not have been derived from any existing chemical theories. No empirical result of experimentation suggested that there was a discrete, step-wise process of substitution. And no prior, explicitly stated goal or aim of Dumas' contributed directly to the introduction of this rule and theory; on the contrary, it undermined the theory to which he had previously given allegiance. Thus Klein sees here a strong interplay between paper tools and theory construction, precisely the same kind of dialectic that Ian Hacking and Andrew Pickering have noted between laboratory apparatus and the modes of data analysis they foster, and theory construction.

Her discussion of this historical process of transformation in the mid-nineteenth century touches on a number of issues that arise in the next chapter. The first is that in nineteenth century chemistry, alcohol and its derivatives are 'model substances' in much the same way that *Drosophila* is for genetics in the early twentieth century. The reasons for this are both natural (alcohol is a very reactive substance that can be transformed easily) and cultural (human beings have been experts in distilling alcohol cheaply and abundantly since the Neolithic era, for obvious reasons). Also, distilled alcohol is relatively pure, and alcohol contains only carbon, hydrogen, and oxygen, the elements essential to and typical of organic chemistry. Chemists found it relatively straightforward to reconstruct the cascade of reaction products in reactions involving alcohol, and to assimilate the study of alcohol and its derivatives to the methods of inorganic chemistry. The existence and central importance of model objects in science support a distinction between generality and abstraction as a goal of scientific (and mathematical) theories. This is a distinction central to the research of another philosophical historian of mathematics, Karine Chemla, which I discuss in the next chapter, for the use of canonical items is also essential to the elaboration of general methods of investigation in mathematics.

The second point addresses Thomas Kuhn's notion of theory incommensurablity. {42} Observing that the scientific culture of chemistry in 1827 and its counterpart in 1840 were incompatible, she qualifies her claim: 'I use the term *incommensurability* not for pointing out a problem of theory choice and theory justification. Rather, I use it as an analytical category to denote the different modes of identifying and classifying the scientific objects, which makes it impossible for us historians of science to compare the two cultures of organic chemistry in any direct way.' {43} She adds that her conception of a scientific culture is a fragile, ambiguous unity of heterogeneous elements, which are not fully determined by any unifying theory. Thus (as her historical narrative shows) the incommensurable cultures of 1827 and 1840 are rationally related by overlapping practices. Defending the generative role of Berzelian formulas in this transformation, Klein reiterates her claim that the innovations they precipitated were not derived from previously existing theory or forced upon researchers by experimental data. Scientists neither intended now foresaw the changes the new notation brought about or the exponential growth of artificial substances in synthetic carbon chemistry it precipitated. 'Paper tools' spur and direct human intentions and actions. All the same, Klein challenges the conclusion that a philosopher like Bruno Latour draws from the power of paper tools and laboratory instruments to channel and organize research: for him, human agency is only secondary when science is understood as a series of chains of inscription. By contrast, Klein invokes the tradition of pragmatism, like Robin Hendry and Nancy Cartwright who have worked through the limitations of syntactic and semantic approaches to scientific knowledge. The dialectical interplay between scientists and their conscious objectives, and the tools they employ to realize those objectives, must be reconstructed in terms of the pragmatics of problem-solving. And I shall do the same in this book. Formal languages play an important role in modern mathematics; but formalization is only one kind of representation. And what eludes complete formalization is legion: the intelligible thing, the constitution of 'combinatorial spaces,' the knower in relation to both item and representation, the generalization of procedures, how representations may be juxtaposed and exploited in juxtaposition, formalization itself, productive ambiguity, and in general mathematical rationality.

Chapter 1 Notes

1 Galileo Galilei, *Dialogues Concerning Two New Sciences*, tr. Henry Crew and Alfonso de Salvio, (New York: Dover, 1914 / 1954); hereafter referred to as *Discorsi*.

2 The philosophical use of the polar terms 'icon' and 'symbol' are due to C. S. Peirce, who distinguished the former as similar to their objects, and the latter as linked to their objects only by convention. In recent essays, I have made use of the distinction, while insisting on the iconic dimensions of symbols and the symbolic dimensions of icons. Peirce also uses the term 'indexical' which I note but make less use of. See C. S. Peirce, "Pragmatism as the Logic of Abduction," in *The Essential Peirce*, Vol. 2 (Indianapolis: Indiana University Press, 1998), 226-241.

3 Galileo, *Discorsi*, 153-156.

4 Ibid., 155-156.

5 See Edith Sylla, 'Compounding Ratios,' in E. Mendelsohn, ed., *Transformation and Tradition in the Sciences* (Cambridge: Cambridge University Press, 1984), 11-43.

6 Galileo, *Discorsi*, 157-158.

7 Ibid., 173-74.

8 A. Koyré, 'La loi de la chute des corps,' in *Etudes galiléenes*, Paris: Hermann, 1939, 11-46. See also my 'Descartes and Galileo: The quantification of time and force,' in *Mathématiques et philosophie de l'antiquité à l'age classique: Hommage à Jules Vuillemin*, Roshdi Rashed, ed. (Paris: Editions du Centre National de la Recherche Scientifique, 1991) 197-215.

9 Galileo, *Discorsi*, 174-175.

10 Ibid., 157-158.

11 Ibid., 175-176.

12 Ibid., 244-257.

13 Ibid., 248-250.

14 Ibid., 244

15 Ibid., 245.

16 Rudolf Carnap, *The Logical Structure of the World* and *Pseudoproblems in Philosophy*, tr. B. Rolf and A. George, (Berkeley: University of California Press, 1967 /69); *Der Logische Aufbau der Welt* (Hamburg: Meiner, 1928).

17 Ibid, 10.

18 Ibid, 61.

19 Ibid, 61.

20 Ibid, 6.

21 Ibid., p. vii.

22 Ibid., 7-8.

23 Ibid., 9

24 Ibid., 154.

25 Ibid., 153.

26 Ibid., 154.

27 Robin F. Hendry, 'Mathematics, Representation, and Molecular Structure,' *Tools and Modes of Representation in the Laboratory Sciences*, ed. U. Klein, (Dordrecht: Kluwer, 2001), 221-236.

28 Hendry, Ibid., 225.

29 See, for example, *Laws and Symmetry* by Bas van Fraassen (New York: Oxford University Press, 1990), and *Discovery and Explanation in Biology and Medicine* by Kenneth Schaffner (Chicago: University of Chicago Press, 1994).

30 See, for example, Schaffner, *Discovery and Explanation*, Ch. 3.

31 Klein, *Tools and Modes of Representation*, p. viii.

32 Ursula Klein, *Experiments, Models, Paper Tools: Cultures of Organic Chemistry in the Nineteenth Century* (Stanford, Ca.: Stanford University Press, 2003), Ch. 1. I wrote a review essay about this book in Studies in History and Philosophy of Science 36 (2005), 411-417.

33 Hendry, 'Mathematics, Representation and Molecular Structure,' 227.

35 Klein, *Experiments, Models, Paper Tools*.

35 François De Gandt (Princeton: Princeton University Press, 1995).

36 Klein, *Experiments, Models, Paper Tools*, 35.

37 (Paris: Editions du Seuil, 1969)

38 (Paris: Librairie Philosophique J. Vrin, 1973)

39 Ibid., 26.

40 Ibid., 119.

41 Ibid., 126.

42 *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1996).

43 Klein, *Experiments, Models, Paper Tools*, 221-222.