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OPERATIONALISM*

1. THE OPERATIONALIST VIEWS OF MACH AND BRIDGMAN

We will begin our discussion of operationalism by an account of Mach's views, and, in particular, of his famous 'operationalist' definition of mass. This is given in the section of Mach's Science of Mechanics [8] which deals with Newton. Mach, while professing boundless admiration for Newton, does not feel that the master's exposition of the foundations of mechanics is entirely satisfactory. He begins his criticism by quoting [8, p. 298] Newton's definition of mass:

> Definition I. The quantity of any matter is the measure of it by its density and volume conjointly... This quantity is what I shall understand by the term *mass* or *body* in the discussions to follow.

Mach's comments [8, p. 300]:

> Definition I is a... pseudo-definition. The concept of mass is not made clearer by describing mass as the product of the volume into the density as density itself denotes simply the mass of unit of volume.

This criticism seems entirely valid. Dugas [3, p. 342] attempts to defend Newton on the grounds that Newton was attempting to define mass in more familiar terms and granted this objective he could hardly have done better. However it is dubious whether 'density' is more familiar than 'mass', and at all events the definition does not serve the Machian function of linking the theoretical concept of mass to observables.

Mach goes on to quote Newton's definitions of force (which we will omit) and the three famous laws which for completeness we will quote:

Law 1. Every body perseveres in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon.

Law 2. The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

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Law 3. To every Action there is always an equal Reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts. Mach has this to say [8, p. 302]:

We readily perceive that Laws I and II are contained in the definitions of force that precede .... The third law apparently contains something new. But ... it is unintelligible without the correct idea of mass ....

We can expand these points as follows. The second law (in modern notation $\bar{P} = m\bar{f}$), Mach regards as a definition of force but one which presupposes a definition of mass. The first law is merely a special case of the second because if $\bar{P} = 0$, $\bar{f} = 0$ and only motion in a straight line with constant velocity is possible. Thus for Mach the first two laws are definitions. The empirical content of the theory is contained in the innocuous looking third law. Mach's idea is to use the experimental facts which lie behind this law to form a definition of mass. The theory of mechanics will then follow without circularity.

These 'facts' can be stated thus. Suppose we have two bodies which interact with each other. The form of interaction can be any of a large number of different kinds. The bodies may collide, they may be connected by a spring, there may be electrical or magnetic interactions between them, or finally they may be heavenly bodies attracting each other by gravity. In all these cases we observe the following law [8, p. 303]:

a. Experimental Proposition. Bodies set opposite each other induce in each other, under certain circumstances to be specified by experimental physics, contrary *accelerations* in the direction of their line of junction.

This is the basic law of Mechanics, and leads to the following definition of mass:

b. Definition. The mass-ratio of any two bodies is the negative inverse ratio of the mutually induced accelerations of those bodies.

An early objection to this account was that the preliminary observations on bodies necessary to establish experimental proposition a could only be made on an astronomical scale. How then could mechanics be terrestrial as well as celestial? Mach is much concerned to refute this [8, p. 267]:

H. Streintz's objection ... that a comparison of masses satisfying my definition can be effected only by astronomical means. I am unable to admit .... Masses produce in each other accelerations in impact, as well as when subject to electric and magnetic forces, and when connected by a string in Atwood's machine.

There are, however, further points which need to be established before the definition of mass can be regarded as adequate.

Let us suppose we have three bodies A, B and C. We measure the mass-ratio of $A:B$, $B:C$ and $A:C$ using the Machian method. Let us suppose these come to m_{AB}, m_{BC} and m_{AC}. We obviously require that $m_{AC} = m_{AB} \cdot m_{BC}$. However, we have not postulated any experimental law that will ensure that this is the case; that will ensure, in effect, that masses are comparable. Suppose now that such a law was not true. Then by suitably arranging three masses we could obtain a perpetual motion machine of a kind which we know does not exist. This point together with a further counter to Streintz's objection is contained in Mach's experimental proposition c [8, p. 303]:

> The mass-ratios of bodies are independent of the character of the physical states (of the bodies) that conditions the mutual accelerations produced, be those states electrical, magnetic, or what not; and they remain, moreover, the same whether they are mediately or immediately arrived at.

This second experimental proposition is really little more than an expansion and completion of the first. Mach now gives a third experimental proposition which really is something new. It in effect states that force is a vector. Since force is later to be defined in terms of acceleration, it takes the form:

> *d. Experimental Proposition.* The accelerations which any number of bodies $A, B, C, \ldots$ induce in a body K, are independent of each other. (The principle of the parallelogram of forces follows immediately from this.)

Mach now concludes his account by defining:

> *e. Definition.* Moving force is the product of the mass-value into the acceleration induced in that body.

The first question we shall raise concerns Mach's experimental propositions. Consider for example the crucial experimental proposition a. In order to check this one would need to observe a large number of bodies interacting in various ways and study the accelerations they produced in each other. Had any such observations been made before Newton or had they been made long after the acceptance of Newtonian mechanics? Certainly not. Admittedly, *now*, using various machines and devices, we could perhaps carry out these observations, but the very machines we use would have been designed in accordance with Newtonian mechanics whose foundations we are supposed to be checking! Plausible though Mach's account seems at first sight, it is not clear on reflection that the experimental basis is really established without circularity.

Let us now pass from Mach's operational definition of mass to the more general formulation of operationalism to be found in Bridgman's *Logic of Modern Physics* [2]. We see in his book the influence of Mach. In fact Bridgman begins by criticizing the Newtonian notion of absolute time on the grounds that the Newtonian formulation does not enable us to measure absolute time. This criticism is almost the same as Mach's [8, p. 271 following]. It is true that Bridgman quotes Einstein on this point, but, to some extent, this puts the cart before the horse as Einstein had himself been greatly influenced by Mach in his rejection of the absolutes.

After these preliminaries, Bridgman comes to his main thesis which he expands in terms of the concept of length [2, p. 5]:

To find the length of an object, we have to perform certain physical operation. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*

Unfortunately, Bridgman is not such a systematic thinker as Mach and he develops his thesis in a confused way. This is shown most clearly in his treatment of force and mass [2, pp. 102–8]. His first suggestion is that we should define force operationally in terms of a spring balance or more generally in terms of the deformation of an elastic body. Our next development of the force concept involves considering [2, p. 102]: "an isolated laboratory far out in empty space, where there is no gravitational field." In this isolated laboratory, we first encounter the concept of mass. It is entangled with the force concept, but may later be disentangled. The details of this disentanglement are [2, p. 102]: "very instructive as typical of all methods in physics, but need not be elaborated here." Compared with Mach's lucid account this is sheer muddle.

On the other hand, being an experimental physicist, Bridgman is more concerned with the ways in which measurements are made in practice. This leads him to make a number of points which Mach did not consider and which, oddly enough, tell against the operationalist thesis. A first point is that when we extend a physical concept we have to introduce a new operational definition. Mach's definition of mass would seem to apply to *any* masses, but consider now the case of length. We might begin by defining length in terms of rigid metre sticks. However [2, p. 11]:

> If we want to be able to measure the length of bodies moving with higher velocities such as we find existing in nature (stars or cathode particles), we must adopt another definition and other operations for measuring length....

Of course our different operational definitions must agree where they overlap, but there is another complication. Let us take the first simple extension of the concept of length. Suppose we wish to measure large terrestial distances of the order of several kilometers, say. We have to supplement our use of metre sticks with theodolites. Now to use these instruments we have to make certain *theoretical* assumptions. For example, we must assume that light rays move in straight lines and that space is Euclidean. However, as Bridgman says [2, p. 15]:

> But if the geometry of light beams is Euclidean then not only must the angles of a triangle add to two right angles, but there are definite relations between the lengths of the sides and the angles, and to check these relations the sides should be measured by the old procedure with a meter stick. Such a check on a large scale has never been attempted and is not feasible.

But if such a check is not even feasible, are we justified in making these assumptions which lie behind our operational definition?

Finally, even our simple-minded definition in terms of rigid metre rods has to be subjected to a great many corrections before it can be regarded as adequate [2, p. 10]:

> We must... be sure that the temperature of the rod is the standard temperature at which its length is defined, or else we must make a correction for it; or we must correct for the gravitational distortion of the rod if we measure a vertical length; or we must be sure that the rod is not a magnet or is not subject to electrical forces.

But how are we to introduce these corrections? The case becomes worse if we remember that the concepts involved in the corrections must *themselves* be operationally defined. Does this not lead to a vicious circle? Popper for one thinks it does [10, p. 62]:

> Against this view (operationalism), it can be shown that *measurements presuppose theories*. There is no measurement without a theory and no operation which can be satisfactorily described in non-theoretical terms. The attempts to do so are always circular; for example, the description of the measurement of length needs a (rudimentary) theory of heat and temperature-measurement; but these in turn involve measurements of length.

Let us now examine the consequences of all this for the Machian point of view. As a matter of fact both Mach and Bridgman did at least partially realise that measurements presuppose theories. We have already mentioned Bridgman as asserting that theodolite readings presuppose theories about light rays and the geometry of space. Mach admittedly would have baulked at the use of the word theory. He says [8, p. 271]:

> All uneasiness will vanish when once we have made clear to ourselves that in the concept of mass no theory of any kind whatever is contained, but simply a fact of experience.

On the other hand, he does recognize that an operational definition of mass must be based on certain laws (his experimental propositions *a* and *c*). These 'facts' are really universal laws and can justly be referred to as theories.

We are now in a position to define the operationalist position more precisely, and in future we will use the word 'operationalism' only in this restricted sense. What it amounts to is this: every new concept introduced into physics must be given an operational definition in terms of experimental procedures, and concepts already defined. The empirical laws which lie behind these definitions must be established by observations before introducing the new concept. Bridgman's various points about experimental method raise two objections against 'operationalism' in this sense.

First of all one single operational definition does not suffice for most concepts. As the use of the concept is extended to new fields, it must be given new operational definitions. It is very difficult to see how the laws on which the operational definition is based can be verified without considering the new concept itself. Imagine for example verifying that space is Euclidean before introducing the concept of length! We shall call this: the objection from conceptual extension.

Secondly we have an objection concerned with the correction and improvement of methods of measurement. Suppose we introduce a naive definition of length in terms of rigid metre rods and employed it to measure lengths up to say half a kilometre. Then the theodolite method is discovered. At once it is employed for lengths of more than 50 metres. Now normally we would say that a *more accurate* method of measuring lengths more than 50 metres had been discovered. On the operationalist view however this manner of speaking is inadmissible. We have *defined* length by the rigid procedure and the most we can say of another method

metre rod

of measurement is that it gives results in approximate agreement with the defining procedure for length. It makes no sense to say that the results given by the alternative method are nearer to the true value of the length than those given by the defining method. That would be like first *defining* a metre as the distance between these two marks on this rod and then saying that more accurate measurement had revealed that the distance was not a metre.

The situation is the same when we come to consider the 'corrections' for temperature, gravitational and electrical distortions, etc. mentioned earlier. Suppose again we had defined length in terms of a measuring procedure using iron rods but without taking temperature corrections into account. One day bright sunshine falls through the windows of the laboratory, heating both the measuring rod and the wooden block being measured. It is observed that relative to the rod the wooden object has changed its length from the day before (in fact contracted). However an intelligent experimeter then suggests that in fact the measuring rod has expanded more than the wooden block. He cools down the rod to normal room temperature and produces a more correct value of the new length of the block. Indeed he now shows that it has expanded rather than contracted. But how is this admissible on the operationalist point of view? Length has been defined by the initial set of procedures and according to this definition the block must have contracted rather than expanded.

The only line the operationalist can take on this is to say that we have decided to adopt a new definition of length. Our naive rigid-metal-bar definition is replaced for distances over 50 metres by a theodolite definition while in certain other circumstances a temperature correction is introduced. But the operationalist now has to give an account of how new definitions are evolved and why we choose to adopt one definition rather than another. Further, in view of Popper's point, he has to show that the new definitions do not involve circularity since many of the correcting terms must themselves be given operational definitions.

These two objections indicate the grave difficulties which stand in the way of any systematic operationalist account of the introduction and development of the concepts of physics. Mach's definition of mass for example gives only the barest beginnings of such an account. These difficulties are I believe insuperable.

Having criticized operationalism, it is now worth pointing out that it is an attempt to solve a serious and difficult problem in the philosophy of science. This difficulty can be described using the notion of 'empirical meaning' or 'empirical significance'. Let us say that a concept has 'empirical meaning (or significance)' if we can assign numerical values to particular instances of it – if we can, in effect, measure it under certain circumstances. Thus the concepts of force and mass have empirical meaning because we can, at least in some cases, measure the masses of bodies and the forces acting on them. But now we can ask: how can new concepts acquire significance? If they are not defined in terms of observables or by means of the methods used to measure them, how *do* they acquire meaning? This we shall call 'the problem of conceptual innovation'.

Our method of tackling this problem will be to give a detailed historical analysis of the introduction of the Newtonian concepts of force and mass. In the next section (Section 2) we will begin by outlining the background knowledge in astronomy and mechanics against which Newton developed his theory. We will then discuss how the theory was tested initially, paying particular attention to the role played by the new concepts of force and mass in these tests. Finally we will consider how forces and mass came to be measurable. We can then, in Section 3, generalize from this example to give a theory of conceptual innovation in the exact sciences. It will be shown that this theory avoids the difficulties which we have noted in operationalism.

2. Force and Mass

Our aim is to study the general problem of conceptual innovation by examining Newton's introduction of the concepts of 'force' and 'mass'. It could first be asked, however: "Is this example of conceptual innovation a genuine one? Did not some idea of 'force' and 'mass' exist before Newton?" Well of course *some* idea of these concepts did exist but very little, so that the example is a surprisingly good one. We can appeal to the authority of Mach on this point. He says [8, p. 236]:

> On perusing Newton's work the following things strike us at once as the chief advances beyond Galileo and Huygens:
> (1) The generalization of the idea of force.
> (2) The introduction of the concept of mass...

Naturally, however, an appeal to authority is not a very satisfactory method of argument, so we will attempt a brief survey.

Let us first take the concept of mass as distinct from weight. In a sense of course Descartes drew a clear distinction between mass (or quantity of matter) and weight. He identified matter with spatial extension and thus quantity of matter was measured by volume. It naturally followed that quantity of matter was not proportional to weight. Indeed a vessel when filled with lead or gold would not contain more matter than when 'empty', i.e. filled with air (Descartes' own example). It seems to me that this concept of 'quantity of matter' is too different from Newton's to be considered as forerunner of the latter. Some authors have given Huygens the credit for being the first to distinguish 'mass' and 'weight'. In his treatise, 'De Vi Centrifuga', Huygens says: "the centrifugal forces of unequal bodies moved around equal circumferences with the same speed are among themselves as the weights or solid quantities – inter se sicut mobilium gravites, seu quantitates solides." (quoted from Bell, 1, p. 118). It has been suggested that this is the earliest hint of a distinction between mass and weight. There are also two notes in Huygens' manuscripts of 1668 and 1669, namely [Bell, 1, p. 162]:

(a) Gravitatem sequi quantitatem materiae cohaerentes in quolibet corpore.
(b) Le poids de chaque corps suit precisement la quantité de la matière qui entre dans sa composition.

For my part I find the 'De Vi Centrifuga' quotation unconvincing. Huygens could simply be using 'solid quantity' as a synonym for 'weight'. The manuscript quotations are more striking. However at all events these ideas of Huygens can be ignored when taking account of the background of Newton's thought. Although the 'De Vi Centrifuga' was composed around 1659, it was not published till 1703, while the Huygens' manuscripts were not published till our time.

The third possible claimant to the concept of mass is Kepler. In the introduction to the *Astronomia Nova* (1609), he says [quoted from Koestler, 7, p. 342]:

If two stones were placed anywhere in space near to each other, and outside the reach of force of a third cognate body, then they would come together, after the manner of magnetic bodies, at an intermediate point, each approaching the other in proportion to the other's mass (moles).

This remarkable passage contains already the principle of universal gravitation, but, as Koestler rightly points out, it remained an isolated insight. Kepler later developed his celestial dynamics on other principles. Thus to a first approximation at least we can say that the concept of mass as distinct from weight is original to Newton.

The case of force is not so clear cut. We must admit that in the study of Statics and Equilibrium a notion of force had evolved, but it was little more than a slight generalisation of the idea of weight. As Mach says [8, p. 57]:

> Previous to Newton a force was almost universally conceived as the pull or pressure of a heavy body. The mechanical researches of this period dealt almost exclusively with heavy bodies.

The two main statical laws that had been discovered at that date, namely the law of the lever and the law of the inclined plane, can in fact be stated using only the notion of weight. On the other hand, the idea of weight had been generalised to give the notion of a tension in a string. This appears, for example, in the second day of Galileo's *Two New Sciences* (Galileo, 5 p. 122).

In 1672, at the end of his *Horologium Oscillatorium*, Huygens published 13 propositions without proof on centrifugal force. He considers a centrifugal force as a real force which balances the tension in a string in the same way as the weight of a body. We can therefore take Huygens' concept of centrifugal force as a generalisation of the previous notion of statical force. However once again I am inclined to exclude Huygens' work from an enumeration of the background to Newton's thought. Admittedly the 13 propositions were published in 1672, long before the *Principia*. However, as Herivel has shown in [6], from a consideration of early manuscripts, Newton evolved his own ideas of centrifugal force in the period 1666–9 and independently of Huygens.

In dynamics too there had been some notion of force. Galileo had worked with a concept of impeto – no doubt derived from the medieval thinkers. But this notion – in so far as it was quantitative – corresponds more closely to the modern notion of momentum than to the Newtonian idea of force. Again Kepler has a theory of bands of force or influence emanating from the sun and carrying the planets round like the spokes of a wheel. However these ideas of Kepler, and indeed of Galileo, were never put on a quantitative basis and were not needed in the statement of these authors' main results. I conclude that Newton's *quantitative* notion of *dynamical* force was indeed original to him. More generally, we can say that we do genuinely have here a case of conceptual innovation and a careful study of it should tell us a great deal about the way in which new concepts can be developed.

We can now state the main quantitative results which had been achieved before Newton in mechanics and astronomy. These were, of course, Kepler's and Galileo's laws. Despite their familiarity it might be worth repeating them briefly. Kepler's laws are three in number:

(a) Every planet moves in an ellipse with the sun at one focus.

(b) The radius vector from the sun to a planet sweeps out equal areas in equal times.

(c) If a is the mean distance from the sun to a planet and T is the time of a full revolution of the planet (the length of the planetary year), then $a^3/T^2 = \text{constant}$.

The only point worth making about these laws is that they were mixed up in Kepler's work with a great deal that was incorrect. In particular, of course, Kepler had a theory about the relation of the solar system to the five regular solids, and he considered *this* theory to be his greatest scientific achievement – much finer than the 3 laws. It therefore required considerable selectivity on Newton's part to obtain *just* those three laws from Kepler.

Galileo's results can be summarised very conveniently into 2 laws, namely:

(a) Neglecting air resistance freely falling bodies have a constant downward acceleration g.

(b) Neglecting air resistance again, bodies which are smoothly constrained to move at angle α to the horizontal (e.g. by an inclined plane) have an acceleration $g \sin \alpha$.

To these main results we may add numerous astronomical observations concerning the moon, which, as we shall see, proved important.

In this statement I have once again rather over-simplified. Other results were known in mechanics – for instance the laws of impact. I think, however, that it is better to omit these as their inclusion would only complicate the discussion without adding any new point of importance.

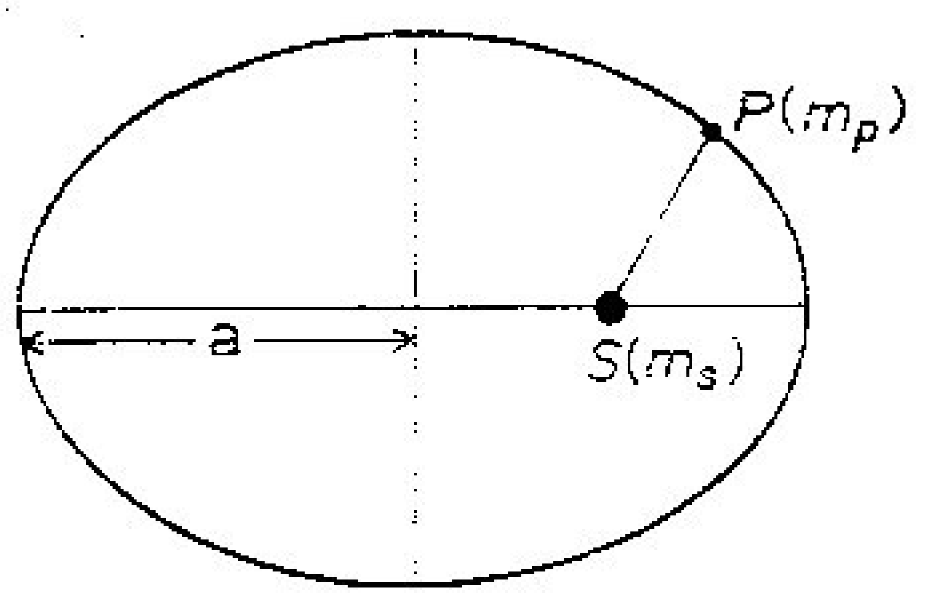
We are now in a position to examine how Newton's theory was checked against experience prior to its acceptance. In doing so we must not fall into the error of supposing that we can test out Newton's laws separately. Indeed this error was committed by Newton himself because, after each law in the Principia, he gives the 'experimental evidence' for it. However in fact the three laws of motion *and* the law of gravity can, in the first instance, only be matched against experience *all together*. This I believe is the source of much of the misunderstanding of Newtonian mechanics. In most of the standard expositions the three laws of motion are first introduced and their consequences for problems of terrestial mechanics are dealt with. Then in a separate section (usually the final chapter or even an appendix to the book) the law of gravity is stated and some of its astronomical consequences are mentioned. This conceals the fact that Newton's 4 laws form a unified cosmological theory and that they were tested out in the first instance on an astronomical scale. Essentially the theory was checked against experience by showing that all the previous results in mechanics, i.e. Kepler's laws, Galileo's laws, etc., could be shown to hold in a high degree of approximation if the theory were true. We must now examine how the concepts of 'force' and 'mass' were used in this deduction.

Newton's theory can be summarised in the familiar vector equations:

$$\bar{P} = m\bar{f} \text{ (which contains the 3 laws of motion)}$$

and

$$\bar{F} = (\gamma m_1 m_2 / r^3)\bar{r} \text{ (the law of gravity)}$$



Let us apply these equations to a planet P, mass m_P moving round the sun S, mass m_S. We first neglect the gravitational interactions holding between the planets themselves. The problem is then reduced to a 2-body problem, and we obtain that P moves on an ellipse of major semi-axis a, say. If the period of its orbit is T, then

$$a^3/T^2 = \gamma(m_s + m_p)/4\pi^2. \qquad (1)$$

We now assume that the mass of the sun is very much greater than that of the planet ($m_S \gg m_P$) and so obtain

$$a^3/T^2 \doteqdot \gamma m_s/4\pi^2 \quad \text{(i.e. constant).}$$

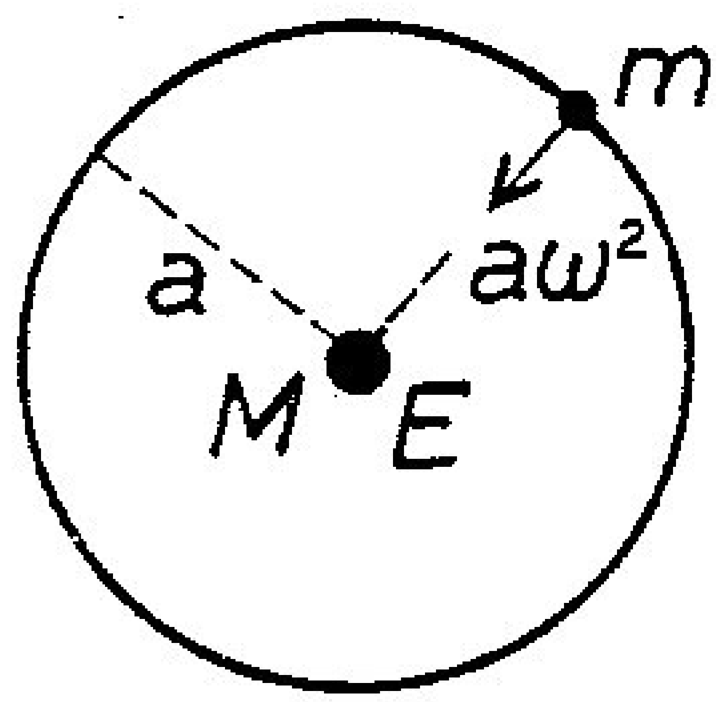
This is an approximate version of Kepler's 3rd law. The assumption $m_S \gg m_P$ though automatically made and easily over-looked for this reason contains the solution to the problem we have been discussing. Do we need an operationalist definition of mass at this point? Not at all! We test out our theory involving masses by making the qualitative physical assumption that one mass is very much greater than another. Moreover this qualitative assumption is justified by a crude (or intuitive) notion of mass. If we think of mass as 'quantity of matter', then observing that the sun is very much larger than the planets and making the reasonable postulate that the density of its matter is at least comparable to that of the matter in the planets, we obtain that $m_S \gg m_P$. So we do not at first need a precisely defined notion of mass. A rather crude and intuitive notion of mass can lead to a qualitative assumption and so to a precise test of a theory involving an exact idea of mass.

Let us now examine how approximations to Galileo's laws are obtained. Write M for the mass of the earth, R for its radius, and m for the mass of a small body at height h above the earth's surface. We now have to use a theorem of Newton's that we can replace a sphere whose mass is distributed with spherical symmetry by a mass point at the centre of the sphere for the purpose of calculating gravitational forces. This theorem incidentally was one which gave Newton a great deal of trouble. He only succeeded in proving it in 1685. It now appears as Principia Book 1 Prop. 76.[2] Using it we have that the mass m is, by the law of gravity, acted on by a force directed towards the centre of the earth and of magnitude $\gamma mM/(R+h)^2$ where γ is the universal constant of gravitation. As $R \gg h$ we can write this approximately as $\gamma mM/R^2$. Now $\gamma M/R^2$ is a constant for all bodies $m(=g$, say). Therefore a body m is acted on by a downward force approximately equal to mg. Since $\bar{P} = m\bar{f}$ this gives a downward acceleration of approximately g in free fall. If, on the other hand, the body is acted on by a smooth constraint at angle α to the horizontal, the component of the force at angle α is approximately $mg \sin\alpha$ (since force is a vector). Therefore the body's acceleration is approximately $g \sin\alpha$. In this way, approximations to Galileo's two laws follow.

It will be noticed that in this derivation too an approximation is made

– namely $R \gg h$. However this is not an assumption involving new concepts, but a statement involving two quantities which were already quite well-known. After all, the first measurements of R date back to Eratosthenes. We see that in this case the laws are derived by *completely eliminating* the new concepts in the deduction. It might be asked: could we test out a new theory with new concepts by the following method: make a series of deductions from the theory in which the new concepts are completely eliminated and compare the results we obtain with experience? I myself do not think so. We might derive some of our results (like the approximations to Galileo's laws) in this way; but I think we need to obtain at least some of the others by qualitative assumptions involving new concepts (e.g. $m_S \gg m_P$). My reason is this. If the new concepts could always be eliminated before a comparison with experience took place, we would be inclined to regard the new concepts not as being physical quantities, but rather as mathematical coefficients introduced to make the calculations easier. A good example of such a 'mathematical coefficient' is $(-1)^{1/2}$ as it is used in the theory of electrical circuits. Here we always write the current i in the complex form $i_0 \exp(-1)^{1/2} \omega t$. This mathematical device greatly simplifies all the calculations. Yet we never give a physical meaning to the imaginary part of the expression. At the end of the calculation the real part of $i_0 \exp(-1)^{1/2} \omega t$ is taken and compared with the experimental findings. However the concept of mass is not in this position.

The deduction of the approximate truth of Kepler's and Galileo's laws provided the main evidence for Newton's theory. However it is interesting also to consider the test of the theory made by observing the motion of the moon. This moon-test was in fact Newton's first test of the law of gravity, and in the course of considering it we can examine Newton's own attitude to the questions we have just been discussing. The logic of the test (using modern methods) is as follows:



Let the earth E have radius R and mass M. Let the radius of the moon's orbit be a and its period (i.e. the lunar month) be T. If the moon's angular velocity is ω its centripetal acceleration is $a\omega^2$ and the gravitation force on it is $\gamma Mm/a^2$. Therefore, since $\bar{P} = m\bar{f}$,

$$\gamma Mm/a^2 = ma\omega^2 .$$

But

$$\omega = 2\pi/T$$

Therefore

$$\gamma M/4\pi^2 = a^3/T^2 \tag{2}$$

On the surface of the earth we have by a previous calculation

$$\gamma Mm/R^2 \doteqdot mg .$$

Therefore

$$\gamma M \doteqdot gR^2 .$$

Substituting, we obtain

$$g \doteqdot (4\pi^2/R^2)\,(a^3/T^2) .$$

Now all the quantities in the R.H.S. of this equation are known. So we may calculate the value of g from the equation, and this can be compared with the value of g observed by means of pendula. When Newton first performed this test (c. 1666), he found a noticeable discrepancy between the two values of g. This led him to abandon his theory for a while. However the disagreement was due to a faulty value of the earth's radius. When he tried the test again much later (between 1679 and 1684) with a corrected value of the earth's radius, it gave agreement within experimental error and this was one of the factors which stimulated him to push his work on the Principia through to its completion.

It looks as if the deduction just made is similar to the deduction of Galileo's laws, i.e. the new concepts simply cancel out. However this is not in fact so. The equations are derived on the assumption that the earth is fixed. Eppur si muove, and we therefore should substitute for (2) our Equation (1), i.e.

$$\gamma(M + m)/4\pi^2 = a^3/T^2 .$$

To regain (2) and carry through the deduction, we must again assume that m is negligible compared with M (i.e. mass(earth) $\gg$ mass(moon)). So the case is really the same as the deduction of the approximation to Kepler's 3rd law.

We can now examine what Newton himself says about this. He describes the moon-test in Principia Book III Prop. 4, and after giving a deduction equivalent to our original one (but using his own mathematical methods), he continues [9, p. 409]:

This calculus is founded on the hypothesis of the earth's standing still; for if both earth and moon move about the sun, and at the same time about their common centre of gravity, the distance of the centres of the moon and earth from one another will be $60\frac{1}{2}$ semidiameters of the earth: as may be found by a computation from Prop. LX, Book I.

Book I, Prop. 60, in effect introduces the corrected Equation (1) instead of the original Equation (2). We can, I think, criticize Newton's logic here. To introduce the corrections he speaks of we need to know the value of the ratio of the moon's mass to the earth's mass. Now this ratio can be calculated once Newton's theory is assumed by a method which we will explain in a moment. However, when Newton's theory is being given its first tests prior to its acceptance, we cannot introduce the exact correction. In fact, in order to get the test at all we have to introduce the qualitative assumption mass(earth) $\gg$ mass(moon), as we have already shown.

Similar criticisms can be raised against Newton's general method in Book III of the Principia. He begins by stating Kepler's laws, the motions of the moon and of the satellites of Jupiter and Saturn as Phenomena 1–6. Then using his rules of reasoning he infers the law of gravity inductively in Book III Props. 1–5 and 6. Finally, assuming the law of gravity, he derives Kepler's laws deductively in Props. 13–16, but this time he incorporates certain corrections. Thus he says at the beginning of Prop. 13 [9, p. 420]:

We have discoursed above on these motions from the Phenomena. Now that we know the principles on which they depend, from these principles we deduce the motions of the heavens *a priori*.

He then goes on to mention some of the corrections which must be introduced. He claims that broadly speaking [9, p. 421]: "the actions of the planets one upon another are so very small that they may be neglected ...." However he goes on to say that Jupiter and Saturn noticeably affect one another in conjuction, and that the orbit of the earth is [9, p. 422]: "sensibly disturbed by the moon."

Newton does not realize that these corrections actually vitiate his inductive-deductive approach. They vitiate it because, as Duhem was the first to point out in [4], Part II, Ch. VI, Part 4, pp. 190–5, the law of gravity is strictly speaking inconsistent with the phenomena from which it was supposedly induced. Moreover, it is very implausible to claim that from given premises we can induce conclusions which logically contradict the premises. Newton's approach also conceals the role which the new concepts 'force' and 'mass' play in the derivation of approximations to Kepler's and Galileo's laws. The role we have tried to analyse in this section.

Duhem's point can be used to provide an additional argument against Mach's philosophy of science. According to Mach, high level mathematical theories (such as Newton's) are merely summaries of experimental laws, and are introduced for 'economy of thought'. Thus presumably Newton's theory is a summary of Kepler's laws, Galileo's laws, the laws of impact, and perhaps other things. But this is not so because Newton's theory, far from summarizing e.g. Kepler's laws, strictly speaking contradicts them. Only a certain approximation to Kepler's laws follows from Newton's theory. We could say that Newton's theory corrects Kepler's original laws; but this is unaccountable on Mach's position. After this brief digression, let us complete our account of how new concepts come to be measurable.

Once a new theory involving new concepts has passed a number of preliminary tests, we can accept it provisionally and use it to devise methods for measuring the values which the new concepts assume in certain particular cases. We will now illustrate this in the example of Newton's theory which we are considering. First let us consider how the mass of a planet might be measured assuming for the moment that the sun has unit mass. Let the planet be distant a_P from the sun and have orbital period T_P. Then assuming $m_S \gg m_P$ we have as usual:

$$a_P^3/T_P^2 \doteqdot \gamma m_S/4\pi^2 .$$

But now suppose the planet has a moon M of mass m_M which is distant a_M from the planet and has orbital period T_M. If we assume again that

$m_P \gg m_M$, we get as before

$$a_M^3/T_M^2 \doteqdot \gamma m_P/4\pi^2 .$$

Therefore dividing we obtain

$$m_P/m_S \doteqdot (a_M/a_P)^3(T_P/T_M)^2 .$$

All the quantities on the R.H.S. of this equation can be determined by astronomical measurement and so we obtain a value for the ratio m_P/m_S. Calculations of this sort are given in the Principia Book III Prop. 8.

For example Mars has a moon Deimos whose period is 30.3 hrs. We hence obtain $m_{\text{Mars}} : m_{\text{Sun}} = 3.4 \times 10^{-7}$. Incidentally, this calculation confirms our original assumption that mass (Mars) $\gg$ mass (Sun), but it is worth noting that to make it, we have to assume not only Newton's theory but also that mass (Deimos) $\ll$ mass (Mars).

As regards measuring the masses of terrestial bodies, the case is so trivial that it is hardly worth mentioning. We identify the downward gravitational force on a body with its weight. But since the downward force is mg and g is constant, we obtain mass $\propto$ weight. This gives us a method of measuring the masses of bodies. The only point worth mentioning is that the theory enables us to correct for the variation in g, which can itself be measured by means of pendula.

We will now attempt to generalise from this example of Newton's theory to obtain a general account of conceptual innovation in the exact sciences. It can then be pointed out that this account avoids the difficulties inherent in operationalism. These matters will occupy us in the next section.

3. Conceptual innovation in the exact sciences

Let us now develop the ideas we have acquired from the case of mechanics by applying them to the problem of introducing the concept of temperature. This time we will not attempt an historical analysis, but confine ourselves to giving a hypothetical series of theories and tests which would have enabled a precisely measurable concept of temperature to evolve without circularity. Some details of the actual history may be found in Roller [11]. Our suggestion is that we begin by proposing the following law: 'For rods or columns of a large number of different materials $\theta \propto l$, where θ is the temperature of the rod or column and l its length.' Now the interesting thing about this law is that 'θ' and 'l' both stand for *new* concepts. We are not assuming any prior notion of length, for the measurement of length, as Popper says, "needs a (rudimentary) theory of heat" (Popper [10], p. 62. Quoted earlier on p. 10). But if both θ and l are new concepts, how can we test the law? The case seems altogether hopeless.

It is not, however, as hopeless as it seems. Once again we proceed by making a series of qualitative assumptions of the form: 'In such and such circumstances the temperature of this body is approximately the same as the temperature of that body', 'the lengths of these two bodies are nearly the same', etc. These assumptions enable us to obtain certain results which can be compared with experience, thereby testing out our law $\theta \propto l$. We will now analyse how this comes about.

Our first step is to select two fixed points on the temperature scale. These are of course melting ice, and boiling water. It is assumed (i) that these two points represent approximately constant temperatures, and (ii) that any body immersed for a sufficient long time in the melting ice or boiling water will acquire approximately at least the *same* temperature as the melting ice resp. boiling water. Assumption (ii) plays much the same role as the assumption that $m_0 \gg m_1$ in the Newtonian case. We can now make a first crude test on our law. If it is assumed that the constant of proportionality in $\theta \propto l$ differs for different materials, then we will expect that rods or columns which have the *same* length in melting ice (which we can call $\theta = 0$) will have different lengths in boiling water ($\theta = 100$). This can be checked. To do so we need not have a general method of measuring length but only an ability to check that two lengths are approximately the same (by putting them end to end), and of judging that one length is greater than another.

Of course we have not really checked the relation $\theta \propto l$, only that length and temperature vary together and at different rates for different materials. However our results show that certain materials, e.g. wood, show very little variation even between temperatures as different as 0 and 100. We now assume that room temperature varies very much less over a period of a few weeks than the difference between 0 and 100. So if we fix on some standard distance, we can construct a rough instrument for measuring length, viz. a ruler.

We have almost reached a position where $\theta \propto l$ can be tested but there are still difficulties. We need to use materials for which the variation is large, and we have to ensure that although the material itself is at various temperatures the length-measuring instrument, i.e. the ruler, is at room temperature (assumed approximately constant throughout the experiment). The way in which these difficulties are overcome is well-known. We choose for our materials different liquids (mercury, alcohol, etc.) encased in thin tubes of glass, closed at one end and terminating in bulbs at the other. The bulb is immersed in the melting ice, boiling water, etc., but the ruler is held against the glass tube at room temperature. It may now be objected that we can only calibrate the thermometer by assuming linearity. In fact, however, we can first test out various consequences of the law $\theta \propto l$. If these are staisfied, we assume the law and use it for our calibration. What then are these preliminary tests?

Let t stand for room temperature, assumed constant throughout the experiment. Let us consider a particular material, say a column of alcohol, and suppose that for it $\theta = kl$. Let us measure its lengths using the ruler at 0, t, 100 and obtain l_0, l_t, l_{100}. Then

$$100 = k(l_{100} - l_0)$$
$$t = k(l_t - l_0).$$

Therefore

$$t = 100(l_t - l_0)/(l_{100} - l_0).$$

Therefore for all substances, we have (on a given day) $100(l_t - l_0)/(l_{100} - l_0)$ is approximately constant. We have here a consequence which can be tested with the crude means at our disposal. Further, we can vary the experiment by taking another fixed point, say the temperature (τ) of a mixture of ice, salt and water, and checking that again $100(l_\tau - l_0)/(l_{100} - l_0)$ is approximately constant for all materials.

We see that once again no operational definition of length or temperature is necessary. We introduce a hypothesis or theory involving these concepts, and test it out in certain ways. An instrument for measuring temperature, viz. the thermometer, is then designed on the basis of the theory. In order to make the tests we have to add to our theory certain qualitative assumptions about the new concepts. The only difference from the Newtonian case is that *there* the qualitative judgments were judgments of inequality ($m_0 \gg m_1$), whereas here they are judgments of equality, viz. (a) the temperature of certain processes, i.e. melting ice and boiling water, is approximately constant; (b) the temperature of two bodies which have been immersed for a long time in boiling water is approximately the same and equal to that of the boiling water; and (c) two rods are the same length (at the temperature in question) if one can be exactly superimposed over the other.

Let us now pursue the development of the temperature concept a little further. Having obtained a method of measuring temperature (the mercury thermometer) we can now test some other laws involving temperature. For example, the gas law $PV = RT$. This law holds very well for gas at very low pressures and we may therefore use it to design a very accurate (though cumbersome) instrument for measuring temperature – the so-called ideal gas thermometer. We can use this instrument in turn to test out further laws – say the thermocouple effect; and this effect can be used in its turn to provide a method for measuring small temperature differences. Now we come to an interesting point. Using our thermocouple, we can test out one of our original assumptions – say that two bodies immersed in boiling water have the same temperature. We may well find that this assumption holds only approximately but not exactly. Our new methods of measurement transcend our original crude ones; but, on the other hand, the original crude assumptions and methods were necessary before the sophisticated and exact methods could be developed.

I have two analogies to illustrate this situation. The first one concerns the process of liquifying a gas. One standard method here is to use the Joule-Kelvin effect. On the other hand, the Joule-Kelvin effect will only cool the gas further if it is already at a sufficiently low temperature. Let us suppose that the gas is initially above this critically temperature. It must then be cooled below it, using some method less sophisticated than the Joule-Kelvin effect. Similarly, we sometimes have to use a cruder method of measurement to test out the theories on which a more sophisticated method of measurement is based. Another analogy is with the method of finding numerical values for the roots of equations by successive approximation. Usually there is an iterative process. We start with some very crude approximation and by applying a certain procedure we obtain a better value. This value is then the starting point for a new application of the procedure etc. After several repetitions we may obtain a very accurate value but this was only possible because of our initial crude approximation.

There is another point worth raising here. We may well discover that our original crude assumption and crude law ($\theta \propto l$) do not hold exactly. Indeed we will naturally hope to correct them because in so doing we will have improved on the situation which held before. On the other hand, suppose that we show that our original laws and assumptions are not just inexact but *wildly wrong*. If this turned out to be the case, we would be in an embarrassing situation. A kind of contradiction would have arisen in the notion of temperature and we would have either to abandon the concept completely or reconstruct it painfully from crude beginnings.

We are now in a position to state, in general terms, out theory of conceptual innovation. Let us suppose a new theory is proposed involving new concepts $C_1, \ldots, C_n$. Our problem was: how do these concepts become measurable? how do they acquire empirical significance? The answer is this. We first test the new theory by deducing from it consequences which do not involve the new concepts and comparing these consequences with experience. In some cases the deduction is strict and the new concepts are eliminated by purely logical moves without making any additional assumptions. This was the case with Galileo's laws. However not all the consequences can be obtained in this way, otherwise the new concepts will be regarded as mathematical auxiliaries similar to $(-1)^{1/2}$ rather than as concepts with physical significance. In general, certain qualitative assumptions of approximate equality or of great inequality in particular physical situations will be made concerning the new concepts. The original theory together with these qualitative assumptions will lead to the conclusion that certain consequences hold approximately. These consequences are then matched against the results of experiments past or future. If the new theory is corroborated by these comparisons, it is accepted and methods for measuring the new concepts are devised on the basis of it. In this way the concepts acquire empirical significance. At a later stage, the original theory, or the qualitative assumptions, may be tested using more sophisticated methods of measurement and found to hold only approximately. The more sophisticated methods could not, however, have been developed without the previous cruder ones.

At the risk of being a little repetitious, we will now point out that this theory avoids the difficulties in operationalism which we noted earlier. The first problem we called the problem of conceptual extension. It was observed that as a concept is extended into new fields we need new operational definitions. The laws on which these new operational definitions are based must be verified "before introducing the concept itself." The simple example we gave of this was extending the rigid metre rod definition of length by using a theodolite. However the theodolite is based on Euclidean geometry whose truth must apparently be verified before introducing the concept of length.

Our main disagreement here is that we regard concepts acquiring meaning *not* through operational definitions, but through their position in a nexus of theories. An account of the logical relations of these theories and of the way we handle them in practice would give us the significance of the concept. Thus a concept can indeed be extended, *not* by acquiring new operational definitions, but rather by becoming involved in a series of new and more general theories. If we accepted the operationalist view, we could not suddenly postulate a new theory with new concepts. The new concepts would only have meaning after they had been operationally defined. An operationalist must therefore check the laws on which his definitions are to be based *before* introducing the concept. We described earlier Mach's attempt to check certain mechanical laws before introducing the concept of mass. In general, however, this programme cannot be carried through as we saw from the absurdity of checking Euclidean geometry without introducing the notion of length. Moreover, from our point of view it is unnecessary. We are quite free to introduce a new undefined concept in a new theory. Our only problem is then how to test this theory and this problem can, as we have seen, be solved.

The second difficulty in operationalism was the question of how the operationalist could give an account of the correction and improvement of methods of measurement. We often, for example, speak of 'discovering a more accurate method of measuring a concept' but if the previous method was the *definition* of the concept, how is any more accurate method of measuring it possible? Again we often introduce corrections for temperature, gravitational forces, etc. But how can be correct a definition?

This difficulty too disappears as soon as we recognise the primacy of theories. Methods of measurement are only introduced on the basis of theories; and there is no reason why starting from a particular set of theories we should not be able to devise two methods of measurement – one more accurate than the other. Again our methods of measurement involve not only the general theories but also certain qualitative assumptions, e.g.

that temperature variations in the laboratory are negligible. We can always replace such an assumption by a more sophisticated one, thus 'correcting' our previous method of measurement.

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NOTES

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[1] A proof of this result is given in Rutherford [12], pp. 66–71.
[2] A proof using modern methods can be found in Rutherford [12], pp. 25–30.