

1. The Logic of Falsification

Falsification is an effective rhetorical strategy. Suppose your opponent defends thesis T . If you can show that T implies Q and Q is false, the opponent has a problem.

Socrates was a master of this technique. In Plato's dialogue *Laches*, for instance, the general maintained that "courage" means "persistence in striving for one's goals in the face of opposition." (I have unpacked Laches' identification of "courage" and "endurance.") Socrates replied that if that understanding of courage is correct then foolhardy or reckless acts of persistence qualify as "courageous," and that such acts clearly are not courageous. Laches agreed and withdrew his thesis that identifies courage and persistence.

This falsification strategy implements the *modus tollens* relation of deductive logic:

$$\begin{array}{c} \text{If } P \text{ then } Q \\ \text{Not } Q \\ \hline \text{Therefore, not } P \end{array}$$

Since *modus tollens* is a valid deduction argument form, it is rational to reject the conclusion "not P " only if one can show that one, or both, of the premises are not true.

Applications of this falsification strategy are widespread within science, even in the early stages of its development. Aristotle, for example, dismissed the suggestion of Herodotus that female fish conceive by swallowing the milt produced by males. He noted that if Herodotus's hypothesis is true then there is a passageway from mouth to uterus. Aristotle maintained that dissections reveal that there is no such passage.¹

Aristotle also applied the falsification strategy to Empedocles' hypothesis that semen that enters a hot womb produces male offspring, whereas semen that enters a cold womb produces a female offspring. Aristotle pointed out that if this hypothesis is true, then twins conceived in the same womb are both males or both females. He noted, however, that there do exist twins one of which is a male and one of which is a female.²

The falsification strategy is particularly effective when a scientist performs an experiment to show that a consequence of a hypothesis is false. An impressive example is Stephen Hales's experiment against the hypothesis of a circulation of sap in plants. According to the circulation hypothesis, sap ascends in the inner core of a plant's stem and descends just inside the outer coating or bark. Hales reasoned that if this hypothesis is correct and a vertical section of the outer coating is removed, then its upper edge should become moist before the lower edge. Hales cut away a three-inch length of bark from an apple branch, placed the branch in water and observed that it was the lower edge of the cut that first became moist.³

HALES'S TREE SAP EXPERIMENT

Experiment XLIII

August 20th, at 1 p.m. I took an Apple-branch *b* (Fig. 26), nine feet long, $1 + \frac{3}{4}$ inch diameter, with proportional lateral branches, I cemented it fast to the tube *a*, by means of the leaden Syphon *7*: but first I cut away the bark, and last year's ringlet of wood, for 3 inches length to *r*. I then filled the tube with water, which was 12 feet long, and $\frac{1}{2}$ inch diameter, having first cut a gap at *y* through the bark, and last year's wood, 12 inches from the lower end of the stem: the water was very freely imbibed, viz. at the rate of $3 + \frac{1}{2}$ inches in a minute. In half an hour's time I could plainly perceive the lower part of the gap *y* to be moister than before; when at the same time the upper part of the wound looked white and dry.

Now in this case the water must necessarily ascend from the tube, thro' the innermost wood because the last year's wood was cut away, for 3 inches length all round the stem; and consequently, if the sap in its natural course descended by the last year's ringlet of wood, and between that and the bark (as many have thought) the water should have descended by the last year's wood, or the bark, and so have first moistened the upper part of the gap *y*; but on the contrary, the lower part was moisten'd and not the upper part.

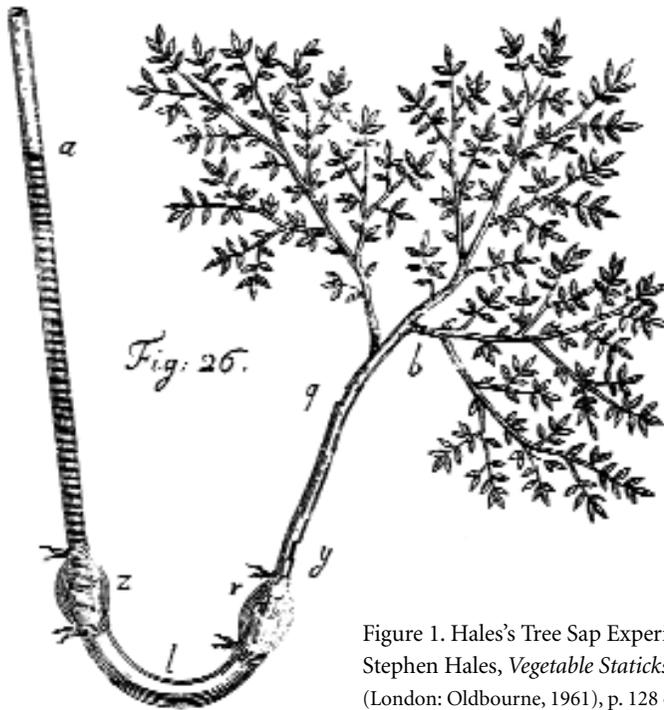


Figure 1. Hales's Tree Sap Experiment.
Stephen Hales, *Vegetable Statics*, 1727
(London: Oldbourne, 1961), p. 128 overleaf.

I repeated this Experiment with a large Duke-Cherry branch, but could not perceive more moisture at the upper, than the lower part of the gap, which ought to have been, if the sap descends by the last year's wood or the bark.

It was the same in a Quince-branch as the Duke-Cherry.

Hales's application of the falsification strategy is straightforward. The circulation hypothesis implies that the upper edge of the cut becomes moist before the lower edge. Direct unaided observation shows that this does not happen. In more complex cases, falsification is achieved only upon theoretical interpretation of observational evidence.

Count Rumford's cannon-boring experiment is another straightforward attempt at falsification.⁴ The experiment was directed against the caloric hypothesis that takes heat to be a substance—an invisible fluid present within bodies. Caloric had been included, along with oxygen, car-

bon, and iron, as an element in Lavoisier's new system of chemistry (1789). The caloric hypothesis enjoyed widespread support during the late 18th and early 19th centuries.

Rumford observed that if heat is a fluid then there is a limit to the amount of heat that can be produced by a mechanical process that generates friction. Rumford showed that the process of boring a cannon barrel produces a seemingly unlimited amount of heat. When conducted within a box containing water the boring process steadily raises the temperature of the enclosing water and eventually boils it.

RUMFORD'S CANNON-BORING EXPERIMENTS

Being engaged lately in superintending the boring of cannon in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of Heat which a brass gun acquires in a short time in being bored, and with the still more intense Heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

The more I meditated on these phænomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of Heat and to enable us to form some reasonable conjectures respecting the existence, or non-existence, of an *igneous fluid*—a subject on which the opinions of philosophers have in all ages been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the Heat actually produced in the mechanical operation above mentioned? Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for all the Heat produced.

But no such change had taken place; for I found, upon taking equal quantities, by weight, of these chips, and of thin slips of the same block of metal sepa-

rated by means of a fine saw, and putting them at the same temperature (that of boiling water) into equal quantities of cold water (that is to say, at the temperature of $59\frac{1}{2}^{\circ}$ F), the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion in which the slips of metal were put.

This experiment being repeated several times, the results were always so nearly the same that I could not determine whether any, or what change had been produced in the metal, in regard to its capacity for Heat, by being reduced to chips by the borer.

From hence it is evident that the Heat produced could not possibly have been furnished at the expence of the latent Heat of the metallic chips.

* * *

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being closed at the same time by means of the circular piston, through the center of which the iron bar passed.

This being done, the box was put in its place, and the joinings of the iron rod and of the neck of the cylinder with the two ends of the box having been made watertight by means of collars of oiled leather, the box was filled with cold water (viz. at the temperature of 60°), and the machine was put in motion.

The result of this beautiful experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had in contriving and arranging the complicated machinery used in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water and touching the outside of the cylinder, that Heat was generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box (the quantity of which fluid amounted to 18.77 lb., avoirdupois, or $2\frac{1}{4}$ wine gallons), that its temperature had been raised no less than 47 degrees; being now 107° of Fahrenheit's scale.

When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the Heat of the water in the box was 142° .

At the end of 2 hours, reckoning from the beginning of the experiment, the temperature of the water was found to be raised to 178° .

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to describe the surprise and astonishment expressed in the countenances of the bystanders, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

* * *

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely, What is Heat? Is there any such thing as an *igneous fluid*? Is there anything that can with propriety be called *caloric*?

We have seen that a very considerable quantity of Heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux *in all directions* without interruption or intermission, and without any signs of diminution or exhaustion.

From whence came the Heat which was continually given off in this manner in the foregoing experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for, in three of the experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident: first, because this water was continually *receiving Heat* from the machinery, and could not at the same time be *giving to*, and *receiving Heat from*, the same body; and, secondly, because there was no chemical decomposition of any part of this water. Had any such decomposition taken place (which, indeed, could not reasonably have been expected), one of its component elastic fluids (most probably inflammable air) must at the same time have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but though I frequently examined the water to see if any air-bubbles rose up through it, and had even made preparations for catching them, in order to examine them, if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

Is it possible that the Heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppo-

sitions appear more improbable even than either of those before mentioned; for Heat was continually going off, or *out of the machinery*, by both these passages, during the whole time the experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the Heat generated by friction, in these experiments, appeared evidently to be *inexhaustible*.

It is hardly necessary to add, that anything which any insulated body, or system of bodies, can continue to furnish without limitation, cannot possibly be a material substance; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the Heat was excited and communicated in these experiments, except it be MOTION.

Rumford's experiments were not a decisive refutation of the caloric hypothesis. He did not establish that an *inexhaustible* amount of heat is generated. Nevertheless, the very great amount of heat released made implausible the hypothesis that the heat produced is a fluid escaping from the metal.

Decades later, a successful competing theory was formulated. According to the kinetic molecular theory, temperature is a measure of the intensity of molecular motions, and heat is the product of an increase in such motions. Supporters of the kinetic molecular theory sometimes cited Rumford's experiments to show that the "heat is an invisible fluid" theory is not a viable alternative.

In the mid-eighteenth century, Pierre de Maupertuis sought to falsify monoparental theories of heredity.⁵ Maupertuis traced the genealogy of a Berlin physician, Jacob Ruhe. Some members of the Ruhe family possessed an extra finger or two (polydactyly). A genealogical chart revealed that this trait is passed to members of the next generation from both male and female parents.

This finding would appear to falsify the then popular theories of ovism and animalculism. Ovism and animalculism are versions of preformationism. Preformationist theories interpret embryological development to be an unfolding of structures already present in the egg or sperm. Ovists (e.g., Swammerdam) held that offspring are encapsulated in the fe-

male egg; and animalculists (e.g., Hartsoeker, Dalenpatius) held that offspring are encapsulated in the male sperm.⁶

MAUPERTUIS'S CONCLUSIONS ABOUT POLYDACTYLY

Jacob Ruhe, surgeon of Berlin, is one of these types. Born with six digits on each hand and each foot, he inherited this peculiarity from his mother Elisabeth Ruhen, who inherited it from her mother Elisabeth Horstmann, of Rostock. Elisabeth Ruhen transmitted it to four children of eight she had by Jean Christian Ruhe, who had nothing extraordinary about his feet or hands. Jacob Ruhe, one of these six-digitated children, espoused, at Dantzic in 1733, Sophie Louise de Thungen who had no extraordinary trait: he had by her six children; two boys were six-digitated. One of them, Jacob Ernest, had six digits on the left foot and five on the right: he had on the right hand a sixth finger, which was amputated; on the left he had in the place of the sixth digit only a stump.

One sees from this genealogy which I have followed with exactitude, that polydactyly (six-digitism) is transmitted equally by the father and by the mother: one sees that it is altered through the mating with five-digitated persons. Through these repeated matings it must probably disappear (*s'eteindre*); and must be perpetuated through matings in which it is carried in common by both sexes.

Scientists nevertheless may accept Maupertuis's finding without conceding that it falsifies preformationism. Perhaps animalculism is correct. Every offspring is an unfolding of rudimentary structures already present in the sperm. Perhaps aberrations such as polydactyly are not hereditary

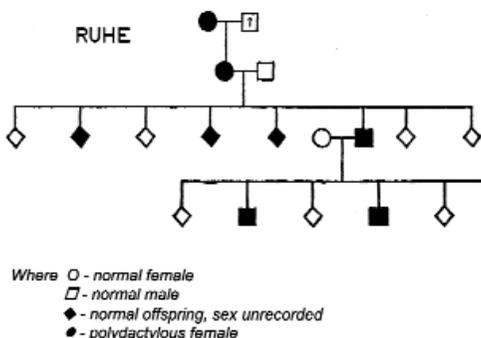


Figure 2. Ruhe Family Genealogy. Elizabeth B. Gasking, *Investigations into Generation, 1651–1828* (London: Hutchinson, 1967), p. 81.

traits at all. Perhaps they are the results of mistakes that occur during embryological development. Perhaps it is just a matter of chance that such mistakes are prevalent in certain families.

Maupertuis himself assessed the probability of such a convergence of chance aberrations and concluded it to be negligible. He declared that

if one wished to regard the continuation of polydactyly as an effect of pure chance, it would be necessary to see what the probability is that this accidental variation in a first parent would be repeated in his descendants. After a search which I have made in a city which has one hundred thousand inhabitants, I have found two men who had this singularity. Let us suppose, which is difficult, that three others have escaped me; and that in 20,000 men one can reckon on one six-digited: the probability that his son or daughter will not be born with polydactyly at all is 20,000 to 1; and that his son and his grandson will not be six-digited at all is $20,000 \times 20,000$ or 400,000,000 to 1: finally the probability that this singularity will not continue during three generations would be 8,000,000,000,000 to 1; a number so great that the certainty of the best demonstrated things of physics does not approach these probabilities.⁷

Maupertuis thus dismissed the appeal to chance. Nevertheless, the documentation of the Ruhe family history, together with the elimination of the chance alternative, still does not falsify preformationism. The preformationist can argue that in cases of polydactyly some yet-undiscovered factor (diet, water, temperature . . .) interferes with the normal course of embryological development. On this defense, offspring are an unfolding of structures present in the sperm (or egg), but occasionally (unspecified) external factors alter this unfolding. All that is falsified by the Ruhe family genealogy is that all polydactylous offspring have a polydactylous male parent (or that all such offspring have a polydactylous female parent).

The falsificationist strategy played a large role in the developments that produced a revolution in chemistry.⁸ In 1774, French chemist Pierre Bayen announced that the product of heating the red calx of mercury is “fixed air” (CO_2). Antoine Lavoisier rejected Bayen’s conclusion. He reasoned that if the decomposition product is fixed air, then this product dissolves in water upon shaking and forms a white precipitate in lime water ($\text{Ca}(\text{OH})_2$). Lavoisier demonstrated that neither effect is observed to take place.

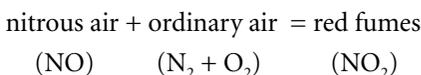
1. Priestley's Nitrous Air Test

1 nitrous air + 1 ordinary air = 1.4 residual
 (NO) .2 O₂ .6 NO
 .8 N₂ .8 N₂
 volume decrease = $\frac{2.0 - 1.4}{2.0} = 30\%$

1 nitrous air + 2 ordinary air = 1.8 residual
 (NO) .4 O₂ .2 O₂
 1.6 N₂ 1.6 N₂
 volume decrease = $\frac{3.0 - 1.8}{3.0} = 40\%$

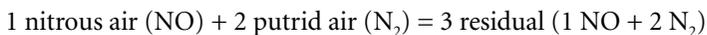
1 nitrous air + 3 ordinary air = 2.5 residual
 (NO) .6 O₂ .1 O₂
 2.4 N₂ 2.4 N₂
 volume decrease = $\frac{4.0 - 2.5}{4.0} = 37.5\%$

Lavoisier was interested at that time in a test Joseph Priestley had developed for the “goodness” of air. Air quality was a pre-occupation of eighteenth-century investigators. It was widely recognized that the quality of air in mines and the holds of ships could deteriorate to life-threatening levels. Priestley’s test showed promise as an indicator of air quality. It was based on a reaction between nitrous air and ordinary air, a reaction that produces red fumes that are soluble in water:



If nitrous air is added to ordinary air over water, the red fumes produced dissolve, leaving a residual gas volume that is less than the initial volume of nitrous air plus ordinary air. Priestley established that a maximum contraction of volume occurs when the initial volume ratio is 1 nitrous air to 2 ordinary air (see table 1 above).

If the air to which a one-half measure of nitrous air is added is not pure, then the volume decrease will be less than 40%. Suppose that before testing, a mouse is allowed to breathe the air, or a candle is burned in that air. The addition of one volume of nitrous air to two volumes of this “diminished air” reduces the initial volume by an amount less than 40%. In the limiting case of “putrid air” in which a mouse has expired or a candle has burned out, there is no decrease in volume:



Lavoisier was impressed by the diagnostic power of Priestley’s nitrous air test. It occurred to him that he could falsify Bayen’s fixed air hypothesis by adding nitrous air to the gas produced by heating the red calx of mercury. He performed the test. The observed volume decrease was slightly greater than the 40% obtained with common air.

The theoretical volume decrease is 50%:

$$\begin{aligned} 1 \text{ nitrous air (NO)} + 2 \text{ gaseous product (O}_2\text{)} &= 1.5 \text{ residual (O}_2\text{)} \\ &\text{of the decomposition} \\ &\text{of the red calx of} \\ &\text{mercury} \\ \text{volume decrease} &= \frac{3.0-1.5}{3.0} = 50\%. \end{aligned}$$

Had the observed decrease been less than 40%, Lavoisier surely would have investigated further. But it was easy for Lavoisier to attribute the greater than 40% volume to experimental error, since he had no reason to believe that there exists a “super-good air.” Lavoisier reported to the French Academy of Science in 1775 that the gaseous product of the decomposition of the red calx of mercury is common air.

Lavoisier’s falsification of Bayen’s fixed air hypothesis was successful despite the fact that he was wrong about the nature of the residual gas. His common air hypothesis quickly met the same fate as Bayen’s hypothesis. Priestley read Lavoisier’s report and promptly applied the falsification strategy to the common air hypothesis.

Priestley argued that if the residual gas from the nitrous air test is common air then the addition of more nitrous air does not reduce the percentage volume decrease. As indicated in Table 1, the maximum volume decrease—40%—is achieved at a ratio of one volume of nitrous air to two volumes of common air. At a one-to-one ratio the volume decrease is only 30%. Priestley showed that when nitrous air is added to the gas produced from the decomposition of the red calx of mercury there is a progressive volume decrease up to a ratio of two volumes of nitrous air to one volume of the gas. At the two-to-one ratio, no gas remains after solution of the red fumes.

$$\begin{aligned} 2 \text{ nitrous air (NO)} + 1 \text{ gas from the} &= 2 \text{ soluble red fumes (NO}_2\text{)} \\ &\text{decomposition} \\ &\text{of the red calx} \\ &\text{of mercury} \\ &\text{(O}_2\text{)} \end{aligned}$$

Priestley concluded that the gas produced from the decomposition of the red calx of mercury is not common air. He provided additional support for this conclusion by showing that the mystery gas is superior to common air in supporting combustion and the respiration of a mouse.

PRIESTLEY ON THE "GOODNESS" OF AIR

On the 8th of this month I procured a mouse, and put it into a glass vessel, containing two ounce-measures of the air from *mercurius calcinatus*. Had it been common air, a full-grown mouse, as this was, would have lived about a quarter of an hour. In this air however, my mouse lived a full half hour; and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled; for, upon being held to the fire, it presently revived, and appeared not to have received any harm from the experiment.

By this I was confirmed in my conclusion, that the air extracted from *mercurius calcinatus*, &c. was, at least, as good as common air; but I did not certainly conclude that it was any better; because, though one mouse would live only a quarter of an hour in a given quantity of air, I knew it was not impossible but that another mouse might have lived in it half an hour; so little accuracy is there in this method of ascertaining the goodness of air: and indeed I have never had recourse to it for my own satisfaction, since the discovery of that most ready, accurate, and elegant test that nitrous air furnishes. But in this case I had a view to publishing the most generally satisfactory account of my experiments that the nature of the thing would admit of.

This experiment with the mouse, when I had reflected upon it some time, gave me so much suspicion that the air into which I had put it was better than common air, that I was induced, the day after, to apply the test of nitrous air to a small part of that very quantity of air which the mouse had breathed so long; so that, had it been common air, I was satisfied it must have been very nearly, if not altogether, as noxious as possible, so as not to be affected by nitrous air; when, to my surprize again, I found that though it had been breathed so long, it was still better than common air. For after mixing it with nitrous air, in the usual proportion of two to one, it was diminished in proportion of $4\frac{1}{2}$ to $3\frac{1}{2}$; that is, the nitrous air had made it two ninths less than before, and this in a very short space of time; whereas I had never found that in the longest time, any common air was reduced more than one fifth of its bulk by any proportion of nitrous air, nor more than one fourth by any phlogistic process whatever. Thinking of this extraordinary fact upon my pillow, the next morning I put another measure of nitrous air

to the same mixture, and, to my utter astonishment, found that it was farther diminished to almost one half of its original quantity. I then put a third measure to it; but this did not diminish it any farther: but, however, left it one measure less than it was even after the mouse had been taken out of it.

Being now fully satisfied that this air, even after the mouse had breathed it half an hour, was much better than common air; and having a quantity of it still left, sufficient for the experiment, viz. an ounce-measure and a half, I put the mouse into it; when I observed that it seemed to feel no shock upon being put into it, evident signs of which would have been visible, if the air had not been very wholesome; but that it remained perfectly at its ease another full half hour, when I took it out quite lively and vigorous. Measuring the air the next day, I found it to be reduced from $1\frac{1}{2}$ to $\frac{2}{3}$ of an ounce-measure. And after this, if I remember well (for in my register of the day I only find it noted, that it was considerably diminished by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouse having been taken out quite vigorous, that the air could not have been rendered very noxious.

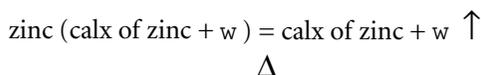
For my farther satisfaction I procured another mouse, and putting it into less than two ounce-measures of air extracted from *mercurius calcinatus* and air from red precipitate (which, having found them to be of the same quality, I had mixed together) it lived three quarters of an hour. But not having had the precaution to set the vessel in a warm place, I suspect that the mouse died of cold. However, as it had lived three times as long as it could probably have lived in the same quantity of common air, and I did not expect much accuracy from this kind of test, I did not think it necessary to make any more experiments with mice.

Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity with as much accuracy as I could, by the test of nitrous air; and I began with putting one measure of nitrous air to two measures of this air, as if I had been examining common air; and now I observed that the diminution was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two thirds of its original quantity, and a third measure to one half. Suspecting that the diminution could not proceed much farther, I then added only half a measure of nitrous air, by which it was diminished still more; but not much, and another half measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures of nitrous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.

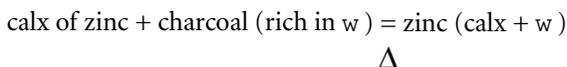
At the same time, air from the red *precipitate* was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now as common air takes about one half of its bulk of nitrous air, before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half-measures before it ceased to be diminished by more nitrous air, and even five half-measures made no addition to its original dimensions, I conclude that it was between four and five times as good common air. It will be seen that I have since procured air better than this, even between five and six times as good as the best common air that I have ever met with.

Being now fully satisfied with respect to the nature of this new species of air, viz. that, being capable of taking more phlogiston from nitrous air, it therefore originally contains less of this principle; my next inquiry was, by what means it comes to be so pure, or philosophically speaking, to be so much *dephlogisticated*.⁹

Priestley's falsification of Lavoisier's common air hypothesis is successful even though his theory about the mystery gas is incorrect. Priestley was committed to the phlogiston theory. According to this theory, the burning of metals releases an invisible gaseous substance called phlogiston:



On the phlogiston theory, the reduction of a calx requires the presence of phlogiston:



However, the red calx of mercury yields the metal directly upon heating without the presence of charcoal or wood. On the phlogiston theory, the metal mercury is a combination of its calx plus phlogiston.

It seemed to Priestley that the required phlogiston could have come only from the air. Hence he interpreted the gaseous product of the mercury-calx decomposition to be air from which phlogiston had been removed, or "dephlogisticated" air:

investigate the scattering process to see if any particles are deflected through large angles. Rutherford recalled that he was surprised that Marsden's data revealed an occasional large deflection. Indeed, one particle in 20,000 is scattered by thin gold foil through an angle greater than ninety degrees.

Rutherford recognized that this result falsified Thomson's model, because that model implies that there should be only small deflections caused by electrostatic repulsion between α -particles and the positively charged "pudding" of the atoms. (An embedded electron is roughly 7,000 times lighter than an α -particle and would not appreciably deflect the particle by electrostatic attraction.) In response to the experimental findings, Rutherford formulated a "nuclear model" of the atom, in which a tiny dense positively charged nucleus is surrounded by electrons.¹⁰

RUTHERFORD ON ALPHA-PARTICLE SCATTERING

In the early days I had observed the scattering of α -particles, and Dr. Geiger in my laboratory had examined it in detail. He found, in thin pieces of heavy metal, that the scattering was usually small, of the order of one degree. One day Geiger came to me and said, "Don't you think that young Marsden, whom I am training in radioactive methods, ought to begin a small research?" Now I had thought that too, so I said, "Why not let him see if any α -particles can be scattered through a large angle?" I may tell you in confidence that I did not believe that they would be, since we knew that the α -particle was a very fast massive particle, with a great deal of energy, and you could show that if the scattering was due to the accumulated effect of a number of small scatterings the chance of an α -particle's being scattered backwards was very small. Then I remember two or three days later Geiger coming to me in great excitement and saying, "We have been able to get some of the α -particles coming backwards. . . ." It was quite the most incredible event that has ever happened to me in my life. It was almost as incredible as if you fired a 15-inch shell at a piece of tissue paper and it came back and hit you. On consideration I realized that this scattering backwards must be the result of a single collision, and when I made calculations I saw that it was impossible to get anything of that order of magnitude unless you took a system in which the greater part of the mass of the atom was concentrated in a minute nucleus. It was then that I had the idea of an atom with a minute massive centre carrying a charge. I worked out mathematically what laws the scattering should obey, and I found that the number of particles scattered

through a given angle should be proportional to the thickness of the scattering foil, the square of the nuclear charge, and inversely proportional to the fourth power of the velocity. These deductions were later verified by Geiger and Marsden in a series of beautiful experiments.

Now let us consider what deductions could be made at that stage. By considering how close to the nucleus the α -particles could go, and yet be scattered normally, I could show that the size of the nucleus must be very small. I also estimated the magnitude of the charge and made it about a hundred times as great as the electronic charge e . It was not possible to make an accurate estimate, but general evidence indicated that the nucleus of hydrogen must have a charge e , helium $2e$, and so on. Geiger and Marsden examined the scattering in different elements and found that the amount of scattering varied as the square of the atomic weight. This result was rough but quite sufficient: it indicated that the charge on a nucleus was roughly proportional to the atomic weight.

Hales, Rumford, Priestley, and Marsden achieved falsification by first noting that hypothesis H materially implies consequent Q and then performing an experiment to show that Q is not the case. This is the usual sequence. It conforms to the “man proposes, nature disposes” image of scientific inquiry.

However, there is nothing about the logic of falsification that dictates that $(H \supset Q)$ be formulated first and $\sim Q$ be determined subsequently. In some cases in which scientists achieve falsification, $\sim Q$ is known first and the material implication $(H \supset Q)$ is established only subsequently. An important example is Isaac Newton’s argument against Descartes’ vortex theory of the solar system.

Newton and his rivals antecedently accepted the truth of Kepler’s laws. The third law states that the period (T) of a planet is proportional to the three halves power of its mean distance from the sun. For any two planets the following proportionality holds:

$$(T_1/T_2) = (D_1/D_2)^{3/2}$$

Newton argued that if the planets are carried in stable orbits around the sun by an invisible ethereal whirlpool, then the density of the vortex at each planet’s distance from the center is equal to that of the planet itself. If

the vortex fluid at a planet's location were more dense than the planet then the planet would spiral in toward the center of the vortex. And if the vortex fluid at a planet's location were less dense than the planet then the planet would recede from the center. Newton thought it implausible that an invisible vortex of such high (and varying) density exists. But he was willing to concede to the Cartesians that there might be such a vortex because he could show that the existence of such a vortex is inconsistent with Kepler's third law. Newton established that a planet could remain in a stable orbit in a Cartesian vortex only if its period is proportional to the square of its distance from the center. Since this result contradicts Kepler's third law, Descartes' vortex hypothesis is false.¹¹

NEWTON ON THE VORTEX THEORY

I have endeavored in this Proposition, to investigate the properties of vortices, that I might find whether the celestial phenomena can be explained by them; for the phenomenon is this, that the periodic times of the planets revolving about Jupiter are as the $3/2$ th power of their distances from Jupiter's centre; and the same rule obtains also among the planets that revolve about the sun. And these rules obtain also with the greatest accuracy, as far as has been yet discovered by astronomical observation. Therefore if those planets are carried round in vortices revolving about Jupiter and the sun, the vortices must revolve according to that law. But here we found the periodic times of the parts of the vortex to be as the square of the distances from the centre of motion; and this ratio cannot be diminished and reduced to the $3/2$ th power, unless either the matter of the vortex be more fluid the farther it is from the centre, or the resistance arising from the want of lubricity in the parts of the fluid should, as the velocity with which the parts of the fluid are separated goes on increasing, be augmented with it in a greater ratio than that in which the velocity increases. But neither of these suppositions seem reasonable. The more gross and less fluid parts will tend to the circumference, unless they are heavy towards the centre. And though, for the sake of demonstration, I proposed, at the beginning of this Section, an Hypothesis that the resistance is proportional to the velocity, nevertheless, it is in truth probably that the resistance is in a less ratio than that of the velocity; which granted, the periodic times of the parts of the vortex will be in a greater ratio than the square of the distances from its centre. If, as some think, the vortices move more swiftly near the centre, then slower to a certain limit, then again swifter near the circum-

ference, certainly neither the $3/2$ th power, nor any other certain and determinate power, can obtain in them. Let philosophers then see how that phenomenon of the $3/2$ th power can be accounted for by vortices.

Proposition LIII, Theorem XLI

Bodies carried about in a vortex, and returning in the same orbit, are of the same density with the vortex, and are moved according to the same law with the parts of the vortex, as to velocity and direction of motion.

For if any small part of the vortex, whose particles or physical points continue a given situation among themselves, be supposed to be congealed, this particle will move according to the same law as before, since no change is made either in its density, inertia, or figure. And again; if a congealed or solid part of the vortex be of the same density with the rest of the vortex, and be resolved into a fluid, this will move according to the same law as before, except so far as its particles, now become fluid, may be moved among themselves. Neglect, therefore, the motion of the particles among themselves as not at all concerning the progressive motion of the whole, and the motion of the whole will be the same as before. But this motion will be the same with the motion of other parts of the vortex at equal distances from the centre; because the solid, now resolved into a fluid, is become exactly like the other parts of the vortex. Therefore, a solid, if it be of the same density with the matter of the vortex, will move with the same motion as the parts thereof, being relatively at rest in the matter that surrounds it. If it be more dense, it will endeavor more than before to recede from the centre; and therefore overcoming that force of the vortex, by which, being, as it were, kept in equilibrium, it was retained in its orbit, it will recede from the centre, and in its revolution describe a spiral, returning no longer into the same orbit. And, by the same argument, if it be more rare, it will approach to the centre. Therefore it can never continually go round in the same orbit, unless it be of the same density with the fluid. But we have shown in that case that it would revolve according to the same law with those parts of the fluid that are at the same or equal distances from the centre of the vortex.

Vortex theorists such as Leibniz and De Molières conceded that Newton's argument was decisive against the single sun-centered vortex postulated by Descartes. They maintained, however, that theories about the interaction of multiple vortices can be developed that are consistent with Kepler's laws.¹²