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GALILEAN IDEALIZATION

> Really powerful explanatory laws of the sort found in theoretical physics do not state the truth . . . We have detailed expertise for testing the claim of physics about what happens in concrete situations. When we look to the real implications of our fundamental laws, they do not meet these ordinary standards . . . We explain by *ceteris paribus* laws, by composition of causes, and by approximations that improve on what the fundamental laws dictate. In all of these cases, the fundamental laws patently do not get the facts right.[1]

IN GALILEO'S dialogue, *The New Sciences,* Simplicio, the spokesman for the Aristotelian tradition, objects strongly to the techniques of idealization that underlie the proposed 'new science' of mechanics. He urges that they tend to falsify the *real* world which is not neat and regular, as the idealized laws would make it seem, but complicated and messy. In a provocatively titled recent book, Nancy Cartwright argues a similar thesis, although on the basis of very different arguments to those of Simplicio. Her theme is that the theoretical laws of physics, despite their claims to be fundamental truths about the universe, are in fact false. They *do* have broad explanatory power, and therein lies their utility. But explanatory power (in Cartwright's view) has nothing to do with truth; indeed, the two tend to exclude one another. Idealization in physics, though permissible on pragmatic grounds, is thus not (as the Galilean tradition has unifor~~mly~~ assumed) truth-producing.

In this essay, I plan to review some of the characteristic techniques of what may broadly be called 'Galilean idealization', and to inquire briefly into their epistemic implications in the natural sciences. I will leave the issues raised by the connected topic of the composition of causes to another paper, in the effort to get straight first what sorts of idealization the 'new science' *did* usher in. My approach will be conceptual – historical. I will make use of texts, mainly from Galileo, in order to clarify the various sorts of 'idealizing' moves. Although I will be concerned on occasion to assign historical responsibilities for these moves, my main intent is not the historical one of inquiring into the origins of the 'idealizing' technique. This would bring us back through the long story of the methods of analysis and synthesis, as these were employed in the Renaissance and Middle Ages, to the abstractive theories of Aristotle and

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[1] N. Cartwright, *How the Laws of Physics Lie* (Oxford: Clarendon Press, 1983), p. 3.

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to Plato's notion of Form. My aims rather are, first, the systematic one of discovering what the techniques were and, second, the epistemological one of deciding whether they need be inimical to the truth-likeness of science.

The term, 'idealization', itself is a rather loose one. I shall take it to signify a deliberate simplifying of something complicated (a situation, a concept, etc.) with a view to achieving at least at a partial understanding of that thing. It may involve a distortion of the original or it can simply mean a leaving aside of some components in a complex in order to focus the better on the remaining ones. The *point* of idealization (but here is the great divide between the Platonic and Aristotelian traditions) is not simply to escape from the intractable irregularity of the real world into the intelligible order of Form, but to make *use* of this order in an attempt to grasp the real world from which the idealization takes its origin.

I have included several techniques below that are not usually called 'idealization', but which do, nevertheless, qualify under the definitions above. By labelling them 'Galilean', I do not mean to imply that Galileo invented all of them or even that he had a major responsibility for all of them. The first one, as we shall see, is much older than Galileo. Nonetheless, I think it is legitimate to group them together as a unit under his name, because each of them played a distinctive part in shaping the 'new science' at its origins.

1. Mathematical Idealization

When Galileo was trying to establish the Copernican doctrine of the motion of the earth in his *Two Chief World Systems,* the most serious objection he faced is clearly and honestly stated in the Second Day of the dialogue. Since objects placed near the edge of a spinning horizontal wheel tend to fly off on a tangent, why don't objects on the earth do the same if (as Copernicus holds) it is spinning on its axis? Galileo's spokesman, Salviati, in response employs an elaborate geometrical analysis of the 'horn angle' between tangent and curve in an attempt to show that the initial departure of the curve from the tangent-line is so small that even the slightest tendency on the part of objects to fall towards the center of the earth will be sufficient to 'bend' their path into the circle required to keep them safely anchored to the earth's surface, no matter how fast the earth may be rotating. The argument is, as we know, fallacious and its conclusion wrong. Galileo did not have the dynamic concepts nor the techniques of differentiation he needed to resolve the challenge.

Our interest here, however, is in Simplicio's response. (Recall that Simplicio is enunciating what Galileo took to be the objections the Aristotelians of his day would be likely to express.)

> After all, Salviati, these mathematical subtleties do very well in the abstract, but they do not work out when applied to sensible and physical matters. For instance,

> mathematicians may prove well enough in theory that a sphere touches a plane at a single point, a proposition similar to the one at hand; but when it comes to matter, things happen otherwise. What I mean about these angles of contact and ratios is that they all go by the board for material and sensible things.[2]

Simplicio is challenging Galileo's plan to make geometry the language of physics. In his view, the Book of Nature is *not* written in the language of mathematics, as Galileo had ebulliently asserted in *The Assayer* ten years before.[3] Geometry is an abstraction, an idealization. It leaves aside the qualitative detail that constitutes the physical singular *as* physical. How then can it serve as the language of a science of nature? This is a fundamental objection to the entire Galilean program. And it has been voiced again and again since Galileo's time. Bergson and Husserl are only two of the distinguished philosophers who have pressed it in our own day.

It may be worth asking in parenthesis whether this objection would have been voiced in quite the same way by Aristotle himself. He did, of course, separate mathematics quite sharply from physics, partly on the basis of the degree of abstraction (or idealization) characteristic of each. Physics abstracts only from the singularity of the changing concrete object; mathematics abstracts in addition from qualitative accidents and change.[4] A physics that borrows its principles from mathematics is thus inevitably incomplete *as physics,* because it has left aside the qualitative richness of Nature. But it is *not* on that account distortive, as far as it goes.

Mathematics, in Aristotle's eyes, is a science of *real* quantity; it is a science of the quantitative aspect of the real word, not just of a postulated construct realm.[5] This is why he can use it so freely in his physics, in his discussions of the continuum, of falling motion, of planetary motions, to mention only some of the more obvious examples. Even in the disorderly sublunary realm, it still furnishes a reliable mode of analysis, as the analysis of the rainbow in his *Meteorology* takes for granted. Furthermore, Aristotle has no objections, on

[2] Drake translation (Berkeley: University of California Press, 1967), p. 203; *Opere,* 7, p. 229. The translations are in some cases slightly modified.

[3] *The Assayer,* in *The Controversy on the Comets of 1618,* S. Drake (translation) (Philadelphia: University of Pennsylvania Press), p. 183; *Opere* **6**, p. 232. Neither Plato nor Aristotle could have allowed this aphorism to pass, but for quite different reasons. Aristotle's reason (apart from a likely hesitation over the theological implications of the 'Book' metaphor) would not be that the language of mathematics is inappropriate to the 'Book', rather that it is not the *only* (nor the primary) language in which the Book is written.

[4] Aristotle, *Metaphysics* **VI**, 1; *Physics,* **II**, 2.

[5] Some of the formulae he uses are still Platonic in overtone and imply the characteristic Platonic separation between mathematics and the sensible order. But it seems fair to say that his mature view is that mathematics deals not with separable Forms but with quantitative characteristics which exist only as embodied in matter. For a full discussion, see A. Mansion, *Introduction à la physique aristotelicienne* (Louvain: Nauwelaerts, 2nd ed. 1946, ch. 5).

truth-grounds, to the 'mixed sciences' of astronomy, optics, mechanics, harmonics. They are not *physics*, of course; they cannot give a full explanation of the order of Nature. But it was Plato, not Aristotle, who said that embodiment in matter was a bar to the proper realization of geometrical concepts.

The long separation between natural philosophers and the exponents of the 'mixed sciences' through the Middle Ages and Renaissance contributed to a shift in the original Aristotelian position, one which may have been augmented by the influence of neo-Platonism. One notes a growing distrust among natural philosophers of broadly Aristotelian sympathies for those who use mathematics in the context of physical problems.[6] The story is a complicated one; the only reason to allude to it here is to suggest that Galileo was not wrong in attributing to Simplicio the scepticism concerning the role of mathematics in science of nature that *did*, in fact, characterize many of the Aristotelians of that day.[7]

Salviati's response to Simplicio is to point to an ambiguity in his objection. If he means that matter is such that when a sphere is realized in it, it may touch a plane at more than one point, this is demonstrably false. If on the other hand, he means that perfect spheres are never, in fact, realized in Nature, or as Simplicio puts it, that 'a metallic sphere being placed upon a plane, its own weight would press down so that the plane would yield somewhat',[8] then this may well be true. But from this it does not follow that *if* such a sphere *were* to be realized in Nature, it would not have the properties that geometry demands of it. Matter cann~~ot~~ *alter* those properties; it merely makes them difficult to reproduce exactly. Just as the businessman must allow for boxes and packings in computing the real from the observed weights of his wares, so the 'geometrical philosopher':

[6] See J. Weisheipl, *The Development of Physical Theory in the Middle Ages*, London: Sheed and Ward, 1959.

[7] In his book, *Explanatory Structures* (London: Harvester, 1978), Stephen Gaukroger argues that for Aristotle "mathematics is simply not applicable to reality" (p. 202), thus making Simplicio's objection a properly 'Aristotelian' one. He goes on to propose this as the fundamental point of separation between the "explanatory structures" characteristic of the old and the new sciences. (Yet he himself recalls in some detail Aristotle's use of mathematics when treating of the speed of fall, p. 211). The roots of his misunderstanding may lie in his characterization of 'abstraction', which he somehow sees as being contrasted with 'reality'. "If we are merely dealing with abstractions, we are dealing with a situation which is not 'real'" (p. 222). This leads him to contrast concept formation in Aristotle's physics (abstractive) with that in Galileo's physics (non-abstractive). Indeed the latter "is specified in terms of state-variables which may be quite alien to everyday experience" (p. 221). Yet he does not show that the two concepts, distance and time, in terms of which Galileo formulates his laws, *are* different from the corresponding 'abstractive' concepts of Aristotle. He thinks they *have* to be, or else they will not grasp the 'real'. But for Aristotle, they *did* grasp the real, even though only the quantitative aspect of it.

[8] *Dialogue*, p. 206; *Opere*, 7, 233.

> when he wants to recognize in the concrete the effects which he has proved in the abstract, must allow for the impediments of matter, and if he is able to do so, I assure you that things are in no less agreement than are arithmetical computations. The errors lie, then, not in the abstractness or concreteness, not in geometry or physics as such, but in a calculator who does not know how to keep proper accounts.[9]

This is a good response. An 'impediment' is not something which prevents, or lessens the force of, the application of mathematics to nature. Rather, it indicates a practical difficulty in realizing the simple relations of the mathematical system within the complexity of the material order.[10] How are we to know to what extent they *are* realized? By 'allowing for the impediments': the assumption is that impediments of this sort *can* be allowed for, that is, that their effects can be calculated. The grounds for this assumption are inductive. We can see whether in practice the science we base on it can be made to work for complex real situations. This response could have been given as easily by Aristotle as by Salviati.[11]

Salviati, it must be admitted, is not entirely single-minded in maintaining this view. He sometimes lapses back into a Platonic pessimism about "the imperfections of matter, which is subject to many variations and defects", and is "capable of contaminating the purest mathematical demonstrations".[12] But if this were the case, the Book of Nature would not really be written in the language of mathematics, or would, at least, be poorly written. Salviati much more commonly seems to take for granted that realization in matter is not a barrier to intelligibility in geometric terms, and that the consequences of 'impediments' due to the difficulty of applying simple geometrical concepts to

[9] *Dialogue*, p. 207; *Opere*, **7**, 234.

[10] For a helpful review of the Galilean texts where 'impediments' are discussed, see N. Koertge, 'Galileo and the problem of accidents', *J. Hist. Ideas*, **38** (1977), 389 – 408.

[11] There has always been a measure of controversy as to Aristotle's views on the contingency of action in the material order. The best-supported view seems to be that natures in their actions are, for him, fully determinate and not impeded by matter. The occurrence of 'chance' events, of events outside the regular and expected *telos* of particular natures, is due to the interaction in the material world of a host of natures which mutually affect one another and frequently impede the achievement of what should (in a teleological sense of 'should') happen. There need be no implication in this of an (in principle) unpredictability, of the sort that in Plato's eyes characterizes the sensible world. What makes an event a 'chance' one is not that, in principle, it could not have been foreseen (*i.e.* in Galileo's language, that the impediments might not have been allowed for), but that the outcome is one that lies outside the normal *telos* of the nature being considered. See J. Lennox, 'Technology, chance and Aristotle's theory of spontaneous generation', *J. Hist. Phil.*, **20**, 1982, 219 – 238.

[12] This occurs in the opening pages of *Two New Sciences* (*Opere*, **8**, 51) where he is discussing the relationship of size to other properties of body. (See the Drake translation (Madison: University of Wisconsin Press, 1974); we shall follow the pagination of the National Edition which is given there.) Galileo goes on then to make the thoroughly Platonic assumption that his discussion will abstract from imperfections of matter, "assuming it to be quite perfect and unalterable and free from all accidental change", in order that mathematics can be "rigorously" applied to it.

the complexities of the sensible world can themselves, in principle, be grasped.[13] More than once, he asserts that Nature is fully accessible to the inquirer who makes proper use of experiment and observation.

There was, of course, an assumption in all this, an assumption which Galileo took over from the empiricist tradition stretching back in this case to Aristotle and forward to Locke and beyond, that the concepts needed to geometrize space and time are, in fact, the simple ones drawn from everyday sense-experience, the ones for which Euclid had long ago provided a definitive grammar. It would not have crossed his mind that there might be an 'impediment' of a much more fundamental sort in regard to the application of Euclidean geometry to the world of sense. What if the barrier were to be an inappropriateness, a lack of fit, on the part of the geometry itself?

> It would be novel, indeed, if computations and ratios made in abstract numbers should not thereafter correspond to concrete gold and silver coins and merchandise.[14]

It was as simple as that. The primary qualities which characterize body are assumed to correspond exactly to our everyday notions of space and time. Because arithmetic and geometry provide a unique grammar for these notions, a science of body may in consequence be constructed. There was no element of hypothesis in this for Galileo. Though he was not interested in the technicalities of epistemology, he could have agreed with Aristotle that the basic concepts of space and time needed for a mechanics are directly derived from experience and can thus characterize the world of sense in an unproblematic way.

Space and time today no longer seem so simple. Not only are there alternative geometries, but in a more general way the empiricist assumption that primary qualities are somehow 'given' us directly, so that mechanics can start from a firm basis, would be questioned. Theoretical choices are involved, and they need to be tested in terms of their success. And 'success' will have to be spelled out in terms of a notion of science that itself requires warranting. The issues are familiar ones in contemporary philosophy of science, so there is no need to rehearse them here.

The consequences for our theme of mathematical idealization are quite far-reaching. What happened is that the 'mathematics' used in physics has gradually incorporated a greater and greater physical content. The Book of Nature is *not* written in the language of mathematics, strictly speaking. The

[13] See T. McTighe, 'Galileo's Platonism: A reconsideration', *Galileo: Man of Science*, E. McMullin (ed.) (New York: Basic Books, 1967), pp. 365 – 387.

[14] *Dialogue*, p. 207; *Opere*, 7, 234.

syntax is mathematical, but the *semantics* is not. And both semantics and syntax are needed to constitute a language. The semantics of such terms as 'mass' and 'energy' is physical, even though m and E can be manipulated by an algebraic syntax. The Book of Nature, though it employs a mathematical grammar, is written in the *language* of physics (or chemistry or biology . . .).[15]

Thus when contemporary physicists conclude that the Cartesian dream of reducing geometry to physics is about to be realized at last,[16] one has to be careful to note that the 'geometry' has first been 'physicalized'. Metaphors like that of J. A. Wheeler: material entities as 'wormholes in space', can function only if 'space' be already endowed with a number of dynamic properties. The space-time metric of general relativity is matter-dependent, in a way the Euclidean metric of Newtonian physics was not. What it is to 'geometrize' physics is quite different in the two cases. In one, the geometry was given in advance as an apparent absolute. In the other, the 'geometry' is tailored to the needs of describing a system of this sort, and concepts (like *field*) are incorporated in it that find their rationale in the first place in physics rather than in mathematics proper. The Aristotelian distinction between quantity and quality finds no place here. The formalism of general relativity would be as much qualitative, in Aristotle's sense, as quantitative provided one took into account its semantics as well as its syntax. What has happened here is not a simple reduction of physics to mathematics but rather an enlargement of mathematics, largely under an impetus from physics.

Salviati met Simplicio's challenge: "can the phenomena of the sensible world be ordered geometrically without distortion?" by pointing out what is *meant* by realizing mathematical quantities in matter, and by promising that, in practice, the 'impediments' could be allowed for. Galileo took for granted that his geometry provided the *proper* language of space and time measurement, and that arithmetic would suffice for *gravità.* There was no sharp break between the language of the new mechanics and that of the older 'mixed science' tradition. There was not even a clear transition in epistemic terms from the language of the old mechanics to that of the new.[17] The element of the *theoretical* in the choice of an appropriate language was not yet apparent. Geometry was in a sense on trial, however, as the language of mechanics. And the elegance of Galileo's two laws of motion was a powerful persuader. It was only with the appearance of Newton's *Principia,* however, that the

[15] See McMullin, 'The language of the Book of Nature', *Proc. Int. Cong. Logic, Methodology and Philosophy of Science,* Hannover, 1979, section **6**, 142 – 147.

[16] C. Misner, 'Mass a form of vacuum', *The Concept of Matter* (Notre Dame: University of Notre Dame Press, 1963), pp. 596 – 608, E. McMullin (ed.); S. Hawking, *Is the End in Sight for Theoretical Physics?,* inaugural lecture (Cambridge: Cambridge University Press, 1980).

[17] Gaukroger argues that the transition from an 'abstractive' to a 'non-abstractive' language of science occurs with Galileo (*op. cit.,* ch. 6).

interconnectedness of geometric and dynamic concepts began to be noticed. The language of Newtonian physics was a single tight unit, to be evaluated as a whole, and warranted by the predictive and explanatory success of the theory.[18] This would not become apparent, however, until two other developments occurred: the discovery of non-Euclidean geometries and the replacement, in certain contexts, at least, of the supposedly definitive language of classical mechanics by the much more complex language of relativity.

Mathematical idealization has worked well for the natural sciences. The extent to which it *is* an idealization has steadily diminished as the mathematical language itself has become progressively more adapted to the purposes of these sciences. It would be hazardous today to argue (as Bergson did in regard to *élan vital* and Teilhard de Chardin in regard to psychic energy) that there are causal factors at work in the natural world that are *inherently* incapable of being grasped in mathematico-physical terms. The weight of the inductive argument is surely in the opposite direction. But it should be underlined once again that what has made this possible is not so much the reducibility of the physical as the almost unlimited plasticity of the mathematical.

The theme of 'idealization', as it has been developed here, presupposes a world to which the scientist is attempting to fit his conceptual schemas, a world which is in some sense independent of these schemas and to which they only approximately conform. This is (it would seem) equivalent to presupposing some version or other of scientific realism. Whether this would be a weaker version acceptable to all save strict instrumentalists or the strong version Putnam and Rorty have castigated under the label of 'metaphysical realism' is not immediately evident. But this would have to be the topic of another essay.

2. Construct Idealization

Mathematical idealization is a matter of imposing a mathematical formalism on a physical situation, in the hope that the essentials of that situation (from the point of view of the science one is pursuing) will lend themselves to mathematical representation. The technique is obviously as old as mathematics itself. It became highly developed first in the 'mixed sciences', notably in astronomy. It is associated with Galileo only because the geometrization of the science of motion was one of its first great challenges and first great achievements. When Husserl speaks of 'Galilean idealization' in *The Crisis of European Sciences,* it is this form that he has in mind.

When Galileo was faced with complex real-world situations, however, he 'idealized' in more specific ways. That is, he shifted the focus to a simpler

[18] See E. McMullin, 'The significance of Newton's *Principia* for epistemology', to appear.

analogue of the original problem, one that lent itself more easily to solution. This might, in turn, then lead to a solution of the original complex problem. This technique had, of course, long been familiar in the mixed sciences, but Galileo laid his own stamp upon it. Idealization in the 'Galilean style' soon became a defining characteristic of the new science.

Galilean idealization can proceed in two very different ways, depending on whether the simplification is worked on the conceptual representation of the object, or on the problem-situation itself. The first will be called 'construct idealization',[19] the second, 'causal idealization'. Though Galileo's constructs were no more than simplified diagrams for the most part, he was aware of at least some of the problems that this sort of idealization entailed. Galileo's diagram is not a theoretical model in the sense we shall later define, since it is not itself being explored or tested. We shall look at a couple of examples from his work first, before going on to the more complex theoretical models of later science.

> Now I am not unaware that someone at this point may object that for the purpose of these proofs, I am assuming as true the proposition that weights suspended from a balance make right angles with the balance — a proposition that is false, since the weights, directed as they are to the center of the universe, are convergent. To such objectors I would answer that I cover myself with the protecting wings of the superhuman Archimedes, whose name I never mention without a feeling of awe. For he made this same assumption in his *Quadrature of the Parabola*. And he did so perhaps to show that he was so far ahead of others that he could draw true conclusions even from false assumptions.[20]

In the perspective of Aristotelian demonstrative science, such idealizations ('false assumptions') were, of course, unacceptable since conclusions were sought that would be "eternal and necessary" in character. Approximation of any sort would thus be excluded.[21] But Galileo's defence here, and elsewhere

[19] This is also sometimes called 'mathematical idealization'; the label is, however, unsuitable because the model (as we shall see) neeed not necessarily be mathematical in form.

[20] *De Motu*, (translation I.E. Drabkin), in *On Motion and On Mechanics* (Madison: University of Wisconsin Press, 1960), p. 67. See also *Two New Sciences*, p. 275.

[21] Koertge (*op. cit.*, p. 397) notes that the Aristotelian whom Galileo would have been likely to have in mind when penning these lines was Guidobaldo del Monte, Galileo's earliest patron, who had given him a copy of his paraphrase of Archimedes' *On Plane Equilibrium* (1588) shortly before. Guidobaldo objected to Archimedes' use of approximations: "Though at times the truth may accidentally follow from false assumptions: nevertheless it is the nature of things that from the false, the false generally follows, just as from true things the truth always follows" (from his *Le Mechaniche*, transl. S. Drake, In *Mechanics in Sixteenth Century Italy* (Madison: University of Wisconsin Press, 1969, p. 278), S. Drake and I. E. Drabkin (ed.). Guidobaldo's objection here was part of a broader attack of the sort discussed in the last section on the use of mathematics in a properly physical science. "These men are moreover deceived when they undertake to investigate the balance in a purely mathematical way, its theory being actually mechanical" (*loc. cit.*). In his view, mathematical idealization inevitably *falsifies* physical reasoning.

(when, for example, he takes a small segment of the earth's surface to be flat or a rolling ball to be perfectly spherical), is to argue that the departure from truth is imperceptibly small. And the idealization enables a calculation to be made that would otherwise be impossibly complicated.

But suppose the departure from truth is *not* so small? In the discussion of projectile motion in the Fourth Day of the *Two New Sciences,* Sagredo and Simplicio both raise troublesome objections to the formal idealization that Galileo employs in his proof that the projectile will follow a parabolic path.[22] Sagredo says bluntly that the path *cannot* be parabolic, since such a path, if extended, could never get the heavy body to the center of the earth, as it should. Simplicio reminds Salviati that a crucial element in his proof, uniform motion on a horizontal plane, is not possible since such a plane will begin to slope upwards as soon as it leaves the tangent point and thus the body will slow down. These are serious objections *if* the projection be supposed to occur on the surface of the earth.

But *does* Galileo suppose this? On the one hand, he had originally set the stage by "mentally conceiving" an infinite plane on which an "equable motion would be perpetual".[23] This *sounds* as though he has idealized the earth away (in which case the objections of his interlocutors can easily be set aside). But he also retains the solid plane as a condition for the body to maintain uniform motion, so that he is still thinking of the body as possessing weight, *i.e.* as having a tendency to move to the center of the earth. So the objections are valid. He has not idealized far enough; he has not enunciated the principle of inertia, though he is close.[24]

He responds to the objections by recalling the precedent of Archimedes once more. Even an artillery shot will travel only at most a thousandth of the earth's radius before hitting the earth; to idealize the earth as flat over such a small relative distance, in order to compute the 'downward' effect of weight, will not, he says, appreciably affect the parabolic shape of the path. Whereas, he concedes, the shape will be 'enormously transformed' if the path be supposed to continue towards the center.

This suggests a second way of dealing with the fact that construct idealizations "depart from the truth". If this departure is appreciably large, perhaps its effect on the associated model can be estimated and allowed for, when the explanatory implications of the model are being worked out.

> If we wish to use these conclusions proved by supposing an immense distance [from the earth's center] for finite distances, we must remove from the demonstrated truth

[22] *Two New Sciences, Opere,* **8**, 274.
[23] *Two New Sciences, Opere,* **8**, 268.
[24] See A. Koyré, 'Galileo and the law of inertia', part III of *Galileo Studies* (London: Harvester, 1978); 'The principle of inertia', in *Galileo, Man of Science, op. cit.*, 27 – 31.

whatever is significant in [the fact that] our distance from the center is not really infinite.[25]

That is, once the idealization has yielded a result, that result can (perhaps) be modified in order to make allowances for the "departures from truth" that the original idealization required. But Galileo did not really have the requisite techniques for meeting in this way either of the objections he himself had put in the mouths of the questioners in the dialogue. Simplicio's objection, in particular, brought out the ultimate ambiguity of his views on inertial motion. What would bodies on the surface of a spinning globe 'naturally' do? He had two incompatible reponses to this. The one on which his defence of Copernicanism rested relied on the assumption that the departure from the rectilinear of a curved path is negligibly small. But *conceptually* this departure makes all the difference. Galileo did not know how to allow for it, and so in the end he had to leave this objection unanswered. Here then is a construct idealization that did not work as it should.

In contemporary science, models are no longer just simplified geometrical diagrams, and so construct idealization has become far more complex. A word first about the term 'model', which conveys very different meanings to different people, to the physicist and the logician, for example.[26] Every physical theory involves a model of the physical object(s) whose behavior the theory is expected to explain. The theory is not identical with the model, it is the 'text' in terms of which the model is specified, instructions are given on problem-solution, and so forth. The model itself is a postulated structure of elements, relations, properties. Inferences ('theoretical laws') can be derived which describe the behavior of the model under specified constraints of context or parameter value. To the extent that these inferences simulate the empirical regularities whose explanation is sought, the theory and its associated models are said to 'explain' these regularities. Note the distinction here between the 'theoretical laws' whose warrant is the explanatory theory, and the 'empirical laws' or observed regularities, whose immediate warrant is observation or experiment. The status of each as 'law' is quite different.[27]

[25] *Two New Sciences*, p. 224; *Opere*, 7, 275.

[26] The logician's 'model' is an entity of known properties which satisfies a particular formal system and which can thus serve, for example, as a warrant for the consistency of the system. The physicist's model, on the other hand, is a tentative representation intended to explain some aspect of a real-world situation. For the logician, the *model* is the 'anchor'; for the physicist, it is the observed world. Thus, though there are affinities between the two sorts of models, since in each case there is a structure which 'satisfies' a formal system, the model plays opposite roles in the two contexts.

[27] See E. McMullin, 'Two ideals of explanation in natural science', *Midwest Stud. Phil.*, H. Wettstein (ed.) (Minneapolis: University of Minnesota Press, 1984), pp. 195 – 210.

Much more would have to be said about the use of models in theoretical science in order to clarify the notion adequately.[28] Our interest here lies in the use of models as idealizations of complex real-world situations, the 'falsity' this may introduce into the analysis, and the ways in which this 'falsity' may be allowed for and even taken advantage of. Every theoretical model idealizes, simplifies to some extent, the actual structure of the explanandum object(s). It leaves out of account features deemed not to be relevant to the explanatory task at hand. Complicated features of the real object(s) are deliberately simplified in order to make theoretical laws easier to infer, in order to get the process of explanation under way.

Idealization enters into the construction of these models in two significantly different ways. Features that are known (or suspected) to be relevant to the kind of explanation being offered may be simplified or omitted in order to obtain a result. Newton knew that the sun should have a small motion in consequence of the earth's attraction upon it. But in his derivation of Kepler's laws in the *Principia,* he assumed the sun to be at rest, which in terms of his theory was to assume it infinitely massive. This made theoretical laws much easier to derive, though now, of course, only approximate consequences. This kind of idealization we shall call *formal.*

Formal idealization need not be mathematically expressed, though in the physical sciences it ordinarily is. The reason has more to do with the nature of physics than it has with that of formal idealization as such. The models used by psychologists (the stimulus/response model, for example) frequently display a formal idealization of a nonmathematical kind. It could be argued that mathematical idealization is necessarily formal in character. But I have chosen to treat the two separately, because the issues that mathematical idealization raises are prior both historically and logically. Still, it should be kept in mind that the two are not strictly distinct.

On the other hand, the model may leave features unspecified that are deemed irrelevant to the inquiry at hand. The kinetic theory of gases postulated molecules as the constituents of which gases in the aggregate are composed. But it did not specify any internal structure for these molecules. It did not exclude their having such a structure. But the question: what is *inside* the molecule? went unasked because it was not relevant to the purposes of that particular theory. More generally, the elements of a theoretical model possess only the attributes expressly assigned to them. Questions about other properties that entities of this physical type might plausibly be supposed to

[28] For a start, see M. Spector, 'Models and theories', *Br. J. Phil. Sci.* **16** (1965/6), 121 – 142; P. Achinstein, 'Theoretical models', same, 102 – 120; M. Redhead, 'Models in physics', same, 31, 1980, 145 – 164; E. McMullin, 'What do physical models tell us?', *Logic, Methodology and Philosophy of Science,* B. van Rootselaar (ed.) (Amsterdam: North-Holland), vol. 3, pp. 389 – 396.

possess cannot be answered, unless of course the model is extended in response to different theoretical needs. This, for reasons we will return to, can be called *material* idealization.

Formal and material idealization are two different aspects of a single technique utilized by scientists: construct idealization. They are worth distinguishing because the 'adding back' that follows the initial idealization and shapes the further progress of inquiry can take two quite different directions, corresponding to two different aspects of the original model. I shall draw on examples from recent physics in order to make this distinction clear, rather than relying on illustrations from Galileo's own work. But it must be emphasized that the elements of construct idealization are already implicit in the tentative theoretical hypotheses he formulated to explain such phenomena as the tides, the 'force of the vacuum', comets, the cohesion of solid bodies.

3. Formal Idealization

Theoretical models ordinarily begin with the most simplified structure that still retains the 'essence' of the original problem situation. Thus, for example, the 'ideal gas law' is derived by assuming that the molecular constituents of a gas are perfectly elastic spheres whose volume is negligible compared to the volume occupied by the gas and whose mutual attractions can also be neglected. This law is identical with the experimental Boyle – Charles' law, which indicates that these idealizations are good approximations, for the normal ranges of temperature and pressure within which the Boyle – Charles law is known to hold fairly well. But then if theoretical corrections are made by allowing for the space occupied by the molecules and for the small attractions between them, a new theoretical law is obtained in which both the volume and pressure factors have to be slightly amended: $(V-b)(P+a/V^2) = RT$ (the Van der Waals' equation). At low pressures, this virtually reduces to the ideal gas law. But for high pressures (and resultant small volumes), it will have a very different form. Experiments on gases at high-pressure support the overall validity of the Van der Waals' corrections. (At low temperatures, the equation fails as the gas approaches liquefaction; no satisfactory corrections, or alternative models, have as yet been found for gases close to their 'critical temperatures'.)

To illustrate in more detail this dual process of idealizing followed by 'de-idealizing' or adding back, let us recall some of the key stages in the development of one of the most successful theoretical models in the entire history of physics, Bohr's model of the hydrogen atom, originally put forward in 1913 to account for the patterns that spectroscopists had been finding in the frequencies of the light emitted by hydrogen when heated.[29]

[29] This example is worked out in detail in 'What do physical models tell us?', *op. cit.*, note 28.

Bohr's initial model was the simplest-possible stable arrangement of proton and electron: the electron is in a circular orbit around the proton, and the much more massive proton is assumed to be at rest. Only certain orbits are permitted (the quantum postulate), and the characteristic frequencies of the light emitted are due to the transition of the electron from a higher to a lower energy-level. Even this very simple model was sufficient to yield some startlingly good results for the basic *H*-spectrum. In particular, it permitted the calculation of the Rydberg constant which occurs in the formula for the spectrum, and which was already known experimentally to a very high degree of accuracy. What Bohr showed was that his model allowed one to derive this constant from the known values of the electron mass and charge, the velocity of light, and the Planck constant. The fit between the theoretical and the measured values was good to three figures, a striking validation of the model.

But the original model was deliberately idealized in at least three respects. Simplifying assumptions were made that were *known* to likely be relevant to the predictive capacities of the model; thus from the beginning, it was realized that these assumptions would ultimately have to be investigated and 'allowed for'. The first assumption was that the nucleus remains at rest, equivalently therefore taking it to be of infinite mass. Allowing for the small motion of the nucleus around the common center of gravity of proton and electron yields a correction factor of $(1 + m/M)$ in the spectral formula, where m/M is the ratio of the electron and proton masses. This correction immediately accounted for the slight separation (discovered experimentally by Pickering in 1897) between the lines of hydrogen and of ionized helium. The latter resembles hydrogen in that a single electron orbits a nucleus: the difference is that the nucleus is four times heavier, thus giving a different m/M factor.

A second assumption was that the electron orbit is circular. But elliptical orbits are the norm for bodies moving under a central force, the circle being a special ('degenerate') case. This assumption does not affect the basic equation (since a circle is equivalent to a 'family' of ellipses), except where the hydrogen is subjected to an intense electrical field. The effect of such a field (the 'Stark effect') was already known experimentally. Schwartzchild was able to show (1916) that in such a field the energy levels associated with different ellipses will 'split', and thus that the emitted frequencies, which depend on differences of energy-level, will show a characteristic fine-structure as well as certain polarization effects. Once again, theory and experiment were shown to match precisely. The original formal idealization had been triumphantly 'allowed for'.

One further omission in the original model was of the relativistic effects due to the rapid motion of the electron. Later on, when these were computed, it was found that two corrections had to be made to the series formula, one of which shifts the entire series by a very small amount and the other which gives a fine structure to *all* the lines, if the resolving power of the instrument is high enough. Michelson had already noticed this fine-structure in the first Balmer line in 1887; extensive experimental work on the spectrum as a whole once more showed that the theoretical consequences of allowing for a factor originally omitted because of formal idealization corresponded, to an impressive degree, with the empirical results.

It would be easy to find other examples of the same kind in the recent history of theoretical science. This one may, however, be sufficient to illustrate the way in which models can be made more specific by eliminating simplifying assumptions and 'de-idealizing', as it were. The model then serves as the basis for a continuing research program. This technique will work only if the original model idealizes the real structure of the object. To the extent that it does, one would *expect* the technique to work. If simplifications have been made in the course of formulating the original model, once the operations of this model have been explored and tested against experimental data, the model can be improved by gradually adding back the complexities. Of course, this requires a knowledge of how that particular 'complexity' operates. For example, if the nucleus has been assumed to be unaffected by the motion of the orbiting electron, an assumption similar to that made by Newton in the *Principia* when deriving Kepler's laws of planetary motion, one will need to know how to allow for the proton motion around the common center of gravity in recomputing the energy levels inferred from the original model.

These levels could, of course, also be modified in an *empirical* way by simply correcting the theoretical laws in the light of empirical evidence. But this correction would itself then go unexplained. It would, equivalently, constitute a *defect* in the original model. Scientists regard such corrections as *ad hoc*, even though they may allow correct predictions to be made. The renormalization techniques required in quantum field theory because of the assumption introduced by the original idealization of the electron as a point-particle would be a case in point. When techniques for which no theoretical justification can be given have to be utilized to correct a formal idealization, this is taken to count against the explanatory propriety of that idealization. The model itself in such a case is suspect, no matter how good the predictive results it may produce. Scientists will work either to derive the corrections theoretically, or else to replace the model with a more coherent one.

Formal idealization is thus a quite powerful epistemic technique, not only because the original model already supports theoretical laws that account approximately for some of the regularities to be explained, but even more because the 'adding back', if it accounts for additional experimental data and especially if it leads to the discovery of new empirical laws, is a strong validation for the model and its accompanying theory, within the limits of the idealizations employed. Indeed, this becomes a strong (though not conclusive) argument for the existence of the structures postulated by the model.[30] If the original model merely 'saved the appearances' without in any way approximating to the structure of the object whose behavior is under scrutiny, there would be no reason why this reversal of a simplifying assumption, motivated by the belief that the object *does* possess something like the structure attributed to it, would work as it does. Taking the model seriously as an approximately true account is what leads us to *expect* the correction to produce a verifiable prediction. The fact that formal idealization rather consistently *does* work in this way is a strong argument for a moderate version of scientific realism.[31]

4. Material Idealization

The other aspect of construct idealization affects inquiry in a quite different way, but it too, far from irretrievably falsifying the theoretical account, ultimately serves to elicit a powerful indirect warrant for the (approximate) truth of the theory. Models are necessarily incomplete; they do not explicitly specify more than they have to for the immediate purposes at hand. Gene theories do not purport to tell all there is to be known about genes. Electron theories leave open the possibility that there is more to be known about electrons. (Genes and electrons are taken here to be the real referents of the corresponding theoretical terms. Dirac's electron theory tells us all there is to know about the *theoretical* Dirac electron, of course.)

The oxymoron, 'material idealization', is suggested here by an analogy with the notion of "material cause" in the Aristotelian tradition. Unlike the other three types of 'cause' (the term, 'explanation', would be preferred today), Aristotle's material cause did not function actively in the explanation itself. In Aristotle's example of the sculptor and the statue, one 'explains' the statue in terms of its form, of the agent who brings it about, and of the goals of its

[30] There are analogies here to Sellars' argument for realism in 'The language of theories', *Current Issues in the Philosophy of Science*, H. Feigl and G. Maxwell (eds.) (New York: Holt, Rinehart, 1961), pp. 57 – 77. 'Theories not only explain why observable things obey certain laws, they also explain why in certain respects their behavior does not obey a law' (p. 72). Thus, for example, the theory of isotopes explains why the weights of chemical elements in nature do not obey the integer laws of Mendeleef. (This example works rather better to illustrate Sellars' point than the example he actually gives.) It is this ability of microtheories to 'explain' random behaviour (Sellars claims) that warrants our confidence in them as "knowledge of what really exists" (p. 72). Sellars' argument bears on laws, on the nomothetic element in science, rather than on models, the retroductive element stressed here. But both arguments rely on the epistemic warrant given by the *corrective* ability of theory.

[31] As developed, for example, in E. McMullin, 'A case for scientific realism', *Scientific Realism*, J. Leplin (ed.) (Berkeley: University of California Press, 1984), pp. 8 – 40.

making. But one can also 'explain' (though in a different, weaker, sense) by alluding to the sort of materials (bronze, wood) the artists had to work with. The explanation here does not require any special knowledge of what bronze itself is for this would be to invoke formal cause once more. Rather, it poses these materials as the 'given', that whose nature constrains the outcome. To understand the statue-making, it is not necessary, however, to inquire in depth into the nature of bronze or wood.[32]

Theoretical models are constructs, not materials in the Aristotelian sense. Nevertheless as we call on them to explain, we constantly encounter their 'given' aspect, that which they leave unspecified, though open for question in a different context. They are the 'materials' the scientist has to work with, the blanks that need not for the moment be filled in further.

To speak of material idealization in the context of Rutherford's nuclear model of the atom, then, is to indicate first, that the 'blank' nucleus is idealized in the sense of being left unspecified, and second, that this blank serves as the 'material' out of which an explanation of the scattering experiments can be constructed. Where the motion of formal idealization signifies that aspects of the *real* object are being left aside in the interests of furthering a better understanding, the notion of material idealization is intended to bring out the fact that further specifications of the *construct* are, for the moment, being laid aside for the same reason.

The nucleus can later, in the context of a different set of empirical explananda, be specified in terms of protons and neutrons. Nuclear models can then be formulated, just as atomic models earlier filled in the blank of the atom of kinetic theory as a means of explaining spectroscopic data. The original model provides a sort of conceptual boundary within which the later theoretical developments can be situated. The success of these developments indirectly validates the original model, blank (idealized) though it was with respect to these further specifications.

More interesting are the cases in which the model is not just a blank outline waiting to be filled in but rather serves to suggest certain modes of further development. It is not a matter of straight inference (as it was in the earlier cases when allowance was being made for factors deliberately omitted from, or simplified in, the original model). Electron spin was not part of the original Bohr model. Its omission was not a formal idealization, strictly speaking, because there was no reason to suppose the electron would possess spin. "Has the electron spin?" was a question that simply had not been asked, and that could not be answered within the original model. But it was a question that at

[32] See McMullin, 'Material causality', *Historical and Philosophical Dimensions of Logic, Methodology, and Philosophy of Science,* R. Butts, and J. Hintikka (eds.) (Dordrecht: Reidel, 1977), pp. 209–241.

some point could easily enough suggest a fruitful line to follow. When hydrogen is subjected to a strong magnetic field, the spectral lines tend to split, an effect discovered in 1897 by Zeeman. For more than ten years attempts were made to explain this effect by means of the Bohr model. Finally, in 1926 the metaphor of electron spin suggested a way out of the difficulty. The model operated here not as a starting-point for strict inference but as a source of suggestion.

This kind of metaphoric extension of features left originally unspecified or only partially specified seems to operate within almost every successful theoretical research program. It would be easy to cite scores of examples of this sort from molecular biology, from astrophysics, from organic chemistry . . . . What makes it heuristically sensible to proceed in this way is the belief that the original model *does* give a relatively good fit to the real structure of the explanandum object. Without such a fit, there would be no reason for the model to exhibit this sort of fertility. This gives perhaps the strongest grounds for the thesis of scientific realism.

The implications of construct idealization, both formal and material, are thus truth-bearing in a very strong sense. Theoretical laws derived from the model give an approximate fit with empirical laws reporting on observation. It is precisely this lack of perfect fit that sets in motion the processes of self-correction and imaginative extension described above. If the model is a good one, these processes are not *ad hoc;* they are suggested by the model itself. Where the processes *are* of an *ad hoc* sort, the implication is that the model is not a good one; the uncorrected laws derived from it could then be described as 'false' or defective, even if they do give an approximate fit with empirical laws. The reason is that the model from which they derive lacks the means for self-correction which is the best testimony of truth. This is the type of defect on which Cartwright mainly rests her case in *How the Laws of Physics Lie.* That there are such laws in physics, particularly in quantum mechanics, may well be true. But that this sort of defect characterizes *all* the theoretical laws of physics or is even typical of the fundamental laws of mechanics, the part of physics she tends to focus on, would seem to be contradicted by the instances of construct idealization of the 'truth-testifying' sort that are commonplace in the history of physics.

5. Causal Idealization

The really troublesome impediments, Galileo said more than once, are the causal ones. The unordered world of Nature is a tangle of causal lines; there is no hope of a "firm science" unless one can somehow simplify the tangle by eliminating, or otherwise neutralizing, the causal lines which impede, or complicate, the action of the factors one is trying to sort out. Here is where

Galileo diverged most sharply from Aristotle, whose notion of nature made this sort of simplification suspect because of the order imposed artificially (as it might seem) on the normal course of events. And it is this sort of idealization that is most distinctively 'Galilean' in origin. His insight was that complex causal situations can only be understood by first taking the causal lines separately and then combining them.

Experiment involves the setting up of an environment designed to answer a particular question about physical processes. The experimenter determines how Nature is to be observed, what factors will be varied, which will be held constant, which will be eliminated and so on. This sort of manipulation is not always possible; experimental method cannot be directly applied in palaeontology or in astrophysics, for instance. The move from the complexity of Nature to the specially contrived order of the experiment is a form of idealization. The diversity of causes found in Nature is reduced and made manageable. The influence of impediments, *i.e.* causal factors which affect the process under study in ways not at present of interest, is eliminated or lessened sufficiently that it may be ignored. Or the effect of the impediment is calculated by a specially designed experiment and then allowed for in order to determine what the 'pure case' would look like.

Galileo did not, of course, invent the technique of experiment. One can find quite sophisticated forms of experiment in medieval optics, for instance. Yet it is striking how rarely recourse was had to experiment in later medieval mechanics, when to our eyes it would seem the obvious means of testing the claims being made about natural and imposed motions, for example. In his earliest writings on mechanics, Galileo was still as conceptualist in approach as the Aristotelians had traditionally been. But he turned to experiment more and more during his years in Padua, at the time that the groundwork for the *De motu locali* was being laid. He was not, of course, an experimenter in the modern sense. He rarely reports his actual results, and in some cases (*e.g.* when he speaks of dropping balls of different materials from a tall building) the 'results' he claims clearly were not obtained by him experimentally but are projections of what he *expected* to find. Nevertheless, he showed the typical ingenuity of the successful experimentalist in devising the apparatus and the instrumentation needed to answer the questions he was putting about the most general categories of falling motion. Most important, the *Two New Sciences* demonstrated that a science could be both mathematical *and* empirical, provided that the questions are put to Nature in a properly experimental way.

But can the laws arrived at in this contrived way be said to be true of Nature? And if so, in what sense? After Salviati has explained the properties of uniformly accelerated motion, using neat geometrical diagrams, Simplicio remains unconvinced as to whether he has demonstrated anything that bears on the real world: "I am still doubtful whether this is the acceleration produced by nature". So he requests an 'experience' to support the claim that the falling motion of real bodies *is* governed by this law. Salviati applauds his caution; his "reasonable demand" is one which is:

> usual and necessary in those sciences which apply mathematical demonstrations to physical conclusions, as may be seen among astronomers, writers on optics, mechanics, music, and others who confirm their principles with sensory experiences that are the foundations of all the resulting structure.[33]

This is the Galileo beloved of empiricists![34] And so Salviati goes on to describe the inclined-plane experiment, where the spaces traversed are found to be as the squares of the time taken. This means, equivalently, that the motions are uniformly accelerated, and "with such precision that these operations repeated time and again never differed by any notable amount".[35] To bring this result about, the influence of the two main "impeding" causes, friction and air resistance, had to be diminished as far as possible. And so he uses "a very hard bronze ball, well-rounded and polished", rolling on "vellum as much smoothed and cleaned as possible". Whether these precautions would have sufficed to enable him to obtain the idealized law he claimed to find exemplified in his results—the results themselves are not given—has been a topic of frequent debate among historians.[36] But Simplicio obligingly avers himself content to accept the claim "as most certain and true" on the basis of his trust in Salviati's diligence as an experimenter.

Galileo is convinced that he has discovered the motion that "nature employs for descending heavy things".[37] This is a new understanding of "natural motion". It is not, as it was for Aristotle, motion as it occurs in the normal course of affairs. It is "natural" in the sense that it defines what the body *would* do on its own, apart from the effects of causes (like the resistance of air) external to it. These latter are to be treated as "impediments", as barriers to an understanding of what the "natural" tendency of body is. Aristotle had argued that a vacuum is impossible in Nature and hence that air-resistance is necessarily a retarding factor in all motion. Galileo rejected this claim. But it was not because of the difference between the two men regarding the

[33] *Two New Sciences, Opere,* **8**, 212.
[34] E. McMullin, 'The conception of science in Galileo's work', *New Perspectives on Galileo,* R. Butts and J. Pitt (eds.) (Dordrecht: Reidel, 1978), pp. 229–240.
[35] *Op. cit.,* p. 213.
[36] See A. Koyré, 'An experiment in measurement', *Proc. American Philos. Society,* 97 (1953), 223–257; T. B. Settle, 'Galileo's use of experiment as a tool of investigation', *Galileo, Man of Science,* pp. 315–337; R. H. Naylor, 'Galileo and the problem of free fall', *Br. J. Hist. Sci.* 7 (1974), 105–134.
[37] *Op. cit.,* p. 197.

possibility of a vacuum that their approaches to motion differed so fundamentally. The logic of Galileo's argument led him to the claim that all bodies in the void fall at the same speed. That it was counterfactual, since he could not in practice realize a vacuum experimentally, did not bother him. What he wanted to discover was the "downward tendency which (body) has from its own heaviness",[38] in the absence of all other causes.

The passage, though well-known, is worth quoting in full:

> We are trying to investigate what would happen to moveables very diverse in weight, in a medium quite devoid of resistance, so that the whole difference of speed existing between these moveables would have to be referred to inequality of weight alone. Hence just one space entirely void of air—and of every other body, however thin and yielding—would be suitable for showing us sensibly that which we seek. Since we lack such a space, let us (instead) observe what happens in the thinnest and least resistant media, comparing this with what happens in others less thin and more resistant. If we find in fact that moveables of different weight differ less and less in speed as they are situated in more and more yielding media, and that finally, despite extreme difference of weight, their diversity of speed in the most tenuous medium of all (though not void) is found to be very small and almost unobservable, then it seems to me that we may believe, by a highly probable guess, that in the void all speeds would be entirely equal.[39]

This asymptotic approach to the "pure case" where only one factor is at work is still an *experimental* idealization. It may, of course, be doubted whether Galileo ever carried through the series of experiments with media of gradually decreasing densities and with bodies of different weight that he describes here. But the principle of it was clear. A single cause could be isolated, by a combination of experimental and conceptual techniques. The (still counterfactual) claim that all bodies fall at the same speed *in vacuo* was warranted by the way the behavior of bodies of different weights converged on a single simple uniformity as the limiting case of zero density is approached. It was also, no doubt, reinforced in his mind by the more general conviction that "in the custom and proceduce of Nature herself . . . she habitually employs the first simplest and easiest means".[40]

We have become accustomed in recent decades to the charge that this sort of experimental idealization can, in the context of the behavioral sciences, distort the behavior one is trying to understand. Ethologists have urged that the

[38] *Op. cit.*, p. 268.
[39] *Op. cit.*, p. 117.
[40] *Op. cit.*, p. 197.

"Galilean" methods[41] advocated by behaviorists and others in biology and psychology inevitably alter the behavior to be studied in ways that undermine the value of these methods as guides to genuine scientific truth. The technique of isolating causal lines does not seem to work so well when the object under study is a complex organism or a social group. The "impediments" cannot be defined and eliminated as they can in the case of a ball on an inclined plane.

But our concern here is with the "new science" of mechanics. The technique of causal idealization enabled Galileo to formulate simple laws of terrestrial motion. (The technique was unnecessary in the case of celestial motion where there are no impediments to remove and where, in consequence, law-likeness had been easier to recognize from an early time.) Galileo's laws state what "would happen if . . .". That is, they are governed by implicit modifiers which remind us that for them to hold, "other things must be held the same".

6. Subjunctive Idealization

This suggests another way in which Galileo (and his predecessors) may be said to have idealized. Suppose the causal simplification be *conceptual* instead of experimental; one focusses on *this* cause (in thought) to the exclusion of others. This resembles formal idealization in its conceptual character. And it resembles causal idealization in that it separates off single causes for special scrutiny. It postulates an answer to the question "what would happen if . . ."; its subjunctive character makes it worth some special note.

Subjunctive assertions occur, of course in a variety of contexts in science. When Aristotle specified the natural motion of a body, this was to say how the body would move if placed in a certain context and if not impeded. To specify the nature of something or to put forward a scientific law is not to describe something that would happen in all possible circumstances. It is to say that under "normal" conditions, or if other causally relevant variables are held constant, this is what *would* happen. Scientific laws are never asserted categorically, though they may appear to be. When Bohr's theory of the H-atom predicts the spectrum of hydrogen, it does so (as we have seen) under the unspoken proviso that the hydrogen is not being subjected to electrical and magnetic fields or other "impediments". One function of an experiment is to

[41] In an influential article appearing in 1931, Kurt Lewin argued that psychology had to adopt the Galilean techniques which had so benefited mechanics over the past three centuries. It was its residual Aristotelianism that had (in his view) prevented it from progressing as it should. See 'The conflict between Aristotelian and Galilean modes of thinking in contemporary psychology', *J. Gen. Psych.* 5 (1931), 141 – 177. Reprinted in *A Dynamic Theory of Personality* (New York: McGraw-Hill, 1935), pp. 1 – 42.

discover what these additional causally relevant factors might be and how they relate to those already studied.

The technique of thought-experiment invokes a 'what-if' in two rather different ways. Both of these can be illustrated in the early history of mechanics. Critics of Aristotle, like Philoponus, would ask, for instance, what would happen if two bodies of the same weight were tied together and then allowed to fall. Would this double the speed of the compound body, as Aristotle's law of fall would imply? The intuitive absurdity of such a claim was taken to refute Aristotle's assertion. In this case, the experiment *could* easily have been done, but it was taken to be unnecessary because our intuitions in regard to motion already gave the answer. On the other hand, there were thought-experiments which would *have* to remain in thought only. Buridan and others of the Paris school, for example, invoked spheres spinning in frictionless media, millwheels on frictionless axles and the like, in order to clarify mechanical principles. The technique suggested itself more readily in mechanics than elsewhere because our everyday experience with moving bodies seemed a reliable guide in regard to at least some of the most general features of motion.

Both types of thought-experiment were put to extensive and ingenious use by Galileo. We have already seen him set balls rolling on infinite frictionless flat planes or perfectly spherical surfaces.[42] Even more characteristic were the cases where he deliberately chooses *not* to call on experiment when he easily could. When discussing the imperceptibility of shared motion in the Second Day of the *Two New Sciences,* he states confidently that a stone dropped from the mast of a moving ship will fall directly under the mast. In response to Simplicio's scandalized "So you have not made a hundred tests or even one? And yet you so freely declare it to be certain"[43], Salviati answers:

> Without experiment, I am sure that the effect will happen as I tell you because it must happen that way. And I might add that you yourself already know that it cannot happen otherwise, no matter how you may pretend not to know it.

Galileo does not want to call on specific experiments here because so much more evident and more general a warrant is available. The Aristotelian position is inconsistent with the most general facts about motion which

[42] See, for example, *op. cit.*, p. 148; *Opere*, 7, 174.
[43] *Op. cit.*, p. 145; *Opere*, 7, 171.

Aristotelians themselves on reflection will have to acknowledge.[44] The technique is the same as that already utilized by Philoponus, Buridan, and the earlier critics of Aristotle. These subjunctive assertions, whether counterfactual or not, rest upon an appeal to simple experience, to an intuition tutored by the most general sorts of observation of motion. No actual experiment is invoked. In that sense, they may be said to "idealize", to prescind from the details of concrete experimentation.

But subjunctive idealization plays a much more central role than this in Galileo's mechanics. The crucial move he makes at the beginning of the Fourth Day of the *Two New Sciences* is to separate off two different component motions in the single motion of the projectile. One of these is a uniform horizontal motion, assumed to be on a plane, which "would be perpetual if the plane were of infinite extent" and the other of which is due to "that downward tendency which it has from its own heaviness". From these two conceptually idealized separate motions, "there emerges a certain motion, compounded from equable horizontal and from naturally accelerated downward (components)"[45]. Showing this motion to be parabolic in form was the great triumph of his mechanics.

There are some notable ambiguities in the proof he gives but we shall leave these aside. It is the technique of focussing on the effects of single causes in cases where multiple causes are operating that interests us. Looking at projectile motion as a product of two motions, each of them definable in a subjunctive way, was what made the analysis possible. The technique was not new, of course; one finds hints of it quite early in the Aristotelian tradition in mechanics. But the Aristotelians were inhibited from developing it because of their views on natural motion, while Galileo made it central to his entire mechanics.

[44] Feyerabend argues that Galileo does not really show the Aristotelian framework to be inconsistent (*Against Method* (London: New Left Bookstore, 1975), ch. 7). Rather, we start with "two conceptual sub-systems of ordinary thought" (p. 85), one of which takes motion to be an absolute process capable of being perceived directly, the other which takes it to be relative so that our senses do not register shared motion. The Aristotelian (according to Feyerabend) "has developed the art of using different notions on different occasions without running into a contradiction" (p. 85), even though these notions are in fact inconsistent with one another. For Galileo to "confound the conditions of the two cases" is to use "trickery" (p. 84) or "propagandistic machinations" (p. 89). I would argue, on the contrary, that what Galileo shows is that if the relativity principle regarding the inoperative character of shared motion be adopted, no inconsistency occurs between the two cases, whereas the Aristotelian has no way of justifying his use of inconsistent paradigms on different occasions. This gives a *prima facie* case for preferring the relativity principle when describing an experience of motion. Feyerabend argues that Galileo offers no independent argument for its validity (p. 91). But surely he does, with the example of the person in the hold of a moving ship, who is unable to detect the effect of the ship's motion on the motions of objects in the hold.

[45] *Opere*, **8**, 268.

7. Cartwright on Causal Idealization

Cartwright's attitude towards causal idealization, whether experimental or subjunctive in form, is ambivalent. Her basic belief is "that even simple isolated processes do not in general behave in the uniform manner dictated by fundamental laws".[46] Scientists are misled by criteria of simplicity that lead them to take for granted that Nature "employs the simplest means". Her own view of the world is very different:

> I imagine that natural objects are much like people in societies. Their behavior is constrained by some specific laws and by a handful of general principles, but it is not determined in detail, even statistically. What happens on most occasions is dictated by no law at all.[47]

Her challenge is thus to idealization, in general, to our manner of imposing order on the "wild profusion" of Nature by glossing over differences:

> The metaphysical belief that underlies these essays is an Aristotelean belief in the richness and variety of the concrete and particular. Things are made to look the same only when we fail to examine them too closely.[48]

Such a distrust of the universal, it might be remarked, is much more nominalist than Aristotelian in inspiration. But she has no objection to the expression of law, it would seem, so long as it is arrived at experimentally. The test of existence in physics is neither naked-eye observation on the one hand nor theory on the other. It is *experiment*: "Experiments are made to isolate true causes and to eliminate false starts. That is what is right about Mill's 'methods'."[49] But if causal idealization of this sort be permitted, why should it not result in simple laws, on occasion at least? Granted, these laws might, like Galileo's law of fall, prove to be only an approximation. But Galileo's law was a *good* approximation because it was based on sound asymptotic method. Presumably it is, in Cartwright's terms, a "phenomenological" (or descriptive) law, the kind of law she is willing to accept.[50]

But what of the case where the causal idealization enables us to formulate a properly causal law, one which not only describes but explains? Such a law is

[46] *Op. cit.*, p. 58.
[47] *Op. cit.*, p. 49.
[48] *Op. cit.*, p. 19.
[49] *Op. cit.*, p. 7.
[50] It should be noted that laws arrived at on the basis of causal idealization need not themselves be causal laws. They might be merely correlations (like Boyle's Law) or kinematic laws (like Galileo's law of fall). The label 'causal idealization' refers to the technique of removal of 'impediments' rather than to the status of the law arrived at by the use of the technique.

in Cartwright's terms, 'theoretical',[51] and it is these she regards as 'false'. Here I find her argument murky. What she appears to be saying is that the warrant for a causally idealized law must in the long run lie in its ability to apply to a causally *complex* situation. She is distrustful of the "composition of causes" technique on which all such applications depend, and she is convinced, besides, that in many cases the simple theoretical laws cannot be extended to complex 'real-life' situations without *ad hoc* modifications becoming necessary.

Two things have to be said here. The warrant for Galileo's law of fall was in the first instance an 'asymptotic' experimental one, as we have seen. The same would be true of a great many other laws in fields like chemistry or genetics. How about dynamics? Cartwright finds Newton's law of gravitation and Coulomb's law problematic. They are false as they stand, she insists.[52] But could they not be arrived at in an experimental way that from her point of view is acceptable? I suspect that she would in the end have to distinguish between two forms of these laws, one of which is descriptive ('phenomenological' in her terms), and the other explanatory. The first would simply specify how bodies move when placed in a certain context; the other would *explain* these movements in terms of 'forces'. She would (I think) have to say that the explanatory ('theoretical') form of the law could not be directly warranted in terms of the broader theory of which it forms a part.[53]

But why should it *not* be so warranted? This brings us to the second point. A broader theoretical justification requires the technique of composition of causes, of adding back either the unknown 'impediments' or the other known causes that have been initially omitted in order to grasp the causally idealized system. It would require the ability to *use* the simple law, in combination with other similar laws, to understand such complex situations. And it would then be the success of the recombined set of laws in explaining causally *complex* situations that would be the primary warrant for the correctness of the laws, taken singly. But it is just this sort of warrant that Cartwright thinks to be lacking to the fundamental laws of mechanics.

The issue hinges around the technique long known as "composition of causes", a technique which both Galileo and Newton utilized but which gave both of them trouble. The topic is a complex one, and will be left to a

[51] I am not sure that I quite understand her use of the term 'theoretical'. She claims to take it over from the physicist to designate laws which are "fundamental and explanatory" (p. 2). But the term 'explanatory' itself is at least as problematic as the term 'theoretical' (as her own book makes clear) so that this attempt at explication fails.

[52] *Op. cit.*, p. 57.

[53] As we shall see in the later article, 'Composition of causes: Galileo and Newton', there is some question as to whether the technique must not ultimately fail in dealing with the strongly interactive systems of nuclear physics.

succeeding article. If composition of causes *were* to be suspect, then causal idealization would automatically be suspect also, as a guide to causally complex situations at least. But I do not think Cartwright succeeds in showing the truth of the antecedent here. I would conclude that the manifest successes of the natural sciences since Galileo's day furnishes adequate testimony to the worth of the technique of causal idealization which has been so central to those successes, as well as to the inadequacy of a metaphysics which would claim that things can only be made to look the same if "we fail to examine them too closely".

6. Conclusion

In this essay, I have briefly reviewed some of the techniques that are loosely grouped together under the label 'Galilean idealization'. There are almost certainly others that I have omitted. My choice here has been in part prompted by Cartwright's book, *How the Laws of Physics Lie,* which can, I think, be fairly construed as an attack on—or at least a series of sceptical challenges to—idealization in the 'Galilean' tradition of science.

We have seen that idealization in this context takes on two main forms. In construct idealization, the models on which theoretical understanding is built are deliberately fashioned so as to leave aside part of the complexity of the concrete order. In causal idealization the physical world itself is consciously simplified; an artificial ('experimental') context is constructed within which questions about law-like correlations between physical variables can be unambiguously answered. Causal idealization, instead of being carried out experimentally, can also be performed in thought, when we focus on the single causal line in abstraction from others and ask 'what would happen if'. This kind of idealization was central to the new science of mechanics fashioned by Galileo. We have called it 'subjunctive' in order to mark it off from the manipulative laboratory technique which Galileo also shaped and which came to define the motion of 'experiment'.

Construct idealization in the physical sciences is carried on in the broader context of a mathematical idealization which would assume that the syntax of the "language of the Book of Nature", in Galileo's phrase, is mathematical. What constitutes this as idealization is not that there are aspects of the physical world which are incapable of being expressed in a mathematical syntax but rather that mathematics can provide only the syntax and not the semantics of the language.