A System of Logic
Ratiocinative and Inductive
Presenting a Connected View of the Principles of Evidence and the Methods of Scientific Investigation

John Stuart Mill

Contents

Glossary

circumstances: In Mill's usage, the 'circumstances' of a given experiment are all the details of what is the case when the experiment is performed—not only in the environment but also in the experiment itself.

coextensive: 'Law L is coextensive with field F' means not merely that nothing in F is a counter-example to L but that everything in F is an example of L.

coincidence: In Mill's usage, the coincidence of two events is simply their occurring at the same time (and usually, perhaps, in the same place). What you and I call a 'coincidence' is the occurring together of two events that have no causal relation to one another; in Mill's terminology that is a 'casual coincidence'. (**Be alert to the difference between 'casual' and 'causal', both of which occur often in this work.**) On page 328 he introduces a different sense of 'coincidence', which he explains there.

collocation: Arrangement in space; structure. When in the footnote on page 167 Mill explains that by 'the constitution of things' he means 'ultimate laws of nature' and not 'collocations', what he is rejecting is the use of 'constitution' to mean 'how things are arranged, structured, in space'. On page 231 we learn that items entering into a 'collocation' can include powers = forces as well as physical things.

concomitant: 'Of a quality, circumstance, etc.: occurring along with something else, accompanying' (OED).

concurrence: The concurrence of several events is their occurring together, usually meaning at the same time and in roughly the same place. From Latin meaning 'run together'.

connote: To say that word W connotes attribute A is to say that the meaning of W is such that it can't apply to anything that doesn't have A. For example, 'man' connotes humanity.

corpuscle: An extremely small bit of matter—far too small for us to be able to pick it out visually. Adjective **corpuscular**.

cultivation: Carefully developing (a skill or habit), analogous to cultivating roses or cabbages. On page 178 the two are linked metaphorically.

data: Until about the middle of the 20th century 'data' was the plural of 'datum'. Since then it has become a singular mass term, like 'soup'.

deus ex machina: Latin literally meaning 'a god out of a machine', referring to the use of theatrical machinery to float a god onto the stage to make everything come right at a crucial point in a drama. Nearly always the phrase is used metaphorically, to refer to some problem-'solving' item that a theorist introduces in a suspiciously convenient way and without good reasons. On page 179 Mill uses the phrase in both ways at once: the suspiciously convenient item that he refers to is literally God.

efficient cause: This is an Aristotelian technical term. The •formal cause of a coin is its design, the plan according to which it was made; its •material cause is the stuff it is made of; its •final cause is its purpose, namely to be used in commerce; and its •**efficient cause** is the action of the die in stamping the coin out of a metal sheet. So the efficient cause is what you and I would call, simply, 'the cause'. But on page 162 and thereafter Mill is clearly using 'efficient cause' to mean something like: real, metaphysically deep, empirically inaccessible causes, as distinct from the mere

orderly event-followings that are the only causes Mill believes in.

fact: In Mill's usage a 'fact' can be a state of affairs or an event or a proposition (not necessarily true) asserting the existence of a state of affairs or event. In the present version, no attempt is made to sort all this out.

inductio per enumerationem simplicem: Latin meaning 'induction by simple enumeration'. This comes from Bacon, who used it meaning something like 'reaching a generalisation by simply looking at positive instances and naively failing to look for counter-instances or complications'. Mill seems to be using it that way too.

irritability: Proneness to respond to physical stimuli.

luminiferous ether: The ether was a supposed finely divided or gaseous matter pervading the whole universe; 'luminiferous' means 'light-bearing': it was thought that light consisted of some kind of disturbance of the ether.

material: The 'material circumstances' are the circumstances or details that matter. A 'material change' is a change that makes a significant difference.

mutatis mutandis: A Latin phrase that is still in current use. It means '(*mutatis*) with changes made (*mutandis*) in the things that need to be changed'.

natural theology: This is theology based on facts about the natural world, e.g. empirical evidence about what the 'purposes' are of parts of organisms etc. In this context, 'natural' is the antonym of 'revealed'.

numeral: A name of a number, usually confined to names like '7' and not like 'seven'. Mill doesn't use the word here, but this version uses it instead of 'name' in some contexts where the topic is obviously names of numbers.

occult: It means 'hidden', but in the early modern period it always carried the extra sense of 'mysterious, out of reach of ordinary understanding' or the like. The statement that gravity is an 'occult force' meant that the ultimate truth about gravity, whatever it is, won't be a part of ordinary physics.

original: Sometimes Mill uses this to mean 'basic' or 'foundational'. An 'original natural agent' (page 170) is a natural cause that wasn't caused by anything we know about. Mill also uses 'primeval' and 'primitive' with the same meaning.

patient: The same Latin words lie behind three contrasts:
- adjectives: 'active' and 'passive'
- abstract nouns: 'action' and 'passion'
- concrete nouns: 'agent' and 'patient'

We don't now use 'passion' to refer to any undergoing or being-acted-on, or 'patient' to refer to anything that is acted on; but until the end of the 19th century both of those uses were current.

petitio principii: A Latin phrase referring to the procedure of offering a 'proof' of P from premises that include P. The English name for this used to be 'begging the question', but that phrase has recently come to mean 'raising the question' ('That begs the question of what he was doing on the roof in the first place.')

popular: It means 'of the people'; in early modern times it usually doesn't mean 'liked by the people'.

precession of the equinoxes: The slow, steady change in the earth's axis of rotation.

principle: In the passage by Whewell on page 145, the phrase 'principle of connection' may mean 'something that *physically connects* them', thus using 'principle' in a sense— now obsolete but extremely common in the early modern

period—in which it means 'source', 'cause', 'driver', 'energizer', or the like. It is certainly used in that sense by Mill on page 182 and page 188 and by Powell on page 289.

putrefy: rot; and the rotten state is **putrefaction**.

quadrature of the cycloid: A cycloid is the curve traced by a point on the rim of a circular wheel rolling on a plane surface. That curve and the line of the surface enclose an area; its quadrature is the process of discovering the size of that area.

resolve: To resolve x into y and z is to analyse x in terms of y and z, to show that all there is to x is y and z, or the like. Mill explains this on page 231. The noun is **resolution**.

rigor mortis: Latin for 'stiffness (or rigidity) of death'. Mill calls it 'cadaveric rigidity', but these days the Latin phrase is also the colloquial English one.

sagacity: Here it means something like 'alert intelligence'.

sui generis: Latin for 'of its own kind'—not significantly like anything else.

synchronous: Occurring at the same time.

type: 'the real type of scientific induction' (page 158) means 'the central defining paradigm of scientific induction'. Similarly with 'the type of uncertainty and caprice' on page 293 and 'the type of a deductive science' on page 316.

vera causa: Latin meaning 'true cause'. A technical term of Newton's. To say that x is a *vera causa* of y is to say that x is already known about independently of its causing of y, or perhaps (see page 247) that x *could be* known about independently etc.

virtue: power, causal capacity, or the like.

vortex: Descartes's term for a rapidly rotating collection of fine particles. The plural is **vortices**.

Book III; Induction

Chapter 1. Preliminary remarks about induction in general

§1. We are now approaching what can be regarded as the principal topic in this work—because it is more intricate than any of the others, and because it concerns a process that I have shown in Book II to be the one that the investigation of nature essentially consists in. I showed that all inference, and consequently all proof and all discovery of truths that aren't self-evident, consists of **inductions and the interpretation of inductions**—i.e. that all our knowledge that isn't intuitive comes from that source. So it has to be accepted that the main question of the science of logic—the question that includes all others—is

> What is induction? and what conditions make it legitimate?

Yet professed writers on logic have almost entirely ignored this question. Metaphysicians haven't altogether neglected its broad outlines. But they haven't known enough about the processes by which science has actually succeeded in establishing general truths, so that their analysis of the inductive operation, even when perfectly correct, hasn't been specific enough to be made the foundation of practical rules that could serve •induction itself in the way the rules of the syllogism serve •the interpretation of induction. As for those who have brought physical science to its present state of improvement, never until very recently have they tried seriously to philosophise on the subject; they haven't regarded their way of arriving at their conclusions—-as distinct from the conclusions themselves—as worth studying. ·It's a pity, because· all they needed to do to get a complete theory of the process was to focus on the methods that *they* had been using, and to generalise these and adapt them to all sorts of problems,

§2. For the purposes of the present inquiry, **induction** can be defined as **the operation of discovering and proving general propositions**. As I have already shown, the process of indirectly ascertaining individual facts. . . .is a form of the very same process, because **(a)** general facts are merely collections of particular facts, definite in kind but indefinite in number; and **(b)** whenever the empirical evidence justifies us in drawing a conclusion about even one unknown case, it would also justify us in drawing a similar conclusion regarding a whole class of cases. The inference either •doesn't hold at all or •holds in all cases of a certain description—all cases which, in certain definable respects, resemble those we have observed.

If I'm right in maintaining that the principles and rules of inference are the same whether we are inferring general propositions or individual facts, then a complete logic of •the sciences would also be a complete logic of •practical affairs and common life. An analysis of the process by which general truths are arrived at is virtually an analysis of all induction whatever. Why? Because in *any* legitimate inference from experience, the conclusion could legitimately be a general proposition. Whether we're inquiring into a scientific principle or an individual fact, and whether we proceed by experiment or by ratiocination, every link in the chain of inferences is essentially inductive, and the legitimacy of the induction depends in both cases on the same conditions.

When a practical inquirer (e.g. an advocate or judge) is trying to ascertain facts for the purposes not of science but of practical affairs, the principles of induction won't help him with his chief difficulty. It lies not in making his inductions, but in the selection of them—choosing from among all general propositions ascertained to be true the ones that provide marks by which he can trace whether the given subject of study does or doesn't have the predicate in question. When an advocate is arguing a doubtful question of fact before a jury, the general propositions he appeals to are mostly in themselves pretty trite, and are assented to as soon as stated; his skill lies in bringing his case under those propositions; in calling to mind any known or accepted maxims of probability that can be applied to the case in hand, and selecting from among them those that are most favourable to the case he is trying to make. His success will depend on his natural or acquired sagacity [see Glossary], aided by his knowledge of the particular subject and of subjects allied with it. Invention, though it can be cultivated [see Glossary], can't be reduced to rule; there's no science to enable a man to bring to mind what he needs when he needs it.

But when he *has* thought of something x, science can tell him whether x will suit his purpose or not. When the inquirer or arguer is selecting the inductions out of which he will construct his argument, his only guide is his own knowledge and sagacity. But the validity of the argument he constructs depends on principles, and must be subjected to tests that are the same for all kinds of inquiries—whether the result is •to give someone an estate or •to enrich science with a new general truth. Either way,

(1) The individual facts must be decided on the basis of the senses, or testimony;

(2) The rules of the syllogism will determine whether the case really falls within the formulae of the inductions under which it has been successively brought; and finally

(3) The legitimacy of the inductions themselves must be decided by other rules. . .

. . . and these rules are what I intend now to investigate. In many everyday practical contexts this third part of the operation is its least difficult part; but we've seen that this is also the case in some big scientific fields. I'm referring to the sciences that are principally deductive, especially mathematics, where •the inductions are few in number and so obvious and elementary that they seem not to need any backing from experience, whereas •combining them so as to prove a given theorem or solve a problem may require the utmost powers of invention and contrivance that our species is gifted with.

If you want further confirmation of my claim that the logical processes that •prove particular facts are the very ones that •establish general scientific truths, consider this: In many branches of science there's a need to prove single facts; they're as completely individual as any that are debated in a court of justice, but are proved in the same way as the other truths of the science—without lessening in the slightest the homogeneity of its method. Astronomy provides remarkable example of this. Most of the individual facts on which that science bases its most important deductions—

•the sizes of the bodies of the solar system,
•their distances from one another,
•the shape of the earth, and
•the earth's rotation

—can't be established by direct observation; they are proved indirectly, using inductions based on other facts that we can more easily reach. [Mill cites the example of the discovery of the moon's distance from the earth; two direct observations

(of the moon's relation to two widely separated points on the earth's surface), followed by sheer trigonometry.]

The process by which that individual astronomical fact was ascertained is exactly like those by which astronomy establishes its general truths; and indeed (as I have shown for all legitimate reasoning) a general proposition could have been concluded instead of a single fact. Strictly speaking, indeed, the result of the reasoning *is* a general proposition; it's a theorem about the distance from the earth of *any* inaccessible object, showing how that distance relates to certain other quantities. The moon is almost the only body whose distance from the earth can really be ascertained in this way, but that's a mere upshot of facts about the other heavenly bodies that make them incapable of providing such data as the application of the theorem requires. The theorem itself is as true of them as it is of the moon. [Mill has here a footnote responding to criticisms by Whewell of Mill's use of 'induction'. He says that Whewell's preferred sense of the word isn't justifiable by any philosophical arguments] or supported by usage, at least from the time of Reid and Stewart, who are the principal legislators (as far as the English language is concerned) of modern metaphysical terminology.

So we shan't fall into error if in treating of induction we limit our attention to the establishment of general propositions. The principles and rules of •induction as directed to this end are the principles and rules of •all induction; and the logic of science is the universal logic, applicable to all inquiries in which man can engage.

Chapter 2. Inductions improperly so called

§1. Induction, then, is the mental operation by which we infer that what we know to be true in a particular case or cases will be true in all cases that resemble the former in certain assignable respects. In other words, induction is the process by which we conclude that what is true of certain individuals in a class is true of the whole class, or that what is true at certain times will be true in similar circumstances at all times.

This definition excludes from the meaning of 'induction' various logical operations that are quite often regarded as examples of 'induction'.

Induction, as I have defined it, is a process of *inference* from the known to the unknown; so it excludes any process in which the apparent conclusion is no wider than the premises it is drawn from. Yet the common books of logic present something of this latter kind as the most perfect—indeed the *only* entirely perfect—form of induction! In those books, every process that sets out from a less general and terminates in a more general expression—which admits of being stated in the form 'This and that A are B, therefore every A is B'—is called an induction, whether or not anything is really concluded in it. And the induction is said not to be perfect unless every single individual of the class A is included in the premise, I.e. unless what we affirm of the class has already been ascertained to be true of every individual in it, so that the supposed conclusion is really a mere re-assertion of the premises. If we say 'All the planets shine by the sun's light' because we have observed that

Mercury, Venus, etc. shine by the sun's light; or that 'All the Apostles were Jews' because we know this regarding Peter, Paul, John, and every other apostle—these and their like are called perfect (and the only perfect) inductions. But this is totally different in kind from my kind of induction; it's not an inference from known facts to unknown facts, but a mere short-hand record of known facts. Their 'conclusions' are not really general propositions. In a general proposition the predicate is affirmed or denied of an unlimited number of individuals, namely all that have the properties connoted by the subject of the proposition—*all*, existing or possible, whether few or many. 'All men are mortal' doesn't mean •all now living but •all men past, present and future. When the word's signification is limited so as to make it a name only for each of a number n of individuals, designated as such and (as it were) counted off individually, the proposition, despite its general language ('·All the planets·...', '·All the Apostles·...') is not a general proposition but merely n singular propositions, written in an abridged form. The operation may be useful, as most forms of abridged notation are; but it's not a part of the investigation of truth, though it often has an important role in preparing the materials for that investigation.

Just as we can sum up n singular propositions in one proposition that will be apparently—but not really—general, so also we can sum up n general propositions in one proposition that will be apparently—but not really—more general. Suppose that for each distinct species of animals we establish by induction that every animal of that species has a nervous system, and on that basis assert that *all ·species of· animals have a nervous system*. This looks like a generalisation, but in fact it merely affirms of •all what has already been affirmed of •each, so it tells us nothing that we didn't already know. This 'conclusion' means the same as 'All *known* ·species of· animals have a nervous system'. Don't confuse this case with the following quite distinct one. Our observations of the various species of animals have revealed to us a law of animal nature, putting us in a condition to say that a nervous system will be found even in ·species of· animals that haven't yet been discovered. This is indeed an induction, in which the conclusion is a general proposition containing more than the sum of the special propositions from which it is inferred. The difference between these two is further marked by the fact that the latter of them—the genuine induction—could be legitimate even if we hadn't examined every single known species of animals.... Returning to the earlier example, think about the difference between these;

>'All *the* planets shine by reflected light.'
>'All *planets* shine by reflected light.'

The latter is an induction; the former is not....

§2. Several mathematical processes should be distinguished from induction, because they are often (wrongly) called by that name, and share something important with genuine inductions, namely leading to conclusions that really are general propositions. For example, when we have proved that a straight line can't meet a •circle at more than two points, and then successively prove the same thing of the •ellipse, the •parabola, and the •hyperbola, we can lay it down as a universal property of *all conic sections*. The distinction drawn in the two previous examples has no place here because there's no difference between 'all *known* conic sections' and 'all conic sections', as a cone demonstrably can't be intersected by a plane except in one of these •four lines. So we can hardly deny that the proposition arrived at is a generalisation, because there's no room for any generalisation beyond it. But there's no induction because

there's no inference; the conclusion is a mere summing up of the content of the four propositions from which it is drawn. The proof of a geometrical theorem by means of a diagram (on paper or in the imagination) is a bit like that though not entirely so. As I said earlier, such a demonstration doesn't directly prove the general theorem; all it proves is that the general conclusion asserted in the theorem is true of the particular triangle or circle exhibited in the diagram. But we can see that we could prove it of *any* circle in the same way that we have proved it of that one; so we gather up all the singular propositions that could be thus proved, and embody them in a universal proposition. Having shown that the three angles of the triangle ABC are together equal to two right angles, we conclude that this is true of every other triangle, not •because it is true of ABC but •for the same reason that proved it to be true of ABC. The term 'induction' isn't really right for this, because although its conclusion is really general it isn't believed on the evidence of particular instances. We don't conclude that all triangles have that property because some triangles have; rather, we accept the conclusion on the evidence that was the basis for our conviction in the particular instances.

In some mathematical arguments—so-called 'inductions'—the conclusion does look like a generalisation based on some of the particular cases covered by it. When a mathematician has calculated a sufficient number of the terms of an algebraic or arithmetical series to have ascertained what is called the *law* of the series, he doesn't hesitate to supply any number of the succeeding terms without repeating the calculations. But I take it that he does this only when it is apparent from *a priori* considerations (which could be exhibited in the form of demonstration) that the way each subsequent term is formed from its immediate predecessor is the same as the way each previous term was formed from

its predecessor. There are instances on record of wrong results' being reached when a series was continued without the backing of such general considerations.

Newton is said to have discovered the binomial theorem by induction, specifically calculating that
$$(a + b)^2 = a^2 + 2ab + b^2$$
and that
$$(a + b)^3 = a^3 + 3a^2b + 3ab^2 + b^3$$
and so on, and comparing all those results until he detected the general relation that the general binomial theorem expresses concerning the general form $(a + b)^n$ for all values of n. It's likely enough that he did; but a mathematician like Newton, who seemed to *leap* to principles and conclusions that ordinary mathematicians reached only by a succession of steps, certainly couldn't have performed the comparison in question without being led by it to the *a priori* ground of the law; since anyone who understands multiplication well enough to venture on multiplying several lines of symbols at one operation can't help seeing that in raising a binomial to a power the coefficients must depend on the laws of permutation and combination; and as soon as that is recognised the theorem is demonstrated. Indeed, once it was seen that the law prevailed in a few of the lower powers, its identity with the law of permutation would at once suggest the considerations that prove it to hold universally. So even cases like this are only examples of what I have called 'induction by parity of reasoning', i.e. not really induction because it doesn't involve inference of a general proposition from particular instances.

§3. It is really important to clear up a third improper use of the term 'induction', because •the theory of induction has been greatly confused by it, and •the confusion shows up in the most recent and elaborate treatise on the inductive philosophy that exists in our language. The error in question

is that of failing to distinguish •an induction from a set of observed phenomena from •a mere description of them in general terms.

Take a phenomenon consisting of parts that can only be observed separately, as it were piecemeal. When the observations have been made, there's a convenience (amounting for many purposes to a necessity) in getting a representation of the phenomenon as a whole by piecing these detached fragments together. A navigator sailing the ocean meets land; he can't by any one observation determine whether it's a continent or an island; but he coasts along it, and after a week sees that he has sailed completely round it, and then declares it to be an island. There was no particular time or place of observation at which he could see that this land was entirely surrounded by water; he learned this fact by a succession of partial observations, and then chose a general expression—'·It's an island·'—which summed up in three words the whole of what he observed during that week. Is there anything in the nature of an *induction* in this process? Did he infer something that hadn't been observed from something that had? Certainly not. He had observed the whole of what the proposition asserts. That this land is an island isn't an inference from the partial facts that the navigator saw in the course of his circumnavigation; it is
- •the facts themselves,
- •a summary of those facts,
- •the description of a complex fact to which those simpler ones are as the parts of a whole.

I don't think there is any difference in kind between this simple operation and the one by which Kepler ascertained the nature of the planetary orbits; and Kepler's operation— or anyway all that was characteristic in it—was no more *inductive* than the navigator's.

Kepler aimed to determine the real path followed by each

of the planets. (Let's take *Mars*, because that was the subject of the two of his three laws that didn't require a comparison of planets.) The only way to do this was by direct observation; and all that observation could do was to ascertain many of the successive places—or rather, apparent places—of the planet. The unaided senses could establish this much:
- •The planet successively occupied all these positions, or anyway positions that produced the same impressions on the eye, and
- •It passed from one of these to another insensibly, with no apparent break in the continuity.

What Kepler did beyond this was to find what sort of a curve these different points would make if they were all joined together. He expressed the whole series of the observed places of Mars by the general conception of *an ellipse.* This operation was much harder than that of the navigator who expressed the series of his observations on successive points of the coast by the general conception of *an island.* But it's the very same sort of operation; and if the navigator's operation is not an induction but a description, this must also be true of Kepler's.

The only real induction consisted in inferring that because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse; and in concluding that the positions of the planet between two observations must have coincided with the intermediate points of the curve. These were *inferences from* the observations—facts inferred, not facts seen—·so they involved genuine induction·. But these inferences, far from being a part of what Kepler did, had been conducted long before he was born. Astronomers had long known that the planets periodically returned to the same places. With this established, there was no induction left for Kepler to make; he merely applied his new conception

to the inferred facts as well as to the observed facts. When he found that an ellipse correctly represented the past path, he knew that it would represent the future path; in finding a compendious expression for the one set of facts, he found one for the other. But that's all he found—the •expression only, not the •inference—and this didn't add anything to the power of prediction already possessed.

§4. Whewell has given an apt name, the 'colligation of facts', to the descriptive operation that enables a number of details to be summed up in a single proposition. I fully agree with most of what he says about that mental process, and would gladly transfer all that part of his book into my own pages. But I think he makes one mistake, namely treating this kind of operation as the central, primary kind of induction, presenting the principles of mere colligation as principles of 'induction'. In fact, colligation is not 'induction' at all in the old and accepted meaning of the word.

Whewell maintains that the general proposition that binds together the particular facts and makes them into one fact is not the mere sum of those facts but something more, because it introduce a mental conception that didn't exist in the facts themselves. He writes:

> 'The particular facts are not merely brought together, but a new element is added to them by the very act of thought by which they are combined...When the Greeks, after long observing the motions of the planets, saw that these motions could be considered as produced by the motion of one wheel revolving inside another wheel, these wheels were creations of their minds added to the facts they perceived by sense. Even if the wheels were no longer supposed to be material, but were reduced to mere geometrical spheres or circles, they were still products of the mind

alone—something additional to the facts observed. The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected until the discoverer supplies from his own store a principle [see Glossary] of connection. The pearls are there, but they won't hang together until someone provides the string.'

In this passage Whewell indiscriminately blends together examples of both the processes that I am trying to distinguish. When the Greeks abandoned the supposition that the planetary motions were produced by the turning of material wheels, and fell back on the idea of 'mere geometrical spheres or circles', more was going on than the mere substitution of an ideal curve for a physical one. There was the abandonment of a •theory, and the replacement of it by a mere •description. No-one would call the doctrine of material wheels a mere description! That doctrine was an attempt to identify the force by which the planets were acted on and compelled to move in their orbits. But when the •materiality of the wheels was discarded and only their •geometrical forms retained, this was a great step in philosophy in which the attempt to *account for* the motions was given up and what was left of the theory was a mere *description* of the orbits. The proposition that

> the planets were carried round by wheels revolving inside other wheels

was replaced by the proposition that

> the planets moved in the lines that *would be* followed by bodies carried by wheels within wheels.

This was a mere way of representing the sum of the observed facts; and Kepler's was another (and better) way of representing the same observations.

It's true that for these merely descriptive operations, as well as for the erroneous inductive one, a mental conception

was required. The conception of an ellipse had to present itself to Kepler's mind before he could identify the planetary orbits with it. Whewell sees the conception as something added to the facts. He implies that Kepler put something into the facts by his way of conceiving them. But Kepler did no such thing. The ellipse was in the facts before Kepler recognised it; just as the island was an island before it had been sailed around. Kepler didn't *put* what he had conceived into the facts, but *saw* it in them. A conception implies, and corresponds to, something conceived; and though the conception itself is not in the facts but in our mind, if it is to convey any knowledge about •the facts it must be a conception of something that really is in •them, some property that they actually have and that they would show to our senses if our senses were able to take cognisance of it. Suppose that Mars left behind it a visible track, and that an observer was in a fixed position that let him see the whole plane of the orbit at once; he would see it to be an ellipse. . . . I don't think anyone would deny that in this case identifying the planet's path with an ellipse is *describing* it; and I can't see why it makes any difference that in fact that path of Mars is not directly an object of sense, given that every point in it is as exactly ascertained as if it were so.

. . . .I don't think that the role of conceptions in the operation of studying facts has ever been overlooked or undervalued. No-one ever disputed that in order to reason about something we must have a conception of it; or that when we include a multitude of things under a general expression the expression implies a conception of something common to those things. But it by no means follows that the conception must be pre-existent, or that the mind constructs it out of its own materials. If the facts are rightly classed under the conception, that's because the facts themselves contain something x of which the conception is a copy;

and if we can't directly perceive x, that's because of the limited power of our organs and not because x isn't there. The conception itself is often obtained by abstraction from the very facts which. . . .it is afterward called in to connect. Whewell admits this himself when he observes. . . .what a great service it would be to the science of physiology if a philosopher were to 'establish a precise, tenable, and consistent conception of life'. Such a conception has to be abstracted from the phenomena of life itself—from the very facts that it is required to connect. In other cases, instead of •collecting the conception from the phenomena we are trying to colligate, we •select it from among the conceptions that have already been collected by abstraction from other facts. That's what happened with Kepler's laws. The facts were out of the reach of any observation that could enable the senses to identify the path of the planet directly, so the required conception couldn't be collected by abstraction from the observations themselves; the mind had to supply hypothetically, from among the conceptions it had obtained from other portions of its experience, some one conception that would correctly represent the series of the observed facts. It had to form a supposition regarding the general course of the phenomenon, and ask itself 'If this is the general description, what will the details be?', and then compare these with the details actually observed. If they agreed, the hypothesis would serve as a description of the phenomenon; if not, it had to be abandoned and another tried. It's this sort of case that gives rise to the ·false· doctrine that the mind in forming the descriptions adds something of its own that it doesn't find in the facts.

Mars *does* follow an ellipse; that is a fact, surely, and one that we could *see* if we had adequate visual organs and a suitable position. Lacking these advantages but possessing the conception of an ellipse. . . ., Kepler looked to see whether

the observed places of the planet were consistent with such a path. He found they were so; which led him to assert as a fact that the planet moves in an ellipse. But this fact, which Kepler didn't add to the motions of the planet but found in them, was the very fact whose separate parts had been separately observed; it was the sum of the different observations.

Having stated this basic difference between my opinion and Whewell's, I must add that his account of how a conception is selected that is suitable to express the facts seems to me absolutely right. The process is tentative: it consists of a series of guesses of which many are rejected until eventually one is found that is fit to be chosen. We know from Kepler himself that before hitting upon the 'conception' of an ellipse he tried nineteen other imaginary paths which he had to reject because they didn't fit the observations. Whewell is right in saying that the successful hypothesis, though it is a guess, is usually not a lucky guess but a skillful one. The guesses that give mental unity and wholeness to a chaos of scattered particulars seldom occur except in minds abounding in knowledge and disciplined in intellectual combinations.

The tentative method is indispensable to the •colligation of facts for purposes of description. How far can it be applied to induction itself? and what functions does it have in that department? I'll discuss this in chapter 14 of this Book. Right now my main task is to distinguish colligation from induction properly so-called; and to make that distinction clearer I'll discuss a curious and interesting remark ·of Whewell's· which is as strikingly true of colligation as it is false of induction, or so it seems to me.

[The 'remark' in question is something Whewell says about the successive accounts of the movements of (for example) the planet Mars—that it moves

•in a circle with the earth as centre;
•in a circle with the earth inside the circle but not at its centre;
•in epicycles, i.e. little circles whose centres move in a circle around the earth;
•in an ellipse.

Whewell says that each of these was correct as far as it went. In Mill's words: 'They all served the purpose of colligation; they all enabled the mind to represent to itself easily and all at once the whole body of facts ·about Mars· that had been established up to then.' Mill also quotes Comte as saying the same thing, and goes on to express his own agreement and then to draw a line:] Whewell's remark, therefore, is philosophically correct. Successive expressions for the colligation of observed facts—i.e. successive **descriptions** of a whole phenomenon that has been observed only in parts—can all be correct as far as they go, although they conflict with one another. But it would surely be absurd to assert this of conflicting **inductions**.

A scientific study of facts may be undertaken purposes of **(a)** the simple description of the facts, **(b)** the explanation of the facts, or **(c)** the prediction of similar facts. Of these, **(a)** is not, while **(b)** and **(c)** are, properly called 'induction'. Whewell's remark is true of **(a)**:. . . . The elliptical theory, as a mere description, was simpler and more easily usable than its predecessors, but it wouldn't really be more true than they were. So different **(a)** descriptions can all be true, but surely not different **(b)** explanations, such as these explanations of the movements of the planets:

(i) They are moved by a 'virtue' [see Glossary] inherent in their celestial nature;
(ii) They are moved by impact (which led to the hypothesis of vortices [see Glossary] as the only pushing force capable of whirling bodies in circles);

(iii) They are moved by the composition of a centripetal force with an original projectile force (Newton).

These are explanations collected by real induction from supposed parallel cases; and each had its time of being accepted. Can it be said of *these*. . . .that they are all true as far as they go? Isn't it clear that at most one of them is true—that only one can be true in any degree, and the other two must be altogether false? Now consider two **(c)** predictions:

- Eclipses will occur when one planet or satellite casts its shadow on another;
- Eclipses will occur when some great calamity is impending over mankind.

Do these two doctrines differ only in expressing real facts with different degrees of accuracy? Assuredly one of them is true and the other absolutely false.

·START OF A LONG FOOTNOTE·

[This footnote reports and responds to two replies that Whewell made to the content of this section up to here. According to Whewell, the three explanations of planetary motion that Mill cites could all be true. His defence of this involves construing each of **(i)** and **(ii)** as being abstract and formal in such a way that **(iii)** can be seen as a factually contentful cashing in of it:]

'If **(i)** had been maintained in such a way as to agree with the facts, the inherent virtue would have had its laws determined; and then it would have been found that the virtue related to the central body; and so, the "inherent virtue" would have coincided in its effect with **(iii)** the Newtonian force and the two explanations would agree—except in regard to the word "inherent". This word indicates a part of theory **(i)** that was found to be untenable, so it was of course rejected in the transition to later and more exact theories.'

[Mill replies:] Whewell says that the theory of an inherent virtue agrees with Newton's when the word 'inherent' is left out, which of course it would be (he says) if 'found to be untenable'. But leave that out and where's the theory? The word 'inherent' *is* the theory. When it is omitted, all that remains is the statement that the heavenly bodies move 'by a virtue'—i.e. by a power of some sort—or by virtue of their celestial nature, which directly contradicts **(iii)** the doctrine that terrestrial bodies fall by the same law.

[Whewell again:] 'The doctrine **(ii)** that the heavenly bodies were moved by vortices was successfully modified so that it came to coincide in its results with the doctrine **(iii)** of an inverse-quadratic centripetal force. . . When this point was reached, the vortex was merely a machinery. . . .for producing such a centripetal force, and therefore didn't contradict the doctrine of a centripetal force. . . .'

[Mill replies:] If the doctrine **(ii)** of vortices had meant not that vortices existed but only that the planets moved as though they were whirled by vortices; if the hypothesis had been merely a way of representing the facts and not an attempt to account for them; if (in short) it had been only a description; no doubt it *would* have been reconcilable with **(iii)** the Newtonian theory. But the vortices were *not* a mere aid to conceiving the motions of the planets, but a supposed physical agent actively pushing them. . . .and according to Newton's theory this was not true. Whewell seems to think of Newton's theory as stating only the •directions of the forces and not •their nature, and therefore as not conflicting with any hypothesis about how they are produced. Well, **(iii)** the Newtonian theory regarded as a mere •description of the planetary motions doesn't conflict with **(ii)**; but the Newtonian theory as an •explanation of them *does*. The explanation consists in

ascribing those motions to a general law that holds between all particles of matter, and identifying this with the law by which bodies fall to the ground.

If the planets are kept in their orbits by a force that draws the particles composing them toward every other particle of matter in the solar system, then they are *not* kept in those orbits by the impulsive force of certain streams of matter that whirl them around. One explanation absolutely excludes the other. . . . Denying this is like saying that there's no contradiction between 'That man died because somebody killed him' and 'That man died a natural death'.

If Whewell is not yet satisfied, any other subject will serve equally well to test his doctrine. He will hardly say that there's no contradiction between the members of each of these pairs:

•Light is a stream of particles
•Light is a series of waves.

•Higher organic forms arose by development from the lower.
•The different organic forms came from separate and successive acts of creation.

•Volcanoes are fed from a central fire.
•Volcanoes come from chemical action at a comparatively small depth below the earth's surface.

If different explanations of the same fact can't both be true, still less can different predictions. Whewell quarrels (never mind why) with my choice of example on this point, and thinks that a theory is sufficiently answered by an objection to an illustration of it. Well, examples not liable to his objection are easily found, if the production of many

examples is really needed to support the thesis that conflicting predictions can't both be true! [Mill then gives some examples.]

Whewell sees no distinction between •holding contradictory opinions on a question of fact, and •merely employing different analogies to help the conception of the same fact. Different inductions belongs to the former class, different descriptions to the latter.

. . . .But induction is *connected* with colligation in two ways. •Induction is always colligation. The assertion that the planets move in ellipses was only a colligation; whereas the assertion that the planets are drawn (or tend) toward the sun was the statement of a new fact, inferred by induction. But it *also* served as a colligation: it brought the facts which Kepler had connected by his conception of an ellipse under the additional conception of bodies acted upon by a central force. . . . •The descriptions reached by colligation are a necessary preparation for induction. . . . Without the previous colligation of detached observations by means of one general conception we could never have obtained any basis for an induction except in the case of very limited phenomena. We couldn't affirm any predicates of a subject that we could observe only piecemeal, let alone extending those predicates by induction to other similar subjects. . . .

§5. . . . .Whewell has replied to all this, re-stating his case but not strengthening it, as far as I can see. Since my arguments have not had the good fortune to make any impression upon him, I will add a few remarks to show more clearly what we are differing about and in some measure to account for the difference.

Nearly all writers of authority define *induction* as drawing inferences from known cases to unknown:

•applying to a class a predicate that has been found true of some members of the class;

•inferring from the fact that things have a certain property that other things resembling them have the same property;

•inferring from the fact that a thing had a property at a certain time that it does and will have that property at other times.

Kepler's operation clearly wasn't an induction in this sense of the term! The statement that Mars moves in an elliptical orbit wasn't any kind of extension from facts to further facts. . . . Kepler didn't extend an observed truth to cases other than those in which it had been observed; he didn't widen the **subject** of the proposition expressing the observed facts. The alteration he made was in the **predicate**. Instead of saying 'The successive places of Mars are so-and-so' he summed them up in the statement 'The successive places of Mars are points in an ellipse'. Whewell says that this statement wasn't the sum of the observations *merely*, and I agree; it was the sum of the observations *seen under a new point of view*. But it wasn't the sum of more than the observations, as a real induction is. It covered only cases that had been actually observed or could have been inferred from the observations before the new point of view presented itself. There was not the transition from known cases to unknown ones that constitutes 'induction' in the original and acknowledged meaning of the word.

Old definitions can't prevail against new knowledge: scientific language ought to adapt itself to the true relations that hold between the things it is used to designate. If the Keplerian operation really is identical—considered as a logical process—with what happens in acknowledged induction, the definition of 'induction' should be widened so as to take it in. This is where I take issue with Whewell. He does think

that the operations are identical. He holds that the only logical process in any induction is one that also occurs in Kepler's case, namely *guessing until a guess is found that squares with the facts*. That leads him to reject all canons of induction, because it's not by means of them that we guess. Whewell's theory of the logic of science would be very perfect if it didn't pass over altogether the question of *proof*. But I think there *is* such a thing as proof, and inductions relate to it quite differently from how descriptions do. Induction is proof; it is inferring something unobserved from something observed; it requires, therefore, an appropriate test of proof; and to provide that test is the special purpose of inductive logic. [That sentence is verbatim from Mill.] When on the other hand we merely collate known observations and (as Whewell puts it) connect them by means of a new conception, if the conception does serve to connect the observations we have all we want. The proposition containing it claims only to have truth that it may share with many other ways of representing the same facts. So all it requires is to be consistent with the facts; it can't be proved and doesn't need to be. It may serve to prove *other* things: it places facts into a mental connection with other facts that hadn't previously been seen to resemble them, and thereby assimilates the case to another class of phenomena concerning which real inductions have already been made. Thus Kepler's so-called 'law' brought the orbit of Mars into the class *ellipse*, thereby proving all the properties of an ellipse to be true of the orbit; but in this proof Kepler's 'law' supplied the minor premise and not (as with real inductions) the major.

[Explaining that last clause: Mill is thinking in terms of syllogisms (dealt with in II.2) of the form:

    (1) All elliptical orbits have the property F.
    (2) Mars's orbit is an elliptical orbit.
Therefore
    (3) Mars's orbit has the property F.

In this syllogisms **(2)**—Kepler's 'law'—is the minor premise (meaning that it contains the subject of the conclusion). If that syllogism expressed a real induction, Mill thinks, it would be the inductive leap from **(1)** to **(3)**.]

Whewell calls something an induction if, and only if, it introduces a new mental conception; but this is running together two very different things, •invention and •proof. Introducing a new conception belongs to invention; this may be required in any operation, but it isn't the essence of any. . . . Most inductions require no conception except what was present in each of the particular instances on which the induction is based. That *all men are mortal* is surely an inductive conclusion, but it doesn't introduce any new conception; if you know that *some man has died* you have all the conceptions involved in the inductive generalisation. Whewell, however, considers the process of invention—i.e. forming a new conception consistent with the facts—to be not merely a necessary •part of all induction but the •whole of it.

The mental operation that extracts from a number of detached observations certain general characters in which the observed phenomena resemble one another, or resemble other known facts, is what Bacon, Locke, and most subsequent metaphysicians have understood by 'abstraction'. I think it is strictly logically correct to call

> a general expression obtained by abstraction, connecting known facts by means of shared characteristics but without inferring further facts from them

a 'description'; and I don't know how else anything could be described! But I don't rely on the use of that particular word; I'm quite content to use Whewell's term 'colligation', or the more general 'mode of representing or of expressing phenomena'; provided it is clearly seen that the process is not induction but something radically different.

[Mill says that he will return to these matters, and remove difficulties that the reader may have had with the present chapter, in Book IV.]

Chapter 3. The ground of induction

§1. Thus, induction *properly so-called* can be briefly defined as **generalisation from experience**. It consists in inferring from some individual instances in which a phenomenon is observed to occur that it occurs in all instances that resemble the former in what are regarded as the material [see Glossary] circumstances.

How are we to tell material [see Glossary] circumstances from ones that are immaterial? Why are some circumstances material and others not so? I'll come to those questions in due course. I must first point out that there's a principle implied in the very statement of what induction is—an assumption about the course of nature and the order of the universe—namely that there are such things in nature as parallel cases; that what happens once will happen again when the circumstances are sufficiently alike, and not only *again* but *as often as the same circumstances recur*. This assumption is involved in every case of induction; and looking at the actual course of nature we see that the assumption is justified. The universe, as far as we know it, is constituted in such a way that whatever is true in any one case is true in all cases of a certain description; the only difficulty is to find what description.

This universal fact, which is our warrant for all inferences from experience, has been stated variously by different philosophers: 'The course of nature is uniform', 'The universe is governed by general laws', and the like. Metaphysicians of the school of Reid and Stewart have popularised one of the most inadequate of these formulations. The human mind's disposition to generalise from experience—a propensity these philosophers regard as an instinct of our nature—they usually describe as something like 'our intuitive conviction that the future will resemble the past'. Now, Bailey rightly said that *time* doesn't come into it. We believe that fire will burn tomorrow because it burned today and yesterday; but we believe on precisely the same grounds that it burned before we were born and that it burns today in China. We don't infer from the •past to the •future as such; we infer from the •known to the •unknown, from observed facts to unobserved facts. . . . This second category includes the whole region of the future; but it also includes nearly the whole of the present and of the past.

Express it how you will, the proposition that *the course of nature is uniform* is the basic principle or general axiom of induction. But it doesn't *explain* the inductive process. On the contrary, it is itself an instance of induction, and induction that is by no means obvious. Far from being the first induction we make, it's one of the last—or anyway one of the last that we make in a philosophically accurate form. In fact it has hardly entered into the minds of any but philosophers, and we'll see that even *they* have haven't always had a sound conception of its extent and limits. This great generalisation is, in fact, itself based on prior generalisations. The obscurer laws of nature were discovered by means of it, but the more obvious ones must have been understood and assented to as general truths before it was ever heard of. We would never have thought of saying that all

phenomena conform to general laws if we hadn't first arrived at some knowledge of many of the laws themselves—which had to be done by induction. In what sense, then, can a principle that is so far from being our earliest induction be our warrant for all the others? In the only sense in which the general propositions that we place at the head of our syllogisms ever really contribute to their validity. (I explained what this is in II.3.) Whately remarks that every induction is a syllogism with the major premise suppressed; a better formulation would be that every induction can be put into syllogistic form by supplying a major premise. If this is actually done, the principle of the uniformity of the course of nature will appear as the ultimate major premise of all inductions; so it will relate to all inductions in the way the major premise of *every* syllogism relates to its conclusion. And what relation is that? It doesn't contribute to proving the conclusion; but it's a necessary condition of its being proved, because no conclusion is proved unless there's a true major premise.

You may want me to explain this claim that the uniformity of the course of nature is the ultimate major premise in all inductions. It certainly isn't the *immediate* major premise in every inductive argument. . . . The induction, 'John, Peter, etc. are mortal, therefore all mankind are mortal' can be put into syllogistic form by prefixing the major premise that what is true of John, Peter, etc. is true of all mankind. How did we get this major premise? It isn't self-evident. . . ., so we must have arrived at it by induction. [Mill says 'by induction or ratiocination', but he drops ratiocination without comment.] If by induction, this process like all other inductive arguments can be put into syllogistic form; and we need to construct this previous syllogism. There is in the long run only one possible construction: the real proof that what is true of John, Peter, etc. is true of all mankind can only be that a

different supposition would be inconsistent with the known uniformity in the course of nature. Whether there actually *would* be this inconsistency may be a matter of long and delicate inquiry; but if there wouldn't, we have no sufficient ground for the major premise of the inductive syllogism. It seems, then, that if we put the whole course of any inductive argument into a series of syllogisms, we'll eventually arrive at an ultimate syllogism whose major premise is the principle or axiom of the uniformity of the course of nature.[1]

Why should this axiom be accepted as true? It wasn't to be expected that thinkers would unanimously give one answer to this question, any more than with other axioms. I have already said that I think it is itself a generalisation from experience. Others think we're compelled by the constitution of our thinking faculty to assume it as true in advance of any verification by experience. Having in II.5–6 fought at such length against a similar doctrine regarding the axioms of mathematics, using arguments that largely apply also to the present case, I'll postpone going into more detail about it until chapter 21. At present it matters more to understand the import of the axiom itself. The proposition 'The course of nature is uniform' has the brevity suitable for ordinary talk rather than the precision required in philosophical language; if it's to be accepted as true, its terms need to be explained, and given a stricter signification than they have in ordinary speech.

§2. Everyone knows that he doesn't always expect uniformity in the course of events; he doesn't always believe that the unknown resembles the known, that the future will resemble the past. Nobody believes that the pattern of rain and sunshine will always be the same as it is this year. Nobody expects to have the same dreams every night; indeed when the course of nature *is* constant in these things, everyone mentions it as something extraordinary. To look for constancy where it's not to be expected—e.g. to expect that a date that once brought good fortune will always be a fortunate date—is rightly regarded as superstition.

The course of nature is not only uniform, it's infinitely various. Some phenomena always recur in the combinations they had when we first met with them; others seem altogether capricious; and some get us used to experiencing them in one particular combination and then unexpectedly break that pattern. The experience of an inhabitant of Central Africa fifty years ago supported 'All human beings are black' as well as it supported anything. To Europeans until recently 'All swans are white' seemed to be an equally straightforward example of uniformity in the course of nature. Each group had to wait fifty centuries for the experience that showed them to be wrong. During that time, mankind believed in a uniformity in the course of nature where no such uniformity really existed.

---

[1]   It needn't be uniformity that pervades all of nature. It's enough that it pervades the particular class of phenomena to which the induction relates. An induction about planetary motion wouldn't be spoiled if we thought that wind and weather are the sport of chance, provided we are assuming that astronomical phenomena are governed by general laws. Otherwise the early experience of mankind would have rested on a very weak foundation, because in the infancy of science it couldn't be known that *all* phenomena are regular in their course.

The major premise doesn't have to be known in advance; it's enough if we *can* now know it. . . . The conclusion 'The Duke of Wellington is mortal', inferred from the instances A, B, and C, implies either that we have concluded all men to be mortal or are now entitled to do so from the same evidence. A vast amount of confusion and bad logic regarding the grounds of induction would be dispelled by keeping these simple considerations in view.

According to the ancients' notion of induction, the 'Black person' and 'White swan' conclusions involved inferences that were as legitimate as any inductions whatever. Because each conclusion was false, the ground of inference must have been insufficient, but still there was as much ground for it as this conception of induction admitted of. The induction of the ancients has been well described by Bacon under the label 'induction by simple enumeration. . . .'. It consists in ascribing the character of general truths to all propositions that are true in every instance that we happen to know of. This is the kind of induction that is natural to a mind that isn't used to scientific methods. The tendency (some say 'instinct', others say 'association') to infer the future from the past, the known from the unknown, is simply a habit of expecting that what has been found true once or several times, and never yet found false, will be found true again. It makes no difference whether the instances are few or many, conclusive or inconclusive; these considerations occur only on reflection. The unprompted unreflective tendency of the mind is to generalise its experience, provided it all points in one direction and no conflicting experience comes •unsought. The notion of •seeking it, •experimenting for it, •interrogating nature (Bacon's phrase) is a much later development. When uncultivated intellects observe nature, they are passive; they accept the facts that present themselves, without actively searching for more. Only a superior mind asks itself 'What facts do I need to come to a safe conclusion?' and then looks out for these.

But though we always tend to generalise from unvarying experience, we aren't always justified in doing so. We aren't entitled to conclude that something is universally true because we have never known a counter-instance to it unless we have reason to believe that if there were any counter-instances in nature we would have known of them. When we

do have this assurance—·this reason to believe. . . etc.·—this may enable induction by simple enumeration to amount practically to proof. But cases where that is how things stand are very •remarkable—I'll discuss them in chapters 21–22 below. No such assurance can be had on any of the •ordinary subjects of scientific inquiry. Popular [see Glossary] notions are usually based on induction by simple enumeration; in science that doesn't take us far. We're forced to begin with it; we often have to rely on it provisionally, in the absence of anything better; but for the accurate study of nature we need a surer and a more potent instrument.

Bacon's usual title of 'Founder of the Inductive Philosophy' is one he deserved primarily for pointing out the insufficiency of this rough and loose conception of induction. The value of his own contributions to a more philosophical theory of the subject has certainly been exaggerated. Although his writings contain. . . .more or less fully developed statements of several important principles of the inductive method, physical science has now far outgrown the Baconian conception of induction. Moral and political inquiry still haven't caught up with that conception. The approved modes of reasoning on these subjects are still. . . .the very induction by simple enumeration that he condemns; and the 'experience' that we hear so confidently appealed to by all sects, parties, and interests is still, in Bacon's own emphatic words, *mera palpatio*. [Latin for 'mere feeling'. The kind of feeling he is referring to can be gathered from this: '·Those who steer by simple experience are· like men in the dark, patting the walls as they go along hoping to find their way, when they'd have done much better to wait for daylight, or light a candle, and *then* set off.']

§3. For a better understanding of the problem the logician must solve if he's to establish a scientific theory of induction, let us compare some incorrect inductions with others that

are acknowledged to be legitimate.... That all swans are white can't have been a good induction, because the conclusion turned out to be false. But....from the earliest records, the testimony of the inhabitants of the known world was unanimous on the point. So the uniform experience of the inhabitants of the known world, agreeing in a common result with no known counter-examples, isn't always sufficient to establish a general conclusion.

Now take an instance that is apparently not very dissimilar to this. Mankind were wrong in concluding that all swans are white; are we also wrong when we conclude that all men's heads grow above their shoulders, and never below, in spite of the conflicting testimony of the naturalist Pliny? As there were black swans, though civilised people had existed for 3000 years on the earth without meeting with them, may there not also be 'men whose heads do grow beneath their shoulders', despite a rather less perfect unanimity of negative testimony from observers? [The quoted phrase is from *Othello*.] Most people would answer No: it's more credible that a bird should vary in its colour than that men should vary in the relative position of their principal organs. Of course they would be right—but *why* are they right? We can't answer that without going into the true theory of induction more deeply than is usually done.

....When a chemist announces the existence and properties of a newly-discovered substance, if we trust his accuracy we feel assured that his conclusions will hold universally, although the induction is based on a single instance. We don't withhold our assent until the experiment is repeated; or if we do, it's because we aren't sure that the one experiment was properly done; we are sure that if it was properly done it was conclusive. So here we have a general law of nature, inferred without hesitation from a single instance; a universal proposition from a singular one. Now contrast that with another case. All the instances that have been observed since the beginning of the world in support of 'All crows are black' wouldn't outweigh the testimony of one apparently reliable witness who said that in a region of the earth not fully explored he had caught and examined a crow and found it to be gray.

Why is it that in some cases a single instance is sufficient for a complete induction, while in others myriads of concurring instances without a single known or presumed exception go such a very little way toward establishing a universal proposition? Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction.

Chapter 4. Laws of nature

§1. ....The uniformity of the course of nature is really *uniformities*.... The course of nature in general is constant because the course of each of the various phenomena that compose it is so. A certain fact invariably occurs whenever certain circumstances are present, and doesn't occur when they are absent; the same is true of another fact; and so on. From these separate threads of connection between parts of the great whole that we call 'nature' a general tissue of connection unavoidably weaves itself, by which the whole is held together. If

A is always accompanied by D,

B by E and

C by F,

it follows that

A B is accompanied by D E,

A C by D F,

B C by E F, and finally

A B C by D E F.

That is how the general character of regularity is produced—a regularity which, along with and in the midst of infinite diversity, pervades all nature.

So the uniformity of the course of nature is itself a complex fact, compounded of all the separate uniformities that exist in respect of single phenomena. The •ordinary name of these various uniformities, when they are established by what we regard as a sufficient induction, is 'laws of nature'. In •scientific usage we use 'law of nature' in a more restricted sense, to refer to the uniformities when reduced to their simplest expression. In the above illustration, already seven uniformities. . . .would be called 'laws of nature' in the everyday loose sense of that phrase; but only ·the first· three of the seven are properly distinct and independent; and when they are presupposed, the others automatically follow. So the first three are 'laws of nature' in the narrower sense; the other four are not, because they are mere cases of the first three, virtually included in them, and are therefore said to result from them. Anyone who affirms those three has already affirmed all the rest.

Here are three uniformities, or call them 'laws of nature':

(1) Air has weight,

(2) Pressure on a fluid spreads equally in all directions.

(3) Pressure in one direction, not opposed by equal pressure in the opposite direction, produces motion that doesn't cease until equilibrium is restored.

These three uniformities should enable us to predict another uniformity, namely the rise of the mercury in the Torricellian tube [= a barometer]. This isn't a 'law of nature' in the narrower sense of the phrase; it's a *result of* laws of nature. It's a case of each of the three laws; and it couldn't be different without infringing at least one of them. If the mercury were not held up in the barometer at a height such that the column of mercury had the same weight as a column of the atmosphere of the same diameter, this would be a case of

(1) the air not pressing on the surface of the mercury with the force that is called its 'weight', or of

(2) the downward pressure on the mercury not being passed on equally in an upward direction, or of

(3) a body pressed in one direction and either not moving in that direction or stopping before reaching equilibrium.

So if we knew the three simple laws but had never tried the Torricellian experiment, we could have *deduced* its result from those laws. The known weight of the air combined with the position of the apparatus would bring the mercury within the range of the first of the three inductions; the first induction would bring it within the second, and the second within the third—all in the way I described in my account of ratiocination. We would be coming to know the more complex uniformity independently of •specific experience, through our knowledge of the simpler ones from which it results; though in due course we'll see reasons why *verification* by •specific experience would still be desirable and might even be indispensable.

[Mill makes remarks about broad and narrow senses of 'law of nature', suggesting that the narrow sense favoured by scientists is explained by a 'tacit reference to the original sense of "law", namely "the expression of the will of a superior".' He then offers two ways of restating the narrow-sense

version of the question 'What are the laws of nature?':]

> •What are the fewest and simplest assumptions, which being granted the whole existing order of nature would result?
> •What are the fewest general propositions from which all the uniformities which exist in the universe might be deductively inferred?

Every great advance that marks an epoch in the progress of science has been a step towards solving this problem. Even a simple colligation of inductions already made, with no fresh extension of the inductive inference, is already an advance in that direction. When Kepler expressed the regularity in the observed motions of the planets by the three general propositions called his 'laws', he was showing three simple suppositions that would suffice to construct the whole scheme of **planetary motion** so far as it was then known. A still greater step was made when these laws....were discovered to be cases of the three laws of **motion** generally.... After this great discovery, Kepler's three propositions, though still called 'laws', wouldn't be called 'laws of nature' by anyone accustomed to using language with precision; that phrase would be reserved for the simpler and more general laws into which Newton is said to have resolved [see Glossary] them.

Every well-grounded inductive generalisation is either •a law of nature or •a result of laws of nature, something that could be predicted from them. And the problem of inductive logic can be summed up in two questions; how to ascertain the laws of nature; and how then to follow them into their results. But this isn't a real analysis of the problem; it's a mere verbal transformation of it.... Still, it is worth something to have reached the insight that the study of nature is the study of laws, not *a* law; of uniformities (plural); that the different natural phenomena have their separate rules or ways of occurring, which—though intermixed and entangled with one another—can to some extent be studied separately; that the regularity in nature is a web composed of distinct threads, and only to be understood by tracing each thread separately, for which purpose it is often necessary to unravel some portion of the web and exhibit the fibres apart. The rules of experimental inquiry are the contrivances for unraveling the web.

§2. In trying in this way to ascertain the general order of nature by ascertaining the particular order of each one of the phenomena of nature, a scientific proceeding can't be more than an improved form of what the human understanding primitively did when not directed by science. When mankind first got the idea of studying phenomena by a method stricter and surer than the one they spontaneously started out with, they didn't start with the supposition that nothing had yet been ascertained (Descartes's well-meant but impracticable advice). Many of the uniformities in phenomena are so constant and so open that they force themselves on involuntary recognition. Some facts are so perpetually and familiarly accompanied by certain others that mankind learned—as children learn—to expect one where they found the other, long before they knew how to put this expectation into a proposition about there being a connection between those phenomena. No science was needed to teach that food nourishes, that water drowns or quenches thirst, that the sun gives light and heat, that bodies fall to the ground. The first scientific inquirers assumed these and their like, and set out from them to discover others that were unknown; and they weren't wrong to do this, though they later came to see that the initial spontaneous generalisations needed to be revised when the progress of knowledge showed limits to them, or showed their truth to depend on some detail

not originally attended to. You'll see later on that there's no logical fallacy in this procedure. Indeed, we can see that no alternative is workable, because it's impossible to develop any •scientific method of induction, or •test of the correctness of inductions, except on the hypothesis that some inductions deserving of reliance have already been made.

For an example, look back at one of our former illustrations. With exactly the same amount of evidence (negative and positive) in each case, why did we accept the assertion that **(i)** there are black swans yet refuse to believe any testimony saying that **(ii)** some men wear their heads underneath their shoulders? Well, **(i)** was more credible than **(ii)**; but why was it more credible?. . . . Apparently because there is less constancy in the colours of animals than in the general structure of their anatomy. But how do we know this? From experience, of course. It appears, then, that we need experience to tell us how much we should rely on experience, and in what cases or sorts of cases. We have to consult •experience to learn from •it when and where arguments from •it will be valid. We have no rock-bottom test to which we subject experience in general; we make experience its own test. Experience testifies that some of the uniformities it exhibits or seems to exhibit are more to be relied on than others. We have experienced uniformity U; how confident should we be that it holds in cases not yet observed? That depends on the extent to which U belongs to a class of uniformities that have hitherto been found to be uniform.

This way of correcting one generalisation by means of another—a narrower generalisation by a wider one that common sense suggests and adopts in practice—is the real type [see Glossary] of scientific induction. Skillfully contrived rules can give accuracy and precision to this process, and adapt it to all varieties of cases, but they can't make any essential difference to its principle.

To apply a test of this sort we must already have some general knowledge of the typical character of the uniformities existing throughout nature. So the indispensable foundation of a scientific formula of induction has to be a survey of the inductions mankind has been unscientifically led to, with the special purpose of ascertaining what *kinds* of uniformities have been found to be perfectly invariable, pervading all nature, and what kinds have been found to vary with difference of time, place, or other changeable details.

§3. The need for such a survey is confirmed by the fact that stronger inductions are the touchstone to which we always try to bring the weaker. If we find a way to deduce a weaker induction from stronger ones, it instantly acquires all their strength; and it even adds to that strength, because the experience on which the weaker induction previously rested becomes additional evidence for the truth of the stronger ones. Suppose we infer from historical evidence that the uncontrolled power of a monarch, of an aristocracy, or of the majority, will often be abused; we can rely on this generalisation with much greater confidence when it is shown to follow from facts that are even better established—•the low degree of elevation of character ever yet attained by the average of mankind, and •the poor success-rate of most known procedures for making reason and conscience predominate over the selfish propensities. And obviously these more general facts get more evidence from what history tells us about the effects of despotism. The strong induction becomes still stronger when a weaker one has been bound up with it.

On the other hand, if an induction conflicts with stronger inductions or with conclusions that follow from them, then the weaker one must give way—unless reconsideration shows that some of the stronger inductions have been expressed with greater universality than their evidence justifies. The

age-old opinion that a comet. . . .was the precursor of calamities. . . .; the belief in the truthfulness of the oracles of Delphi or Dodona; the reliance on astrology or on the weather-prophecies in almanacs; these were doubtless inductions supposed to be based on experience; and it seems that faith in such delusions can hold out against many failures, as long as it's nourished by a reasonable number of casual coincidences between the prediction and the event.[1] What has really put an end to these insufficient inductions is their inconsistency with the stronger inductions subsequently obtained through scientific inquiry, concerning the causes that terrestrial events really depend on. In places where those scientific truths haven't yet penetrated, the same or similar delusions still prevail.

Here is a ·two-part· general principle about *any* two inductions, whether strong or weak:

(1) If they can be connected by ratiocination, they tend to confirm one another.

(2) If they lead deductively to consequences that are incompatible, they become tests of each other, showing that one or other must be given up or at least limited in some way.

In case **(1)** the induction that becomes a conclusion from ratiocination becomes at least as certain as the weakest of those from which it is deduced; while in general all are more or less increased in certainty. Thus the mercury-in-tube experiment, though it's a mere case of three more general laws, doesn't just strengthen greatly the evidence on which those laws rested but raises the level of one of them (the weight of the atmosphere) from 'doubtful' to 'completely established'.

Thus, if among the uniformities that have been found to exist in nature there are •some that may be considered quite certain and quite universal (so far as any human purpose requires certainty), then we may be able to use •these to raise multitudes of other inductions to the same point on the ·certainty· scale. For if we can show that either •inductive inference I_2 is true or •the certain and universal induction I_1 has an exception, then I_2 will attain the same certainty and security as I_1. . . . It will be proved to be a law; and if it's not a result of other and simpler laws, it will be a law of nature.

There *are* such certain and universal inductions; and it's because there are that a logic of induction is possible.

---

[1]   Whewell won't allow these and their like to count as 'inductions', because such superstitious fancies 'were not •collected from the facts by seeking a law of their occurrence, but were •suggested by an imagining of the anger of superior powers. . . .' But the question is not 'How were these notions first suggested?' but 'What evidence is. . . .supposed to support them?' If the believers in these erroneous opinions had been challenged to defend them, they would have referred to experience: to the comet that preceded the death of Julius Caesar, or to oracles and other prophecies known to have been fulfilled. Analogous superstitions exist even today, and their hold on the believers' minds depends on the supposed evidence of experience—·mostly consisting of casual coincidences· [see Glossary]. I admit that the influence of such coincidences wouldn't be what it is if it weren't strengthened by an antecedent presumption; but this is not a special feature of superstitions; preconceived notions of probability help to explain many other cases of belief on insufficient evidence. The *a priori* prejudice improperly predisposes the believer's mind to interpret his experience in that way, but the believer still sincerely regards his belief as a legitimate conclusion from experience.

—My theme could easily be illustrated by cases where antecedent prejudice has no role. Whately writes: 'For many ages all farmers and gardeners were firmly convinced •that the crops would never turn out good unless the seed was sown when the moon was waxing, and •that they had learned this from experience.' This was induction, but bad induction; just as an invalid syllogism is reasoning, but bad reasoning.

Chapter 5. The law of universal causation

§1. The phenomena of nature exist in two relations to one another—simultaneity and succession. Every phenomenon is related in a uniform manner to •some phenomena that coexist with it and to •some that have preceded and will follow it.

Of the uniformities that exist among synchronous [see Glossary] phenomena, the most important in every way are the laws of number, closely followed by the laws of space, i.e. of extension and figure. The laws of number are common to synchronous and successive phenomena. That two and two make four is equally true whether the second two follow the first two or accompany them; it's as true of days and years as of feet and inches. In contrast with that, the laws of extension and figure (i.e. the theorems of geometry) are laws only of simultaneous phenomena. The various parts of space and of the objects that are said to 'fill' space *coexist*, and the unvarying laws that are the subject of the science of geometry express *how* they coexist. [Mill is here taking 'x coexists with y' to mean 'x exists at some time when y also exists'.]

To understand and prove these laws—i.e. these uniformities—you don't have to suppose any lapse of time, any variety of facts or events succeeding one another. The propositions of geometry are independent of the succession of events. All things that have extension, i.e. that fill space, are subject to geometrical laws. Having extension they must have figure; so they must •have some figure in particular and •have all the properties that geometry assigns to that figure. An example: If x is a sphere and y a cylinder with the same height and diameter, x's volume will be exactly two-thirds of y's, no matter what stuff x and y are made of. Another example: Each body and each point within a body

must occupy some place or position among other bodies; and the position of two bodies relatively to each other, whatever stuff they are made of, can be unerringly inferred from the position of each of them relatively to any third body.

In the laws of number, then, and in the laws of space, we clear cases of the rigorous universality that we're looking for. Those laws have always been the type [see Glossary] of certainty, the standard of comparison for all lower degrees of evidentness. They are so perfectly invariable that we can't even *conceive* any exception to them (and philosophers have been led—though wrongly—to think that what makes them evident is not experience but the basic constitution of the intellect). So if we could deduce from the laws of space and number any other kind of uniformities, that would be proof positive that those other uniformities had the same rigorous certainty. But we can't do this. From laws of space and number alone nothing can be deduced but laws of space and number.

For us the most valuable truths about phenomena are the ones concerning the order of their succession. Our knowledge of these truths is the basis for every reasonable anticipation of future facts, and for any power we have to influence those facts to our advantage. Even the laws of geometry—·which don't involve succession·—are chiefly of practical importance to us because they enter into premises from which we can infer the order of the succession of phenomena. The motion of bodies, the action of forces, and the propagation of influences of all sorts take place along certain lines and over definite distances; so the properties of those lines and distances are an important part of the laws to which those phenomena are subject. Similarly, •motions,

•forces or other influences, and •times are numerable quantities; so the properties of number are applicable to them as to everything else. But the laws of number and space can't *unaided* contribute to the discovery of uniformities of succession. They can be made to do that work only when we combine with them premises about uniformities of succession that we already know. Take for example the propositions:

> •Bodies acted on by an instantaneous force move with uniform velocity in straight lines.
> •Bodies acted on by a continuous force move with accelerated velocity in straight lines.
> •Bodies acted on by two forces in different directions move in the diagonal of a parallelogram, whose sides represent the direction and quantity of those forces.

If we combine these truths with certain geometrical propositions (e.g. that a triangle is half a parallelogram of the same base and altitude), we can deduce another important uniformity of succession:

> •A body moving around a centre of force marks off areas proportional to the times.

But we must have laws of succession in our premises if we are to reach truths of succession in our conclusions. . . .

The laws of space are only laws of simultaneous phenomena; and the laws of number, though true of successive phenomena, don't relate to their succession; so the rigorous •certainty and •universality of these laws don't carry through to laws of succession. We must try, then, to find some law of succession that has •those attributes, making it a fit basis for processes of discovering and testing all other uniformities of succession. This basic law must resemble the truths of geometry in their most remarkable special feature, namely that they are never *ever* defeated or suspended.

Of the uniformities in the succession of phenomena that common observation brings to light very •few have even an *apparent* claim to this rigorous indefeasibility; and of those few only •one has been completely sustained in this claim. That one, however, is a law that is universal also in another sense; it is coextensive [see Glossary] with the entire field of successive phenomena, all instances of succession being examples of it. This law is the Law of Causation. The truth that *every fact that has a beginning has a cause* is coextensive with human experience.

You may think that this doesn't amount to much, because it only says 'It's a law that every event depends on some law' or 'It's a law that there's a law for everything'. But we shouldn't conclude that the principle's generality is merely verbal; when we look into it we'll find that far from being vague or meaningless it is a most important and really fundamental truth.

§2. The notion of *cause* is the root of the whole theory of induction; so we must at the outset of our inquiry get it fixed with as much precision as we can manage. There is an old and still-running battle among different schools of metaphysicians concerning the •origin and •analysis of our idea of causation; but—fortunately!—we don't need to settle *that* before starting our search for the true theory of induction. The science of *the investigation of truth by means of evidence* is •independent of many of the controversies that perplex the science of *the ultimate constitution of the human mind*, and •has no need to push the analysis of mental phenomenon to the extreme limit that a metaphysician ought to demand.

Thus, when in the course of this inquiry I speak of the 'cause' of any phenomenon, I don't mean a cause that isn't itself a phenomenon; I am not inquiring into the ultimate or ontological cause of anything. To adopt a distinction familiar

in the writings of Reid and other Scottish metaphysicians, the causes I'm concerned with are not **efficient causes** [see Glossary] but **physical causes**. They are 'causes' purely in the sense in which one physical fact is said to be the 'cause' of another. I'm not called upon to give an opinion about the 'efficient causes' of phenomena, or whether there are any. According to the schools of metaphysics that are most currently most fashionable,

> The notion of causation implies a mysterious and most powerful tie of a kind that can't (or anyway doesn't) exist between two physical facts x and y such that x is always followed by y and is popularly caused y's 'cause'. So if we want to find the true cause, the cause that isn't only •followed by the effect but actually •produces it, we have to ascend higher [Mill's phrase; we might prefer to say 'dig deeper'] into the essences and inherent constitution of things.

I have no need to do that for the purposes of the present inquiry, and no such doctrine will be found in the following pages. The only notion of *cause* that the theory of induction needs is one that can be gained from experience. The Law of Causation, the recognition of which is the main pillar of inductive science, is merely the familiar truth that *invariability of succession* is empirically found to obtain between every fact in nature and some other fact that has preceded it—independently of any question about •the ultimate ·or absolutely basic· cause of phenomena or about •the nature of 'things in themselves'.

So there's an invariable order of succession between phenomena existing at any instant and the phenomena that exist at the next instant. . . . Certain facts always are—and, we believe, always will be—followed by certain other facts. The invariable antecedent is termed the 'cause', and the invariable consequent the 'effect'. And the law

of causation holds universally because every consequent is connected in this way with some particular antecedent or set of antecedents. Every fact that has begun to exist was preceded by some fact(s) with which it is invariably connected. For every event E there's some combination of objects or events—some combination of circumstances, positive and negative—the occurrence of which is always followed by E. Even when we don't yet know what this combination is for a given E, we never doubt that there is one, and that it never occurs without E as its effect or consequence. If this truth weren't universal, we couldn't express the inductive process in rules. . . .

§3. This invariable sequence seldom if ever holds between a consequent and •a single antecedent. It's usually between a consequent and •the sum of several antecedents, the concurrence [see Glossary] of all of them being needed to produce—i.e. to be certain of being followed by—the consequent. People often single out one of the antecedents as the 'cause' and call the others merely 'conditions'. [Mill elaborates this in more detail than we need. Someone dies because of his eating some oysters. Many will say that his eating the oysters was 'the cause' of his death, but other things were also needed: his general physical constitution, his state of health at this moment, perhaps the room-temperature, etc. These plus the eating of the oysters made up the cause of his death: •the other causes were waiting to have the oyster-meal added to them so that the effect could be produced. •They tend to be left out of 'the cause' because they were relatively long-lasting states of affairs and not short-term *events* like the eating of the meal. Because the total cause was topped up by that one event, people get the impression that the event had 'a more immediate and close connection' with the death than did the other conditions; but it didn't.]

Even when we're aiming at accuracy we don't list all the conditions, but that's because some of them are understood without being expressed, or because our immediate purpose won't be harmed by omitting them. When we say that the cause of a man's death was that •his foot slipped when he was climbing a ladder, we omit •his weight as part of the clause (though it related to his death in the same way as his foot-slip did) because there was no need to mention it in this context. . . . When the decision of a legislative assembly is settled by the casting vote of the chairman, we sometimes say that *he* was the cause of everything that resulted from the enactment. We don't really think that his single vote contributed more to the result than any other affirmative vote; but for our present purpose, namely to insist on his individual responsibility, the part that anyone else had in the transaction is not material.

In all these examples, the fact that was picked out as 'the cause' was the condition that came into existence last. But don't think that in the use of 'the cause' we are strictly guided by this or any other rule. There's no scientific basis for the distinction between the •cause of a phenomenon and its •conditions; you can see this from how capriciously we select the condition of an event that we choose to call its 'cause'. However many conditions there are, almost any of them might count as 'the cause' because our immediate purpose can afford to neglect the others. Take a case where a stone thrown into water sinks to the bottom. What are the conditions of this event? In the first place there must be •a stone, and •water, and •the stone's being thrown into the water; but these suppositions are part of the statement of what the event *is*, and it's bad, a tautology, to include them among the 'conditions' of the event. This class of conditions have never been called 'cause' except by the Aristotelians, who called them 'the material cause'. [Actually, they'd have

said this about the stone and the water, but not about the stone's being thrown into the water.] The next condition is there being •an earth; and accordingly it's often said that the fall of a stone is caused by the earth, i.e. by
  •a power of the earth,
  •a property of the earth, or
  •a force exerted by the earth,
all of which are merely roundabout ways of saying that it is caused by the earth. Or the fall may be said to be caused by
  •the earth's attraction.
That's a technical way of saying that *the earth* causes the motion, with an extra special feature, namely that the motion is toward the earth—which is a feature of the effect, not of the cause. Now pass to another condition: it's not enough that the earth should exist; the body must be close enough to the earth for the earth's attraction to outweigh the attraction of any other body. So we can correctly say that the cause of the stone's falling is its being within the sphere of the earth's attraction. A further condition: because the stone is immersed in water, it can't reach the bottom unless its specific gravity exceeds that of the water, i.e. unless it weighs more than an equal volume of water. So it would be regarded as correct to say that the cause of the stone's going to the bottom was its exceeding in specific gravity the fluid in which it was immersed.

Thus we see that *each* condition of the phenomenon may be taken in its turn and spoken of as if it were the entire cause—with equal propriety in everyday speech and equal impropriety in scientific discourse. The particular condition picked out as 'the cause' is usually the one •whose share in the event is superficially the most conspicuous, or •whose status as required for the event we happen to be emphasising at the moment. This second consideration can even lead us to select as 'the cause' one of the negative conditions, as in '

The army was taken by surprise because the sentinel was off his post'. The sentinel's absence didn't create the enemy or put the soldiers to sleep, so how did it cause them to be surprised? All that is really meant is that the event wouldn't have happened if he had been at his duty. His being off his post was not

- a producing cause, but merely
- the absence of a preventing cause.

It was simply equivalent to his non-existence. No consequences can come from nothing, from a mere negation. All effects are connected by the law of causation with some set of *positive* conditions, though *negative* ones are almost always required in addition. In short: every fact or phenomenon that has a beginning arises when some certain combination of positive facts exists, provided certain other positive facts do not exist.

We tend to associate the idea of the cause of E_2 with the event E_1 that immediately precedes E_2, rather than with any of the earlier states—i.e. permanent facts—that are also conditions of E_2. (You can see this in the example of death caused by eating oysters.) The reason for this tendency is that E_1 begins to exist immediately before E_2, whereas the other conditions may have pre-existed for an indefinite time. We see this tendency at work in the different logical fictions that even men of science resort to so as not to give the name 'cause' to anything that existed for an indeterminate length of time before the effect. Thus, rather than saying that *the earth* causes the fall of bodies, they ascribe it to *a force exerted by the earth* or *an attraction by the earth*; and they think of these abstractions as used up by each effort and therefore constituting at each successive instant a new fact that is simultaneous with the effect or immediately preceding it. . . .

·START OF A LONG FOOTNOTE·

An intelligent reviewer of this work in the *Prospective Review* [R. H. Hutton] disputes my thesis that *any* condition of a phenomenon may be—and on some occasions and for some purposes actually is—spoken of as 'the cause'. He says: 'We always apply the word "cause" to the element in the antecedents that exercises *force*. . . .' Also: 'everyone would feel' it to be wrong to say that the cause of a surprise was the sentinel's being off his post, but would feel that the 'allurement or force which *drew* him off his post might be so called. . . .'. I can't think that of these two:

- The event occurred because the sentinel was absent
- The event occurred because the sentinel was bribed to be absent

one is wrong and the other right. The only direct effect of the bribe was his absence, so the bribe could be called the remote cause of the surprise; but only because the absence was the immediate cause. I don't think anyone would accept one expression and reject the other unless he had a theory to support.

[The reviewer claimed that several statements implied by Mill's account—e.g. that a man's having bodily organs was part of the cause of his dying when he took poison—are things that no-one would say. Mill accepts this, and patiently repeats his explanation of why such things sound wrong though they are true. He continues:]

As for the assertion that nothing is called 'the cause' unless it exerts active force: I'll set aside the question of what 'active force' means, and will use the phrase in its popular sense. Well, then, of these two—

- He fell because his foot slipped in climbing a ladder
- He fell because of his weight

—which sounds better? The active force bringing about his fall was his weight, not the motion of his foot! [Mill gives other

examples in which the most intuitively-plausible candidate for the role of 'the cause' is *not* the force-exerting one, ending with:] The opening of flood-gates is said to be the cause of the flow of water; yet the active force is exerted by the water itself, and opening the flood-gates merely supplies a negative condition. The reviewer adds: 'Relations of space and time are absolutely passive conditions yet are absolutely necessary to physical phenomena; but no-one ever applies the word "cause" to these without being immediately stopped by those who hear him.' I have to disagree even with this. Few people would feel that it was wrong or strange to say that a secret became known because it was spoken of when X was within earshot (a condition of space), or that the cause why this tree is taller than that one is that it has been longer planted (a condition of time).

·END OF THE LONG FOOTNOTE·

Philosophically speaking, then, the cause is the sum total of the conditions, positive and negative—the whole of the contingencies of every sort from which the consequent invariably follows. But the negative conditions of any phenomenon can be all summed up in one phrase, 'the absence of preventing or counteracting causes', which spares us the wordy labour of listing them separately. The convenience of this form of expression is mainly based on the fact that in most cases cause C_1's effects in counteracting cause C_2 can with strict scientific exactness be regarded as a mere extension of C_1's own proper and separate effects. When gravity retards the upward motion of a projectile and deflects

it into a different trajectory, it is producing the very same •kind of effect—and even (as mathematicians know) the same •quantity of effect, as it does when causing an unsupported body to fall to the ground. When an alkaline solution mixed with an acid destroys its sourness, and prevents it from reddening vegetable blues, it's because the specific effect of the alkali is to combine with the acid and form a compound with totally different qualities. Causes of all sorts have this property of preventing the effects of other causes by virtue of the same laws according to which they produce their own.[1] This enables us to do without any mention of negative conditions: we can •establish the general axiom that all causes are liable to be counteracted in their effects by one another, and •limit the notion of cause to

•the sum of the positive conditions, and
•one negative condition, always the same one, namely the absence of counteracting causes;

·and just because the negative condition is always the same it can be silently understood, and in that spirit dropped from the story·.

§4. . . . .In most cases of causation a distinction is commonly drawn between •something that acts and •some other thing that is acted upon; between an •agent and a •patient [see Glossary]. Everyone would agree that both of these are •conditions of the phenomenon, but it would be thought absurd to call the latter the 'cause', that label being reserved for the former. But when we look into this we find that this distinction vanishes, or rather turns out to be only verbal. Its

[1] There are a few ·apparent· exceptions; for some properties of objects seem to be purely preventive, e.g. the property of opaque bodies by which they intercept the passage of light. This, as far as we can understand it, seems to be a case of an agency that shows up *only* in defeating the effects of another agency. If we knew what other relations to light, or what peculiarities of structure, opacity depends on, we might find that this is only an apparent exception to the general proposition in the text, not a real one. Either way, it needn't affect the practical application. The formula that includes all the negative conditions of an effect in the single one of the absence of counteracting causes is not violated by such cases as this; though if all counteracting agencies were like this there would be no point in employing the formula.

source is a mere fact about wording: the object that is said to be acted on—and is regarded as the scene in which the effect occurs—is usually included in the phrase by which the *effect* is spoken of, so that if it were also counted as part of the cause there would be the appearance of something's being incongruously said to cause itself. Return to falling bodies, and the question: 'What is the cause that makes a stone fall?' If the answer had been 'the stone itself', that would seem to contradict the meaning of the word 'cause'. So the stone is conceived as the patient, and the earth is represented as the agent or cause. [Mill wrote 'the earth (or, according to the common and most unphilosophical practice, an occult [see Glossary] quality of the earth)'.] But this is a superficial matter: we can conceive the stone as causing its own fall, as long as we word this so as to avoid the mere verbal incongruity. We might say: 'The stone moves toward the earth by the properties of the matter composing it'; and then there's nothing wrong with calling the stone itself the 'agent'. (Wanting to save the established doctrine that matter is inactive, men have usually preferred to say that the cause is not •the stone itself but •its weight or gravitation—an occult quality.)

Those who have defended a radical distinction between agent and patient have generally had this thought:

> The 'agent' is what causes some state of, or some change in the state of, another object that is called the 'patient'.

But a little reflection will show that our way of speaking of phenomena as *states* of the various objects that take part in them. . . .is simply a sort of logical fiction, sometimes useful as one among several formulations, but never to be mistaken for an expression of a scientific truth. Even the attributes of an object that might seem with greatest propriety to be called 'states' of the object—its sensible qualities, its colour, hardness, shape, and the like—are really. . . .phenomena of

causation, in which the substance is the agent or producing cause, and the patient is our own organs and those of other sentient beings. What we call 'states of' objects are sequences ·of events· into which the objects enter, usually as antecedents or causes; and things are never more *active* than when they are producing the phenomena in which they are said to be '*acted on*'! According to the theory of gravitation, a falling stone is as much an agent as the earth is—the earth attracts the stone but is also attracted by it. When a sensation is produced in our organs, the laws of our organisation—and even the laws of our minds—are as directly operative in determining the effect as are the laws of the external object. We call prussic acid the 'agent' of a person's death, but the whole of his vital and organic properties are as actively instrumental as the poison in the chain of effects that kills him. In the process of education, we may call the teacher the 'agent', and the pupil the material that is acted on; but actually all the facts that pre-existed in the pupil's mind *act* either for or against the teacher's efforts. The agent in vision isn't light alone but light coupled with the active properties of the eye and brain, and with those of the visible object. The 'agent'/'patient' distinction is purely verbal; patients are always agents; in most natural phenomena, indeed, they are agents to such a degree that they react forcibly on the causes that acted on them. . . . All the positive conditions of a phenomenon are alike agents, alike active; and in any account of the cause that professes to be complete, none of them should be excluded except ones that have already been implied in the words used for describing the effect. . . .

§5. I should deal separately with the case of causation where the effect is to invest an object with a certain property—i.e. not to produce a certain phenomenon but to fit something

else for producing it. When sulphur, charcoal, and nitre are put together in a certain way and in certain proportions, the effect is not an explosion but a mixture that is explosive—i.e. that will explode in certain circumstances. The various natural and artificial causes that educate the human body or the human mind have for their principal effect not •to make the body or mind immediately do anything but •to endow it with certain properties—i.e. to ensure that in certain circumstances certain results will take place •in it or •as consequences of it. . . . Painting a wall white doesn't merely produce the sensation of white in those who see the wall; it confers on the wall the permanent property of giving that kind of sensation. In relation to the sensation, the painting of the wall is *a condition of a condition*; it is a condition of the wall's causing that particular fact. The wall may have been painted years ago, but it has acquired a property that has lasted till now, and will last longer; the antecedent condition needed to enable the wall to become in its turn a condition has been fulfilled once for all. In a case like this, where the immediate effect is a property produced in an object, no-one these days thinks that the property is a substantive entity—·a special kind of *thing*·—'inherent' in the object. What has been produced could be called a state of preparation in an object for producing an effect. . . . In the case of the gunpowder •this state of preparation consists in the particles' coming to be close to one another. In the example of the wall, •it consists in a new spatial closeness of the wall to the paint. In the example of the moulding influences on the human mind, •its involving spatial relations is only conjectural; even if we assume the materialistic hypothesis, there's still a *question* as to whether the increased ease with

which the well-trained brain sums up a column of figures is a result of some permanent new arrangement of some of its material particles. So we must content ourselves with what we know, and include among the effects of causes the *capacities* given to objects of being causes of other effects. A 'capacity' isn't a real thing existing in the object; it's merely a name for our belief that the object *will* act in a certain way *if* certain new circumstances arise. We can give this assurance of future events a fictitious objective existence by calling it 'a state of the object'. But unless the state consists in a spatial arrangement of particles (as with the gunpowder), it expresses no present fact and is merely a contingent future fact re-presented under another name. . . .

§6. I now present a distinction that is of first-rate importance both for clarifying the notion of cause and for blocking a plausible objection that is often brought against the view I have taken of the subject.

When I define the *cause* of x—in the only sense in which this book has any concern with causes—to be 'the antecedent that x invariably **follows**' I do not mean 'the antecedent that x invariably **has followed**' in our past experience'. An account of causation in terms of 'has followed' would be open to Reid's very plausible objection that then night must be the cause of day, and day the cause of night, because day and night *have* invariably followed one another from the beginning of the world. It's essential to our use of 'cause' that we should believe not only •that the antecedent always *has* been followed by the consequent, but that as long as the present constitution of things[1] endures, it always *will* be so. And this isn't true of day and night. We believe that night will be followed by day not

---

[1]   By 'the present constitution of things' I mean the ultimate laws of nature (whatever they may be), as distinct from the derivative laws and from the collocations [see Glossary]. The daily revolution of the earth (for example) is not a part of the constitution of things, because it could be terminated or altered by natural causes.

•under all imaginable circumstances, but only
•provided the sun rises above the horizon.

If the sun stopped rising (and for all we know, its doing so may be perfectly compatible with the general laws of matter), night could be eternal. On the other hand, if the sun is above the horizon, its light not extinct, and no opaque body between it and us, we firmly believe that unless there's a change in the properties of matter •this combination of antecedents will always be followed by *day*; that •if this combination were indefinitely prolonged, it would always be day; and that •if the same combination had always existed, it would always have been day, quite independently of night as a previous condition. That's why we don't call night the 'cause' of day, or even a 'condition' of day. The only conditions of day are •the existence of the sun (or some such luminous body), and •there being no light-blocker in a straight line[1] between that body and the part of the earth where we are situated; and the combination of these, without any superfluous details, constitutes the cause. This is what writers mean when they say that the notion of cause involves the idea of *necessity*. If 'necessity' has any agreed meaning it is *unconditionalness*. To say that x is necessary, that x *must* be, is to say that x *will* be, no matter what else happens. The succession of day and night is obviously not 'necessary' in this sense, because it is conditional on the occurrence of other antecedents. If x will be followed by y when and only when z is the case, x isn't the cause of y even if no instance of x has ever occurred without y following it.

. . . .So we can define the *cause* of a phenomenon to be the antecedent (or combination of antecedents) which it invariably *and unconditionally* follows. Or if we adopt the convenient usage in which 'cause' is confined to the combination of *positive* conditions, then we must replace 'unconditionally' by 'subject to no conditions except negative ones'.

Some may want to object:

'The sequence of night and day is invariable in our experience; we have as much ground in •this case as experience can give in •any case for recognising the two phenomena as cause and effect. To say that more is necessary—to require a belief that the succession is unconditional—is to admit that causation involves an element of belief that isn't derived from experience.

I answer that it is experience itself that teaches us that one uniformity of sequence is conditional and another unconditional. When we judge that the succession of night and day is a derivative sequence, depending on something else, we are going by experience. It's the evidence of experience that convinces us that day could exist without being followed by night, and night without being followed by day. To say as Tulloch does that these beliefs are 'not generated by our mere observation of sequence' is to forget that twice in every 24 hours, when the sky is clear, we have decisive evidence that the cause of day is the sun. We have empirical knowledge of the sun that justifies us on empirical grounds in concluding that if the sun were always above the horizon there would ·always· be day even if there had been no night, and that if the sun were always below the horizon there would ·always· be night even if there had been no day. [Mill adds a reminder that if x is only a conditionally invariable antecedent of y, then x's status as an invariable antecedent of y is fragile.]

---

[1]   I say 'straight line' for brevity and simplicity. In reality the line in question is not exactly straight: because of refraction, we actually see the sun for a short interval during which the opaque mass of the earth is interposed in a direct line between the sun and our eyes. This provides us with a limited version of the luxury of seeing round a corner.

[Mill now offers a paragraph explaining how a combination of causes (unconditionally invariable antecedents) can generate conditionally invariable relations. He uses this to rebut something said by a contemporary philosopher. And other contemporaries—especially Whewell—are criticised in a further paragraph that is all about terminology.]

§7. Does a cause always relate to its effect as antecedent to consequent? Don't we often speak of two simultaneous facts as cause and effect—fire as the cause of warmth, the sun and moisture as the cause of vegetation, and the like? Since a cause doesn't necessarily go out of existence because its effect has been produced, the two do very generally coexist; and there are appearances, and common expressions, that seem to imply not only that causes *can* but that they *must* be contemporaneous with their effects. The scholastics have had as a dogma 'When the cause ceases, so does the effect' [Mill gives this in Latin], and there was a time, it seems, when it was generally believed that the continuance of an effect requires the continued existence of the cause. Kepler's numerous attempts to explain the motions of the planets on mechanical principles were doomed by his always supposing that the agency that set them in motion must continue to operate in order to keep up the motion it at first produced. Yet there have always been many familiar examples of effects continuing long after their causes had ceased. Sun-stroke gives a person brain-fever; will the fever go off as soon as he is moved into the shade? A sword is run through someone's body; must the sword remain in his body for him to continue to be dead? Once a plough has been made, it remains a ploughshare without any continuance of heating and hammering. . . . On the other hand, the pressure that forces up the mercury in a vacuum-tube must be continued in order to keep it up there. This (it may be replied)

is because another force—the force of gravity—is acting continually, and would bring the mercury down again if it weren't counterbalanced by an equally constant force. Well, then: a tight bandage causes pain, which will sometimes stop as soon as the bandage is removed. The illumination that the sun diffuses over the earth ceases when the sun goes down.

So there's a distinction to be drawn. Sometimes the conditions needed for the first production of a phenomenon are also needed for its continuance; though more often its continuance requires no condition except negative ones. •Most things, once produced, continue as they are until something changes or destroys them; but •some require the permanent presence of the agencies that produced them at first. We could choose to regard these as instantaneous phenomena, needing to be renewed at each instant by the cause that first generated them. The illumination of any given point of space has always been regarded as an instantaneous fact, which perishes and is perpetually renewed as long as the required conditions obtain. This way of talking spares us the necessity of admitting that the continuance of a cause is *ever* required to maintain the effect; because we can say that the cause is required not to •maintain the effect but to •reproduce it or else to •counteract some force tending to destroy it. This may be a convenient terminology, but that's all it is—terminology. The fact remains that in some cases (though only a minority) the continuance of the conditions that produced an effect is necessary for the continuance of the effect.

Is it strictly necessary that a cause should precede—by ever so short an instant—the production of its effect? For my present purposes this doesn't matter. There certainly are cases where the effect follows with no interval that we can detect; and when there is an interval we can't tell how

many intermediate links, imperceptible to us, may fill it up. But even if an effect *can* start simultaneously with its cause, this doesn't affect the view of causation that I am defending. Whether or not the cause and its effect must be successive, •it's the beginning of a phenomenon that implies a cause, and •causation is the law of the succession of phenomena. If these ·two· axioms are granted, we can drop the words 'antecedent' and 'consequent' as applied to cause and effect, though I don't see any need to do so. I have no objection to defining a *cause* as an assemblage of phenomena such that, when it occurs, some other phenomenon invariably starts or has its origin. It doesn't matter whether the effect •coincides with the last of its conditions, or •immediately follows it. It doesn't *precede* it; and when we are wondering which of two coexistent phenomena is the cause and which the effect, we rightly regard the question as answered if we can ascertain which of them came first.

§8. . . . .A single phenomenon is often seen to be followed by several different sorts of effects that happen simultaneously. . . .

- •The sun produces the motions of the planets, daylight, and heat.
- •The earth causes the fall of heavy bodies, and the phenomena of the magnetic needle.
- •A crystal of galena [lead sulphide] causes the sensations of hardness, weight, cubic shape, gray colour, and many others between which we can trace no interdependence.

The terminology of 'properties' and 'powers' is specially adapted to this sort of case. When a single phenomenon is followed. . . .by effects of radically different kinds, it's usual to say that each different sort of effect is produced by a different 'property' of the cause. Thus we distinguish the gravitational 'property' of the earth from its magnetic 'property'; the gravitational, light-making and heat-making 'properties' of the sun; a crystal's 'properties' of colour, shape, weight, and hardness. These are mere phrases: they don't explain anything or add anything to our knowledge of the subject; but considered as abstract names denoting the connection between an object and the different effects it produces, they're a powerful instrument of abridgment and of the acceleration of thought that abridgment brings about.

All this leads to a conception that we'll find to be important, namely that of a permanent cause, or original [see Glossary] natural agent. A number of permanent causes have existed for as long as the human race has, and indeed longer—probably vastly longer. The sun, the earth, and the planets are such permanent causes, as are their various constituents—air, water, and other simple or compound substances of which nature is made up. These have existed, and their effects have taken place, from the very beginning of our experience. But we can't explain the origin of the permanent causes themselves. Why *these* particular natural agents and no others existed originally, why they occur in such-and-such proportions, why they are distributed in such-and-such a way throughout space—we don't know the answers to any of this. Furthermore, we can't discover anything regular in the distribution itself. Given how these causes or agents are distributed in •one part of space, there is no uniformity or law that will let us conjecture what the distribution is •elsewhere. So the coexistence of this and that primeval cause is a mere casual coincidence, so far as we are concerned; so we don't classify as a case of causation or a law of nature any regularity (of following or coexisting) between the effects of this one and the effects of that one. We have no basis for expecting such regularities, except where we have direct evidence about how the relevant

natural agents—the things on whose properties the regularities ultimately depend—are distributed in space. These permanent causes aren't always objects; they are sometimes events, i.e. periodical cycles of events (that's the only sense in which events can be 'permanent'). The earth is a permanent cause or primitive natural agent, and so is its rotation. It's a cause that has always produced. . . .the succession of day and night, the ebb and flow of the sea, and many other effects; and because we can't assign a cause for the rotation itself (except by guessing!), it is entitled to be classified as a primeval ·or original· cause. But it's only the *origin of* the rotation that is mysterious to us; once the rotation has begun, its continuance is accounted for by •the first law of motion (that of the permanence of rectilinear motion once it has been started) combined with •the gravitation of the parts of the earth toward one another. [In that paragraph, 'produced the succession of day and night' replaces Mill's 'produced (by the aid of other necessary conditions) the succession of day and night'. Throughout §8, almost every statement about x causing y includes a clause about other necessary conditions; these clauses are omitted here in the interests of brevity.]

All phenomena that begin to exist, i.e. all but the primeval causes, are immediate or remote effects those primitive facts or of some combination of them. Throughout the known universe no thing comes into existence and no event happens that isn't connected by a uniformity or invariable sequence with one or more phenomena that preceded it; so that it

will happen again as often as those phenomena occur again and no counteracting cause coexists. These antecedent phenomena were connected in a similar way with some that preceded them,. . . . and so on until we reach our limit, the properties of one or more primeval causes. The whole of the phenomena of nature were therefore the necessary—i.e. the unconditional—consequences of some former collocation [see Glossary] of the permanent causes.

The state of the whole universe at any instant, I believe, is the consequence of its state at the previous instant; so that someone who knew

> •all the agents that exist right now,
> •their collocations in space, and
> •all their properties, i.e. the laws of their agency,

could predict the whole history of the universe from now on, unless some new volition of a power capable of controlling the universe should take over. [How did *volition* get into the picture? Mill is talking about a change in the *basic* causal organisation of the universe, and is assuming that if that were to change it would have to be because God—'a power capable of controlling the universe'—decided to change it.][1] [Mill adds the somewhat isolated remark that if any one total state of the universe came around a second time, the whole history of the universe would repeat itself for ever, 'like a circulating decimal'. Then he gets back on track:] The whole series of events in the history of the universe, past and future, is intrinsically capable of being constructed *a priori* by anyone who is acquainted with the

---

[1]   In this footnote Mill mentions those who think that human volition constitutes an exception to the determinist thesis that whatever happens is caused to happen. He says that he'll deal with this thoroughly in VI.2, and right now will make just one point:] These metaphysicians base their objection ·to determinism· on the claim that it conflicts with our consciousness. I think they mistake the proposition that consciousness testifies against. If they look into themselves carefully, they'll find that what their consciousness objects to is the thesis that human actions and volitions are *necessary* in the everyday sense of that term. I agree with them about that. But the statement 'A person's actions necessarily follow from his character' really means what is meant in *any* statement about causation, namely that •the person invariably *does* act in conformity to his character, and •that anyone who thoroughly knew his character could predict with certainty how he would act in any supposable case. They probably wouldn't find *this* contrary to their experience or revolting to their feelings. And no-one claims more than this, except for Asiatic fatalists.

original distribution of all natural agents, and with all their properties—i.e. the laws of succession existing between them and their effects. Of course this would require superhuman powers of combination and calculation, even for someone who knew all the original facts.

§9. Because everything that occurs is determined by laws of causation and collocations of the original causes, it follows that coexistences among effects can't be themselves the subject of any similar set of laws distinct from laws of causation. There are uniformities of coexistence and succession among effects; but these must all result from the identity or the coexistence of their causes; if the causes didn't coexist, nor could the effects. And these causes being effects of still earlier causes, and these of others,. . . . until we reach the primeval causes, it follows that (apart from effects that can be traced back to a single cause) the coexistences of phenomena can't be universal unless the coexistences of their primeval causes can be reduced to a universal law; but we have seen that they can't. So there are no original and independent—i.e. no *unconditional*—uniformities of coexistence, between effects of different causes; if they coexist, it's only because their causes have happened to coexist. The only independent and unconditional coexistences that are invariable enough to have a claim to be laws are between different and mutually independent effects of a single cause, i.e. between different properties of the same natural agent. This portion of the laws of nature will be treated of in the chapter 22.2 .

§10. Since the first edition of this work, the sciences of physical nature have made a great advance in generalisation, through the doctrine of the conservation or persistence of force. Building and laying out this imposing edifice of theory has for some time been the principal occupation of the most systematic physicists. It consists of two stages: **(1)** one

consisting of ascertained fact, and **(2)** one containing, along with some fact, a large element of hypothesis.

(1) It is proved by numerous facts, some experimental, some informal, that agencies that had been regarded as distinct and independent sources of force—heat, electricity, chemical action, nervous and muscular action, momentum of moving bodies—are interchangeable with one another in definite and fixed quantities. It had long been known that these dissimilar phenomena had the power, under certain conditions, to produce one another; what is new in the theory is a more accurate estimation of what this production consists in. What happens is that phenomena of one kind disappear and are replaced by phenomena another kind, and that there is an equivalence in quantity between the phenomena that have disappeared and the ones that have replaced them; so that if the process is reversed, the exact same quantity that had disappeared will re-appear. For example, the amount of heat that will raise the temperature of a pound of water one degree centigrade will, if used in the expansion of steam, lift a weight of 772 pounds one foot, or a weight of one pound 772 feet; and the same quantity of heat can by certain means be recovered through the expenditure of exactly that amount of mechanical motion.

The establishment of this comprehensive law has led the scientific world to change how it speaks about what are called the 'forces of nature'. Before this correlation between very different phenomena had been discovered, their unalikeness had caused them to regarded as upshots of such-and-such distinct forces. Now that they are known to be convertible into one another without loss, they are spoken of as all the results of one single force, showing itself in different ways. This force (it is said) can only produce a limited and definite quantity of effect, but it always *does* produce that definite quantity; and produces it (according

to circumstances) in one or another of the forms, or divides it among several, but so as always to make up the same sum; and no one of the manifestations can be produced except by the disappearance of the equivalent quantity of another, which in its turn, in appropriate circumstances, will re-appear undiminished. This mutual interchangeability of the forces of nature according to fixed numerical equivalents is the part of the new doctrine that rests on unchallengeable fact. (The judgments about equivalents are based on a scale of numerical equivalents established by experiment.)

An indefinite and perhaps immense interval of time may elapse between the disappearance of the force in one form and its re-appearance in another. A stone thrown up into the air with a given force and falling back immediately will, by the time it reaches the earth, recover the exact amount of mechanical momentum which was expended in throwing it up (minus a small portion of motion given to the air). But if the stone lands on a high ledge it may not fall back for years, perhaps ages, and until then the force used in raising it is temporarily lost, being represented only by what the language of the new theory calls 'potential energy'. The coal embedded in the earth is considered by the theory as a vast reservoir of force that has remained dormant for many geological periods, and will remain dormant until by being burned it gives out the stored-up force in the form of heat. . . . This means simply that when the coal does at last. . . .generate a quantity of heat (transformable like all other heat into mechanical momentum and the other forms of force), this heat is the re-appearance of a force derived from the sun's rays, expended myriads of ages ago in the growth of the organic substances that were the material of the coal.

(2) The theory of conservation of force has a part that is a combination of fact and hypothesis. Briefly, it is as follows:

The conservation of force is really the conservation of motion. In each interchange between forms of force it is always motion that is transformed into motion.

This requires the assumption of motions that are hypothetical. The supposition is that •there are molecular motions that appear to our senses only as heat, electricity, etc.— oscillations, invisible to us, among the minute particles of bodies; and that •these molecular motions can be changed into molar motions (motions of masses), and vice versa. We do have positive evidence of the existence of molecular motion in these manifestations of force. In chemical reactions, for instance, the particles separate and form new combinations, often with a great visible disturbance of the mass. And with heat the evidence is equally conclusive, since heat expands bodies (i.e. causes their particles to move apart), and enough heat changes them from solid to liquid, or from liquid to gaseous. Again, the mechanical actions that produce heat—friction, and the collision of bodies—must from the nature of the case produce a shock, i.e. an internal motion of particles, which we often find is so violent as to break them apart. Such facts are thought to justify the conclusion that we were wrong when we thought that heat causes the motion of particles, and that really the motion of particles causes heat; the original cause of both being the earlier motion—molar or molecular, collision of bodies or burning of fuel—that formed the heating agency. This conclusion already contains an hypothesis; but at least the supposed cause, the internal motion of molecules, is a *vera causa* [see Glossary]. But in order to reduce the conservation of force to conservation of motion, it was necessary to attribute to motion the heat propagated through apparently empty space from the sun. This required the supposition (already made for the explanation of the laws of light) of a superfine *ether* pervading space; we can't feel it, but it must have the

property that constitutes matter, namely *resistance*, because waves are propagated through it by an impulse from a given point. This ether must be supposed (and the theory of light doesn't require this) to penetrate into the minute crevices in all bodies. The story goes like this:

> Vibratory motion in the heated mass of the sun is passed on to the particles of the surrounding ether, and through them to the particles of the same ether in the gaps and crevices of terrestrial bodies; and this is done with enough mechanical force to make the particles of those bodies vibrate strongly enough to make the bodies expand and create the sensation of heat in sentient creatures.

This is all hypothesis, but I'm not expressing doubt as to it legitimacy as hypothesis. It seems to follow from this theory that *force* can and should be defined as *matter in* **motion**. But this can't be right because, as we have seen, the matter doesn't have to be actually moving. It isn't necessary to suppose that the motion manifested when the coal burns is actually taking place among the molecules of the coal during its time in the earth;[1] certainly not in the stone at rest on the high ledge onto which it has been thrown. The true definition of *force* must be **potentiality of motion**; and what the doctrine. . . .amounts to is not that •there is at all times the same quantity of actual motion in the universe, but that •the possibilities of motion are limited to a definite quantity that can't be added to and can't be exhausted; and that all actual motion is a draft on this limited stock [this is a metaphor based on the idea of a bank-draft saying 'Withdraw £n from account number. . .'.]. It needn't all have existed *ever* as actual motion. There's a vast amount of potential motion in the

universe in the form of gravitation, and it would be a great abuse of hypothesis to suppose that *that* was stored up by the expenditure of an equal amount of actual motion in some former state of the universe! Nor does the motion produced by gravity take place, as far as we know, at the expense of any other motion of any kind.

If we adopt this theory as a scientific truth, thus accepting its change in our conception of the most general physical agencies, does this require •any change in the view I have taken of causation as a law of nature? As far as I can see, •none whatever. The manifestations that the theory regards as modes of motion are just as distinct and separate phenomena when attributed to •a single force as when attributed to •several. Whether the phenomenon is called a transformation of force or the generation of one, it has its own set(s) of antecedents with which it is connected by invariable and unconditional following; and that set, or those sets, of antecedents are its cause. [Mill now embarks on a long discussion of how he should word his theory in the light of the conservation theory. He refers in friendly terms to a detailed discussion of this by Bain, to whom he attributes the conclusion that:]

> In the assemblage of conditions that constitutes the cause of a phenomenon we must distinguish two elements—•the presence of a force and •the collocation or position of objects that is required for the force to undergo the particular transmutation that constitutes the phenomenon.

[Mill accepts this, sort of, but argues at great length that this doesn't really require him to alter any of his formulations. He accepts and indeed insists that the cause of any change

---

[1]   I understand that the accredited authorities *do* suppose that molecular motion, equivalent in amount to what will be manifested when the coal burns, *is* actually taking place during the whole of that long interval, not in the coal but in the oxygen that will then combine with it. You can see how purely hypothetical this supposition is; and I venture to say that it is unnecessarily and extravagantly hypothetical.

must include a change: 'To produce a bonfire there must not only be fuel and air and a spark, which are collocations [see Glossary], but chemical action between the air and the materials, which is a force.' But the insistence on motion, he says, is simply wrong unless we include potential motion; and we must be careful about what we mean by that: 'The force said to be laid up and merely potential is no more a really existing thing than any other properties of objects are really existing things. The phrase ·"potential force" or "potential motion"· is a mere linguistic device that is convenient for describing the phenomena.' He concludes:]

We thus see that no new general conception of causation is introduced by the conservation theory. The indestructibility of force doesn't interfere with the theory of causation any more than the indestructibility of matter does.... It only enables us to understand better than before the nature and laws of some of the sequences.

This better understanding, however, lets us join Bain in accepting the expenditure or transfer of energy as one of the tests for distinguishing causation from mere concomitance. If the effect being explained includes matter's beginning to move, then any of the objects present that has lost motion has contributed to the effect; and this is the true meaning of the thesis that the cause is the one of the antecedents that exerts active force.

§11. This is the place to discuss a rather ancient doctrine about causation that has been revived in recent years and now shows more signs of life than any other theory of causation that conflicts with the one I have been defending.

According to the theory in question, the only cause of phenomena is *mind*, or more exactly *will*. The type [see Glossary] of causation, as well as the only source for our idea of it, is our own voluntary agency. The theory's friends say:

Our voluntary agency is our only direct evidence of causation. We know that we can move our bodies. Regarding the phenomena of inanimate nature, all we have other direct knowledge of is what happens before what. But in our voluntary actions we're conscious of •power before we have experience of •results. An act of volition, whether or not followed by an effect, is accompanied by a consciousness of effort, 'of force exerted, of power in action, which is necessarily causal or causative' [quoted from Francis Bowen]. This feeling of energy or force, inherent in an act of will, is knowledge *a priori*; assurance before experience that we have the power to cause effects. So volition is more than an •unconditional antecedent; it is a •cause in a different sense of 'cause' from that in which physical phenomena are said to cause one another. It is an efficient cause [see Glossary].

It's easy to move from this to the doctrine that volition is the *only* efficient cause of all phenomena. 'It is inconceivable that dead force could continue unsupported for a moment beyond its creation. We cannot even *conceive of* change or phenomena without the energy of a mind' [quoted from R. H. Hutton]. And another writer of the same school says: 'The word "action" itself has no real significance except when applied to the doings of an intelligent agent. Let anyone conceive, if he can, of any power, energy, or force inherent in a lump of matter' [Bowen again]. Phenomena may seem to be produced by physical causes but they are really produced, say these writers, by the immediate agency of mind. Everything that doesn't proceed from a human (or, I suppose, an animal) will proceed, they say, directly from divine will. The earth is not moved by the combination of a •centripetal and a •projectile force; this is a mere way of speaking that helps to make our conceptions easier. The earth (they say) is moved by the

direct volition of an omnipotent Being, on a path coinciding with the one that we deduce from the hypothesis of •these two forces.

As I have so often said, the general question of the existence of •efficient causes doesn't fall within the limits of my present subject; but a theory that represents •them as capable of being known by humans, and passes off as efficient causes what are only physical or phenomenal causes, belongs to logic as much as to metaphysics, and is a fit subject for discussion here.

As I see it, a volition isn't an efficient cause but simply a physical cause. Our will 'causes' our bodily actions in exactly and only the sense in which cold causes ice, or a spark causes an explosion of gunpowder. The volition, a state of our mind, is the antecedent; the motion of our limbs in conformity to the volition is the consequent. This sequence is not a subject of direct consciousness in the sense intended by the theory. The antecedent and the consequent are indeed subjects of consciousness; but the connection between them is a subject of experience. Our consciousness of the volition doesn't contain in itself any *a priori* knowledge that the muscular motion will follow. If our nerves of motion were paralysed. . . .and had been so all our lives, I don't see the slightest reason to think that we would ever (unless told by other people) have known anything of volition as a physical power, or been conscious of any tendency in feelings of our mind to produce motions of our body. . . . Would we in that case have had the physical feeling that I suppose these writers mean by 'consciousness of effort'? I don't see why not, because that physical feeling is probably a state of nervous sensation beginning and ending in the brain, without involving the motor apparatus; but we certainly wouldn't have called it anything like 'effort'. . . . If we were conscious of this sensation, we'd have been conscious of it, I think, only as a kind of uneasiness accompanying our feelings of desire.

Hamilton argues well against the theory in question, thus:

'It is refuted by the consideration that between •the overt fact of corporeal movement that we know and •the internal act of mental determination that we also know, there intervenes a series of intermediate agencies of which we know nothing; so we *can't* be conscious of any causal connection between the volition and the movement, as this hypothesis asserts. . . . A paralytic learns after the volition that his limbs don't obey his mind; and it's only after the volition that the healthy man learns that his limbs do obey the mandates of his will.'[1]

Those I am arguing against have never produced, and don't claim to produce, any positive evidence that the power of our will to move our bodies would be known to us independently of experience.[2] What they say about this is that

---

[1] . . . .This acute thinker has a theory of causation that is all his own. As far as I know it has never been analytically examined, but I think it can be refuted as completely any one of the false or insufficient psychological theories that strew the ground under his potent metaphysical scythe. (Since writing that I have examined and controverted his theory in my *Examination of Sir William Hamilton's Philosophy*, ch. 16.)

[2] Bowen disagrees: 'The result to be accomplished is preconsidered or meditated, and is therefore known *a priori* or before experience.' This merely says that when we will a thing we have an idea of it. . . .but that doesn't imply a prophetic knowledge that it will happen. You may object: 'The first time we exerted our will, when we had no *experience* of any of our powers, we must have known that we had those powers, because we can't will something that we don't believe to be in our power.' But that's a merely verbal impossibility. We can **desire** something that we don't know to be in our power, and when we find by experience that our bodies move according to our desire *then* we can pass into the more complicated mental state that is termed **will**. . . .

the production of physical events by a will seems to carry its own explanation with it, while the action of matter on matter seems to require something else to explain it, and is even 'inconceivable' unless we suppose that some *will* intervenes between the apparent cause and its apparent effect. So they base their argument on an appeal to ·what they think to be· the inherent laws of our conceptive faculty, mistaking for •the ·innate· laws of that faculty •its acquired habits based on the spontaneous tendencies of its uncultured state. The sequence from •the will to move a limb and •the actual motion is one of the most direct and instantaneous of all the sequences we observe, and is familiar to every moment's experience from our earliest infancy. It is more familiar to us than any succession of events exterior to our bodies, and especially more so than any other case of the apparent beginning (as distinguished from the mere passing on) of motion. Our mind naturally tends to be constantly trying to help its conception of unfamiliar facts by assimilating them to familiar ones. And so our voluntary acts. . . .in the infancy and early youth of the human race are spontaneously taken as the type [see Glossary] of causation in general, and all phenomena are supposed to be directly produced by the will of some sentient being. I shan't describe this primitive idol-worship in the words of Hume or of any of his followers; rather, I'll take the words of a religious metaphysician, Reid, in order to bring out that all competent thinkers are unanimous on this topic.

·Start of quotation from Reid·

'When we turn our attention to external objects and begin to exercise our rational faculties about them, we find that there are some motions and changes in them that we have power to produce, and that many must have some other cause. Either the objects must have life and active power, as we have, or they must be moved or changed by something that has life and active power, as external objects are moved by us.

'Our first thoughts seem to be that the objects in which we perceive such motions have understanding and active power as we have. "Savages", says the Abbé Raynal, "wherever they see motion that they can't account for, postulate a soul." All men can be considered as "savages" in this respect, until they can be taught and can use their faculties better than savages do. . . .

'Raynal's remark is sufficiently confirmed both from fact and from the structure of all languages.

'Primitive nations really do believe that the sun, moon, and stars, the earth, sea, and air, and fountains and lakes have understanding and active power. Savages find it natural to bow down to these things and beg for their favour, as a kind of idolatry.

'All languages carry in their structure the marks of their having been formed at a time when this belief prevailed. The division of verbs and participles into •active and •passive, which is found in all languages, must have been originally intended to distinguish what is really active from what is merely passive; and, in all languages we find active verbs applied to the sorts of things in which, according to Raynal, savages think there is a soul.

'Thus we say "The sun rises and sets", "The moon changes", "The sea ebbs and flows", "The winds blow". Languages were formed by men who believed these objects to have life and active power in themselves, and so ·for them· it was proper and natural to report such motions and changes with active verbs.

'There's no surer way of tracking what nations believed before they had records than by the structure of their language; despite the changes produced in it by time, a language will

always bear traces of the thoughts of those who invented it. When we find the same beliefs indicated in the structure of *all* languages, those beliefs must have been common to the whole human species when languages were being invented.

'When a few people with superior intellectual abilities find leisure for speculation, they begin to do science [Reid writes: 'to philosophise'], and they soon discover that many of the things they used to regard as thinking and active are really lifeless and passive. This is a very important discovery. It elevates the mind, frees men from many ignorant superstitions, and opens the door to further discoveries of the same kind.

'As science advances, life and activity in natural objects retreats, leaving the objects dead and inactive. We find that rather than •moving voluntarily they •are moved necessarily; rather than •acting they are •acted-upon; and nature appears as one great machine in which one wheel is turned by another, that by a third; and the scientist doesn't know how far back this necessary sequence may reach.' [Reid, *Essays on the Active Powers of Man* IV.3.]

·END OF QUOTATION FROM REID·

So there's a spontaneous tendency of the intellect to explain all cases of causation by assimilating them to the intentional acts of voluntary agents like itself. This is the instinctive philosophy of the human mind in its earliest stage, before it has become familiar with invariable sequences other than those between volitions and voluntary acts. As the notion of fixed laws of succession among *external* phenomena gradually takes hold, the propensity to explain all phenomena in terms of voluntary agency slowly gives way. But the suggestions of daily life continue to be more powerful than those of scientific thought, so the original instinctive philosophy maintains its ground in the mind, underneath the growths obtained by cultivation [see Glossary], and keeps

up a constant resistance to their driving their roots deep into the soil. The theory I'm attacking is fed by that substratum. Its strength lies not in argument but in its link with an obstinate tendency of the infancy of the human mind.

There's plenty of evidence that this tendency isn't the result of an inherent mental law. The history of science right from the beginning shows that mankind haven't been unanimous in thinking either that **(i)** the action of matter on matter *wasn't* conceivable or that **(ii)** the action of mind on matter *was*. To some thinkers, ancient and modern, **(ii)** has seemed much more inconceivable than **(i)**. Sequences that are entirely physical and material, as soon as they became familiar, came to be thought perfectly natural, and were regarded not only as not needing to be explained but as being able to explain other sequences—and even of serving as the ultimate explanation of things in general.

One of the ablest recent supporters of the volitional theory [Hutton] has provided an historically true and philosophically sharp account of the Greek philosophers' failure in physical inquiry—an account in which, it seems to me, he unconsciously depicts his own state of mind:

> 'Their stumbling-block concerned the nature of the evidence they expected for their conviction. . . They hadn't grasped that they mustn't expect to understand the •processes of external causes but only their •results; so the whole physical philosophy of the Greeks was an attempt to identify mentally the effect with its cause, to probe for a •connection that was not only necessary but natural—meaning that •it would carry within itself some reason why this antecedent should produce this consequent—and they confined themselves to looking such reasons.'

That is, they weren't content merely to know that one phenomenon x was always *followed* by another y; they thought

that science's true aim was to perceive something in x's nature from which they could have known or presumed *previous to trial* that it would be followed by y. . . . To complete his statement of the case, the quoted writer should have added that these early speculators not only •had that aim but •thought they had achieved it. The writer can see plainly that this was an error, because he doesn't believe that any relations between material phenomena can account for their producing one another; but the Greeks' persistence in this error shows that their minds were in a very different state; the assimilation of physical facts to other physical facts gave them the kind of mental satisfaction that we connect with the word 'explanation'. . . . When Thales and Hippo held that moisture was the universal cause and external element of which all other things were merely sensible manifestations; when Anaximenes said the same thing about air, Pythagoras about numbers, and the like; they all thought they had found a real explanation and were content to settle for this as ultimate. The ordinary sequences of the external universe seemed to them. . . .to be inconceivable without the supposition of some universal agency to connect the antecedents with the consequents; but they didn't think that mental volition was the only agency that fulfilled this requirement. Moisture, or air, or numbers, carried to their minds a precisely similar impression of making intelligible what was otherwise inconceivable, and gave the same full satisfaction to the demands of their conceptive faculty.

It wasn't only the Greeks who 'wanted to see some reason why the physical antecedent should produce this particular consequent'. . . . Among modern philosophers, Leibniz laid it down as a self-evident principle that *all* physical causes must contain in their own nature something making it intelligible that they should be able to produce the effects that they do produce. Far from admitting volition as the only kind of cause that carries internal evidence of its own power, and as the real bond of connection between physical antecedents and their consequents, he demanded some naturally and intrinsically efficient [see Glossary] physical antecedent as the bond of connection between volition itself and its effects. He clearly refused to admit the will of God as a sufficient explanation of anything but miracles; and insisted on finding something that would account better for the phenomena of nature than a mere reference to divine volition.

And the action of mind on matter (which, we're being told, needs no explanation and itself explains all other effects) has seemed to some thinkers to be itself the grand inconceivability. This was the difficulty the Cartesians were trying to solve with the system of 'occasional causes'. They couldn't conceive that thoughts in a mind could produce movements in a body, or that bodily movements could produce thoughts. They couldn't see any necessary connection, any *a priori* relation, between a motion and a thought. And their insistence—greater than any other philosophical school before or since—that their own minds were measure of all things led them to refuse on principle to believe that Nature had done what they couldn't see any reason why she *must* do, so they said it was impossible that a material and a mental fact could be causes one of another. They regarded them as mere 'occasions' on which the real agent, God, thought fit to exert his power as a cause. When a man wills to move his foot, they said, it's not his will that moves it; God moves it on the occasion of the man's will. And when they looked more carefully into the action of matter on matter they found *this* inconceivable too, and therefore (according to their logic) impossible. The *deus ex machina* [see Glossary] was ultimately called in to produce a spark on the occasion of a flint and steel coming together, or to break an egg on the occasion of its falling on the ground.

All this shows that mankind in general is disposed not to be satisfied with knowing that one fact is invariably antecedent and another consequent, but to look for something that may seem to *explain* their being so. But we also see that this demand can be completely satisfied by a purely physical agency, provided it's much more familiar than what it is invoked to explain. To Thales and Anaximenes it seemed inconceivable that the antecedents that we see in nature should *produce* the consequents, but perfectly natural that water or air should produce them. The writers I am opposing in this section declare this to be inconceivable, but they can conceive that mind or volition is an efficient cause; while the Cartesians couldn't conceive even that, but briskly declared that the only conceivable mode of production of any fact whatever is the direct agency of an omnipotent being; all of which is further evidence for something that finds new confirmation in every stage of the history of science—namely that •what people can conceive and what they can't is very much an affair of accident, and depends entirely on their experience and their habits of thought; that •by cultivating the required associations of ideas people can make themselves unable to conceive any given thing, and make themselves able to conceive most things, however inconceivable these may at first appear; and that •the facts in each person's mental history that determine what is or isn't conceivable to him also determine which sequences in nature will appear to him so natural and plausible as to need no other proof of their existence, and to be evident by their own light independently of experience and of explanation.

By what rule can we decide between one theory of this sort and another? The theorists don't direct us to any external evidence; each appeals to his own subjective feelings.

One (X) says:

> The succession C, B appears to me more natural, conceivable, and intrinsically credible than the succession A, B; so you are wrong in thinking that B depends on A; I am certain—though I can't give any other evidence of it, that C comes between A and B, and is the real and only cause of B.

Another (Y) answers:

> The successions C, B and A, B appear to me equally natural and conceivable, or the latter more so than the former. A is quite capable of producing B without any other intervention.

A third (Z) says:

> Like X I can't conceive that A can produce B; but ·I don't share his view that C produces B, because· I find the sequence D, B more natural than C, B, so I prefer my D theory to the C theory.

The only universal law operating here is the one saying that each person's conceptions are governed and limited by his individual experiences and habits of thought! We're justified in saying of all three, what each of them already believes of the other two, namely that they exalt into an original law of the human intellect and of outward nature one particular sequence of phenomena that they find more natural and more conceivable than other sequences, only because it is more familiar. And I apply this judgment to the theory that volition is an efficient cause.

Before leaving this subject I must mention the additional fallacy contained in the inference from this theory that because volition is an efficient cause, therefore it is the only cause and the direct agent in producing even what is apparently produced by something else. Volitions are not known to produce anything directly except activity in the nerves, because the will influences even the muscles

only through the nerves. Suppose we grant that every phenomenon has an efficient [see Glossary] cause and not merely a phenomenal cause, and that volition in the case of the special phenomena that are known to be produced by it *is* that efficient cause; are we therefore to say (as these writers do) that because we know of no other efficient cause, and oughtn't to assume one without evidence, *there is* no other, and volition is the direct cause of all phenomena? [Mill scornfully dismisses this. If our volition is an efficient cause, it's the only one we *can* be conscious of because 'it is the only one that exists within ourselves'; and it's absurd to infer that volition is the only efficient cause in the universe. Mill likens this to the inference that because we know for sure that there is life on this planet, we can infer that there is life on every heavenly body. He concludes:] I ascribe to certain other creatures a life like my own, because they show the same sort of signs of it as I do. . . . Earth, fire, mountains, trees, are remarkable agencies, but their phenomena don't conform to the same laws as my actions do, so I don't attribute animal life to them. But the supporters of the volition theory ask us to infer that volition causes everything simply because it causes one particular thing; although that one thing. . . .is utterly special, its laws being enormously unlike those of any other phenomenon, organic or inorganic.

·Supplementary note to chapter 5·

[In this densely learned four-page note, Mill responds to critics who accuse him of misrepresenting the views of Thales and Anaximines, and of Descartes and Leibniz. (He points out that he didn't mention Descartes, only the Cartesians.) Mill's response opens with all cannons firing: 'A greater quantity of historical error has seldom been comprised in a single sentence.' Regarding the ancient philosophers he adduces more evidence, and also shows that his critics'

rival views about Thales and Anaximines are based on misreadings of ancient texts, and ignorance of what Aristotle and others thought about who had had what theory.

[Mill side-tracks at some length into Aristotle's views about causation in the natural world, mainly so as to highlight two aspects of them. **(a)** Aristotle held that *chance* is an efficient cause (though not of everything). We now know that this was an error, Mill writes, but it wasn't a disreputable one:] Chance had as good a claim to real existence as many other of the mind's abstract creations; it had been given a name, and why should it not be a reality? **(b)** The parts of nature that Aristotle regards as representing evidence of *design* are the uniformities—the phenomena that conform to laws. The common interpretation of nature—we could call it the instinctive, religious interpretation of nature—is the reverse of this. The events in which men spontaneously see the hand of a supernatural being are the ones that they *can't* bring under physical laws. Events that they can clearly connect with physical causes, and especially ones they can predict, seem to them not to bear so obviously the mark of a divine will (though they may think that God is responsible for those too). . . . Some eminent writers on natural theology [see Glossary] . . . .think that although design is present everywhere, the irresistible evidence of it is to be found not in the laws of nature but in the collocations [see Glossary], i.e. in the part of nature that shows no signs of any law. A few properties of dead matter might, they think, conceivably account for the regular and invariable succession of effects and causes; but they see proof of a divine providence in the way the different kinds of matter have been so placed as to promote beneficent ends. [Very roughly: It might be possible to explain how your body works without bringing in God, but we have to appeal to God to explain how there comes to be such a material configuration as your body.]

[Mill shows that he was certainly right in what he wrote about the views of Leibniz. We needn't spend time on this, except to note the tone of anger:] The critics say that what Leibniz found to be inconceivable was not •that mind acts on matter but •how it does so. This is an abuse of the privilege of writing confidently about authors without reading them! If my critics knew *anything* about Leibniz they would know that for him 'the inconceivability of *how*' and 'the impossibility of *the thing*' were equivalent expressions. . . .

[Regarding the Cartesians, Mill focuses on Malebranche, the best known Cartesian and the chief expositor (though not the inventor) of 'the system of occasional causes', and easily shows that he (Mill) was right about his views. He concludes: 'If Malebranche hadn't believed in an omnipotent Being, he would have held all action of mind on body to be a demonstrated impossibility.'

[There's a further half-page of tussle with the critics, but we can safely by-pass this efficient operation of garbage-removal.]

Chapter 6. The composition of causes

§1. To complete the general notion of causation. . . ., one distinction still remains to be pointed out. It is so radical and so important that it requires a chapter to itself.

We're now familiar with the case in which several agents or causes jointly produce an effect. It is indeed the usual case: very few effects are produced by just one agent. Suppose that two agents, operating jointly, are followed (under certain collateral conditions) by a given effect. If either of them had operated alone (under those same conditions), some effect would probably have followed, an effect •different from the joint effect of the two and •more or less dissimilar to it. When we know what would be the effect of each cause acting alone, we can often arrive deductively—i.e. *a priori*—at a correct prediction of what will arise from their joint agency. We can do this just so long as

the law expressing the effect of each cause acting by itself also correctly expresses that cause's part of the effect that follows from the two together.

That's how things stand in the important class of phenomena commonly called 'mechanical', namely the phenomena of the communication of •motion from one body to another (or of •pressure, which is tendency to motion). In this class of cases it never happens that one cause defeats or frustrates another; both have their full effect. If a body is propelled by one force tending to drive it to the north and by another to the east, it is caused to move in a given time exactly as far in both directions as the two forces would separately have carried it. It ends up precisely where it would have arrived if it had been acted on first by one of the two forces and then by the other. In dynamics this law of nature is called the principle of the composition of forces; and in imitation of that well-chosen label I shall give the name 'composition of causes' to the principle that is exemplified whenever the joint effect of several causes is identical with the sum of their separate effects.

This principle doesn't prevail in all parts of nature. The chemical combination of two substances produces, as is well known, a third substance with properties different from

those of either of the two substances separately. . . . No trace of the properties of hydrogen or of oxygen is observable in their compound, water. The taste of lead acetate isn't the sum of the tastes of its component elements, acetic acid and lead or its oxide; nor is the colour of blue vitriol a mixture of the colours of sulphuric acid and of copper. This explains why mechanics is a deductive or demonstrative science, and chemistry is not. In mechanics we can compute the effects of combinations of causes from the laws that we know to govern those causes when acting separately, because they conform to the same laws when in combination that they conform to when acting separately. . . . Not so in the phenomena that are the special subject of the science of chemistry. There most of the uniformities the causes conform to when separate cease altogether when they are conjoined, and we can't (at least in the present state of our knowledge) foresee what result will follow from any new combination until we have tried the specific experiment.

If this is true of chemical combinations, it's even more true of the far more complex combinations of elements that constitute organised bodies—combinations out of which arise the extraordinary new uniformities called the 'laws of life'. All the parts of organised bodies are similar to the parts of inorganic things, and have themselves existed in an inorganic state; but the phenomena of life that result from the juxtaposition of those parts in a certain manner are utterly unlike all the effects that would be produced by the action of the component substances acting as mere physical agents. No imaginable knowledge of a living body's ingredients, however wide-ranging and complete, could enable us to predict the events of the living body itself from our knowledge of the separate actions of its elements. The

tongue, for instance, is composed of gelatine, fibrin, and other products of the chemistry of digestion; but from no knowledge of the properties of those substances could we ever predict that the tongue could taste, unless gelatine or fibrine could themselves taste, for no elementary fact can be in the conclusion that wasn't in the premises.

Thus the combined action of several causes can belong to either of two types, from which arise two ways in which laws of nature can conflict or interfere with one another. Take a case where at a given point of time and space there are two or more causes which, if they acted separately, would produce effects contrary to—or at least conflicting with—each other, one of them tending to undo some or all of what the other tends to do. Examples:

> •The expansive force of the gases generated by the ignition of gunpowder tends to launch a bullet toward the sky, while its gravity tends to make it fall to the ground.
> •A stream running into a reservoir at one end tends to raise its level higher and higher, while a drain at the other end tends to empty it.

In cases such as these, although the two causes exactly annul one another the laws of both are still fulfilled; the effect is the same as if the drain had been open for half an hour first, and the stream had flowed in for half an hour afterward.[1] Each agent produces the same amount of effect as if it had acted separately, though the contrary effect occurring at the same time obliterated it as fast as it was produced. Here, then, are two causes producing by their joint operations an effect that •at first seems quite unlike the effects they produce separately but •on examination proves to be really the sum of those separate effects. . . .

---

[1]   Strictly speaking, in the second case the draining would be a little slower because there would be less pressure to create it, but that doesn't affect the truth of what I'm saying, because that would involve a change in the conditions under which the drain was acting.

So there's one kind of mutual interference of laws of nature in which, even when the joint causes annihilate each other's effects, each exerts its full efficacy according to its own law as a separate agent. In the other kind of case, the agencies that are brought together cease entirely, and a totally different set of phenomena arise—e.g. when two liquids are mixed in certain proportions they instantly become not a larger amount of liquid but a solid mass.

§2. This difference between. . . .•laws that work together without alteration and •laws which, when called on to work together, cease and give place to others, is one of the fundamental distinctions in nature. The former case (the composition of causes) is the usual one; the other is always special and exceptional. There are no objects that don't obey the principle of the composition of causes with regard to *some* of their effects. For instance, a body retains it weight in all the combinations in which it is placed. The weight of a chemical compound, or of an organised body, is equal to the sum of the weights of the elements composing it. The weight will be lessened if the body is moved further from the centre of attraction, but it will be the same lessening for the compound as for the elements. The component parts of a plant or animal don't lose their mechanical and chemical properties as separate agents when they are spatially inter-related in the special way such that they, as an aggregate whole, acquire physiological or vital properties in addition. Those bodies still obey mechanical and chemical laws, because the operation of those laws isn't counteracted by the new laws that govern the bodies as organised beings. To put that in another way: when two or more causes jointly operate in a way that calls into action new laws with no resemblance to any we can find in the separate operation of the causes, the new laws, while superseding one portion of the previous laws,

may coexist with another portion, and may even compound the effect of those previous laws with their own.

Also, laws that were themselves generated in the second way may generate others in the first. The laws of chemistry and physiology (for example) owe their existence to a breach of the principle of composition of causes, but these *heteropathic* laws, as we might call them, are capable of composition with one another. The causes which by one combination had their laws altered may carry their new laws with them *un*altered into further combinations. So we needn't despair of eventually raising chemistry and physiology to the condition of deductive sciences; for though it's impossible to deduce all chemical and physiological truths from the laws or properties of simple substances or elementary agents, perhaps they are deducible from laws that come into play when these elementary agents are brought together into some moderate number of not very complex combinations. The **laws of life** will never be deducible from the mere laws of the ingredients, but the prodigiously complex **facts of life** may all be deducible from comparatively simple laws of life—which do indeed depend on combinations, but comparatively simple ones. These laws of life may in more complex circumstances be strictly compounded with one another *and* with the physical and chemical laws of the ingredients. We already know enough about the vital phenomena to know of countless cases where they enter into the composition of causes; and the more precisely we study these phenomena the more reason we seem to get for believing that the laws that operate in the simpler combinations of circumstances do in fact continue to be observed in more complex ones. This will be found equally true in the phenomena of mind; and even in social and political phenomena, which are results of the laws of mind. It's with chemical phenomena that the least progress has been made, so far, in bringing the special

laws under general ones from which they can be deduced; but even in chemistry there are many circumstances to encourage the hope that such general laws will eventually be discovered. There's no chance that the different actions of a chemical compound will ever be found to be the sums of the actions of its separate elements; but between the properties of •the compound and those of •its elements there may be some constant relation that would enable us to foresee the sort of compound that will result from a new combination before we have actually tried it, and to judge what sort of elements some new substance is compounded of before we have analysed it. (The relation would of course have to be discovered by a sufficient induction.) The law of definite proportions, first presented in its full generality by [John] Dalton ·in his atomic theory· is a complete solution of this problem so far as •quantity is concerned; and for •quality we already have some partial generalisations suggesting that we may eventually get further. We can know in advance some properties of the kind of compounds that result from combining, in each of the small number of possible propor-tions, any acid with any base. We also have the curious law discovered by Berthollet: two soluble salts mutually decompose one another whenever the new combinations that result produce a compound that is less soluble than either of the original two. . . . Thus it appears that even heteropathic laws—laws of combined agency that aren't derived •by simple addition from the laws of the separate agencies—are in some cases derived •according to some fixed principle from the separate laws. So there may be laws governing the generation of laws from others that are unlike them; and in chemistry these undiscovered laws of the dependence of a compound's properties on the properties of its elements may, together with the laws of the elements themselves, provide the premises by which chemistry is perhaps destined

eventually to be made deductive.

So it seems •that the composition of causes occurs in every class of phenomena; •that as a general rule causes in combination produce exactly the same effects as when acting singly; but •that this rule, though general, isn't universal because in some instances, at some particular points in the transition from separate to united action, the laws change and an entirely new set of effects occur in place of (or in addition to) the effects arising from the separate agency of those same causes; and •that the laws of these new effects are in their turn capable of composition. . . .

§3. That *effects are proportional to their causes* is laid down by some writers as an axiom in the theory of causation. It has been worked hard in reasonings about the laws of nature, though it is burdened with many difficulties and apparent exceptions which much ingenuity has been expended in showing not to be real ones. What truth there is in this 'axiom' is just a special case of the composition of causes— the case where compounded causes are homogeneous, so that their joint effect might be expected to be the sum of their separate effects. ·The 'axiom' is illustrated by this·:

> A force equal to 100lb will raise a certain body a certain distance along an inclined plane; a force equal to 200lb will raise two ·such· bodies the exact same distances; so the effect is proportional to the cause.

But the 200lb force contains two forces each equal to 100lb— forces each of which would raise one of the bodies if it were employed separately. So the fact used to illustrate the 'axiom' results from the composition of causes; it's a mere instance of the general fact that mechanical forces are subject to the law of composition. And it's the same in every other conceivable case. The doctrine of the proportionality of effects to their causes obviously can't apply to cases where

adding something to the cause alters the *kind* of effect. . . . Suppose that the application of a certain quantity of heat to a body merely •increases its size, that a double quantity •melts it, and a triple quantity •decomposes it: because these three effects are heterogeneous, there can't be *any* ratio between them, let alone one that matches the ratio among the quantities of heat applied. Thus the 'axiom' of the proportionality of effects to their causes fails at the precise point where the principle of the composition of causes fails. . . .

This is the end of my general remarks on causation, which I thought were needed as an introduction to the theory of the inductive process—a process that is essentially an inquiry into cases of causation. All the uniformities in the •succession of phenomena, and most of the uniformities in their •coexistence, are either laws of causation or consequences of such laws. If we could determine what causes are correctly assigned to what effects, and what effects to what causes, that would virtually amount to knowing the whole course of nature. All the uniformities that are mere results of causation might then be explained, and every individual fact or event might be predicted, provided we knew the relevant facts about the circumstances that preceded it.

To ascertain, therefore, what laws of causation there are in nature—to determine the effect of every cause, and the causes of all effects—is the main business of induction. And the chief object of inductive logic is to point out how this is done.

Chapter 7. Observation and experiment

§1. One upshot of what I have been saying is that the process of ascertaining •what consequents are invariably connected with what antecedents, i.e. •what phenomena are related to each other as causes and effects, is a sort of process of analysis. We can take it as certain that every fact that begins to exist has a cause, and that this cause is some fact (or facts) that immediately preceded it. The totality of present facts is the infallible result of the totality of past facts, and more immediately of all the facts that existed a moment ago. So we have here a great sequence that we know to be uniform: if the whole moment-ago state of the entire universe could occur again, it would again be followed by the present state. How, then, are we to resolve [see Glossary] this complex uniformity into the simpler uniformities that compose it, and assign to each part of the vast antecedent the part of the consequent that comes from it?

This operation, which I have called 'analytical' because it's the resolution of a complex whole into its component elements, is more than a merely mental analysis. We shan't get what we want merely by thinking about the phenomena, dividing them by the intellect alone. But such a mental partition is an indispensable first step. At first glance the order of nature looks at every moment like a chaos followed by another chaos! We must decompose each chaos into single facts. We must learn to see in the chaotic antecedent a multitude of distinct antecedents, in the chaotic consequent a multitude of distinct consequents. But this won't tell us which of the antecedents produces each consequent. To determine *that* we must try to separate the facts from one another, not only in our minds but in nature. The mental

analysis, however, must be done first. And we all know that intellects differ immensely in how they do this. It is of the essence of the act of *observing*; because the observer doesn't merely see the thing before his eyes but sees what parts it is composed of. The ability to do this well is rare:

- one person, from inattention or attending only in the wrong place, overlooks half of what he sees;
- another sets down much more than he sees, mixing it up with what he imagines or infers;
- a third takes note of the *kind* of all the circumstances, but because he's inexpert in estimating their *degree* he leaves the quantity of each vague and uncertain;
- a fourth sees the whole, but makes such an awkward division of it into parts—throwing into one mass things that should be separated, and separating others that would be better considered as one—that the result is no better, perhaps worse, than if he hadn't attempted any analysis.

We might discuss what qualities of mind and kinds of mental culture equip someone to be a good observer; but that belongs not logic but to the theory of *education* in the broadest sense of that term. There's no art of observing if 'art' is being used properly. There may be rules for observing. But these—like rules for *inventing*, are really instructions for how to put one's own mind into the state in which it will be most fitted to observe, or most likely to invent. So they are essentially rules of self-education, which is different from logic. They don't teach how to *do* the thing but how to *make ourselves capable of doing* it. They're an art of strengthening the limbs, not of using them.

How wide and how detailed does the observation have to be? How far down do we have to go in the mental analysis? That depends on the purpose in view. To ascertain the state of the whole universe at any moment is impossible, and would also be useless. When making chemical experiments we don't think it necessary to note the position of the planets, because experience has shown. . . .that in such cases *that* detail isn't relevant to the result. Thus, at times when men believed in the occult influences of the heavenly bodies it might have been unphilosophical [here = 'unscientific'] to fail to check on the precise condition of those bodies at the moment of the experiment. As for the degree of minuteness of the mental subdivision, if we had to break down what we observe into •its very simplest elements. . . ., it would be hard to say where we would find •them; we can hardly ever affirm that our divisions of any kind have reached the ultimate unit. But fortunately this doesn't matter either. The point of the mental separation is to suggest the required physical separation, as something to be done by us or sought for in nature; and we needn't go beyond the point at which we can see what observations or experiments we require. What *does* matter is this: at whatever point our mental decomposition of facts has stopped, we should be ready and able to carry it further if there's a need for that, not allowing the freedom of our discriminating faculty [= 'our ability to make distinctions'] to be imprisoned by the straps and bindings of ordinary classification. That's what happened with all early speculative inquirers, the Greeks included. It seldom occurred to them that something called by •one abstract name might actually be •several phenomena, or that the facts of the universe might be decomposable into elements other than the ones already recognised in ordinary language.

§2. Suppose that we have ascertained the different antecedents and consequents and have discriminated them from one another as far as the case requires, we now face the question: Which is connected with which? There are

always many antecedents and many consequents. If the antecedents couldn't be separated from one another except in thought, or if the consequents were never found apart, it would be impossible for us to distinguish the real laws empirically, or to assign to any cause its effect, or to any effect its cause. To do that we have to encounter some of the antecedents apart from the rest, and observe what follows from them; or some of the consequents, and observe what they are preceded by. In short, we must follow the Baconian rule of **varying the circumstances**. This is indeed only the first rule of physical inquiry, and not the sole rule, as some have thought; but it is the foundation of all the rest.

If we want to vary the circumstances, we can rely on •observation or •experiment; we can either •find in nature an instance suited to our purposes, or •make one by an artificial arrangement of circumstances. The value of the instance depends on what it is in itself, not on how it is obtained; its role in induction depends on the same principles in each case, just as the uses of money are the same whether it is inherited or earned. So there's no difference in kind, no real logical distinction, between the two processes of investigation. But there are practical differences that it's important not to overlook.

§3. The most obvious difference is that experiment is an immense extension of observation. As well as enabling us to produce many more variations in the circumstances than nature spontaneously offers, it also (in thousands of cases) enables us to produce just exactly the sort of variation we need for discovering the law of the phenomenon ·we are studying·. Nature is seldom so friendly as to *give* us that, because it's not constructed on a plan of helping us to study it!

For example, in order to ascertain what principle [see Glossary] in the atmosphere enables it to sustain life, we need a living animal to be immersed in each component element of the atmosphere separately. But **nature** doesn't supply either oxygen or nitrogen in a separate state. We are indebted to **artificial** experiments for our knowledge that it's oxygen, not nitrogen, that supports respiration; and for our knowledge of the very existence of those two ingredients.

Everyone realises that experimentation has the advantage over simple observation that it enables us to •obtain ever so many combinations of circumstances that aren't found in nature, and so •add to nature's experiments a multitude of experiments of our own. But many people *don't* realise that there's another superiority. . . .of artificially obtained instances over spontaneous ones—of our own experiments over even the same experiments when made by nature—which is at least as important.

When we produce a phenomenon artificially, we can (as it were) take it home with us, and observe it in circumstances [see Glossary] that we know about in detail. When we want to know what the effects are of the cause A, if we can produce A by means at our disposal we can generally determine at our own discretion. . . .the whole of the circumstances that are present along with it; and because this lets us know exactly the simultaneous state of everything else that could interact with A, we have only to observe what alteration is made in that state by the presence of A.

For example the electric machine lets us produce in thoroughly known circumstances the phenomena that nature displays on a grander scale in the form of lightning and thunder. Think about it: How much could mankind have learned about the effects and laws of electric agency from the mere observation of thunder-storms? And compare that with what they have learned and may expect to learn from

electrical and galvanic experiments! What makes this example especially striking is the fact—as we now have reason to believe—that electric action is of all natural phenomena (except heat) the most pervading and universal. This might lead you to think that electricity has the least need to be artificially produced in order to be studied; but the fact is the reverse of that—without the electric machine, the Leyden jar, and the voltaic battery we would probably never have suspected the existence of electricity as one of the great agents in nature; the few electric phenomena we would have known of would have gone on being regarded either as supernatural or as a sort of anomaly, an eccentricity in the order of the universe.

When we have insulated the phenomenon we're investigating by placing it among known circumstances, we can vary the circumstances in any way we like, choosing the variations that we think have the best chance of bringing the laws of the phenomenon into a clear light. By introducing one well-defined circumstance after another into the experiment, we discover how the phenomenon behaves in an indefinite variety of possible circumstances. Thus, when chemists have obtained some newly-discovered substance in a pure state. . . .they •introduce various other substances one by one, to discover whether it will combine with them or decompose them, and with what result; and also •apply heat, or electricity, or pressure, to discover what will happen to the substance in each of these circumstances.

But if we can't produce the phenomenon, and have to look for occurrences of it in nature, the task before us is very different.

Rather than *choosing* what the concomitant [see Glossary] circumstances shall be, we now have to *discover* what they are; and it's next to impossible to do this with any precision and completeness except in the simplest and most accessible

cases. Here's a phenomenon that we have no means of making artificially—*a human mind.* **Nature** produces many; but because we can't produce them by **art** we can see a human mind developing or acting on other things only when it is surrounded and obscured by an indefinite multitude of undiscoverable circumstances, making the use of ordinary experimental methods almost delusive. To get a sense of the scope of this difficulty, consider the fact that whenever nature produces a human mind she produces a body closely connected with it; i.e. a vast complex of physical facts, with no two of these complexes being exactly alike (probably), and most of them being radically out of the reach of our means of exploration (except for the mere structure, which we can examine in a coarse way after it has ceased to act [i.e. in an autopsy]). And if instead of a human mind we try to investigate a human society or a state, we encounter all the same difficulties—the same only worse.

We are now within sight of a conclusion that later chapters will (I think) make shiningly evident: in the sciences dealing with phenomena in which •artificial experiments are impossible (such as astronomy), or in which •they have a very limited range (as in psychology, social science, and even physiology), induction from direct experience is practised at a disadvantage that usually amounts to impossibility. If those sciences are to learn anything worth learning, therefore, their methods must be largely and perhaps principally deductive. This is already known to be the case with astronomy; that it's not generally recognised as true of the others is probably one reason why they aren't in a more advanced state.

§4. Although pure ·hands-off· observation is at a great disadvantage compared with artificial experimentation in one branch of the direct exploration of phenomena, there's another branch where the advantage is all on the other side.

Because inductive inquiry aims to learn what causes are connected with what effects, we can begin this search at either end of the road: we can inquire either into •the effects of a given cause or into •the causes of a given effect. The fact that light blackens silver chloride could have been discovered either

(a) by experiments on light, trying what effect it would have on various substances, or

(b) by observing that portions of the chloride had repeatedly become black, and investigating the circumstances.

The effect of the poison curare could have become known either

(a) by administering it to animals, or

(b) by examining how it came about that the wounds the Indians of Guiana inflict with their arrows are always fatal.

A quick look at those examples, with no need for theoretical discussion, shows that artificial experimentation is possible only with the (a) procedures. We can take a cause and *try* what it will produce; we can't take an effect and *try* what it will be produced by. We can only watch till we see it produced, or are enabled to produce it by accident.

This wouldn't matter much if it was always up to us to choose which end to start from. But we seldom have any option. We can only travel from the known to the unknown, so we have to start at whichever end we know most about. If the agent is more familiar to us than its effects, we watch for or contrive instances of the agent in whatever varieties of circumstances we can manage, and observe the result. If the conditions on which a phenomenon depends are obscure but the phenomenon itself is familiar, we must start our inquiry from the effect. If we're struck with the fact that silver chloride has been blackened, and have no idea of the cause,

all we can do is to compare instances where the blackening has happened to occur, until through that comparison we discover that in all those instances the substances had been exposed to light. If we knew nothing of the Indian arrows but their fatal effect, accident alone could turn our attention to experiments on the poison; in the regular course of investigation we could only investigate or try to observe what had been done to the arrows in particular instances.

Whenever we have no leads on the cause and therefore have to start from the effect and apply the 'varying the circumstances' rule to the consequents, not the antecedents, we're deprived of the resource of artificial experimentation. But this is a matter of looking for or waiting for cases of the consequent in varying circumstances; we can't *produce* them because the only way to produce an effect is through its cause, and we don't *know* the cause. . . . If nature happens to present us with instances sufficiently varied in their circumstances, and if we can discover something that is always found—either immediately before the effect or some distance further back—when the effect is found and never found when it isn't, we can discover by mere observation and without experiment a real uniformity in nature.

But although this is certainly the most favourable case for •sciences of pure observation, as contrasted with •sciences in which artificial experiments are possible, there's really no case that more strikingly illustrates the inherent *imperfection* of direct induction when not based on experiment. Suppose that by comparing cases of the effect y we find an antecedent x that appears to be invariably connected with it; we haven't proved x to be the cause of y until we have reversed the process and used x to produce y. If we can produce x artificially, and if when we do so y follows, the induction is complete: we know that antecedent x is the

cause of that consequent y.[1] But we got there by adding the evidence of experiment to that of simple observation. [Mill then goes through it all again, with different words but the same content. He sums up:] In short, observation without experiment (and with no aid from deduction) can discover sequences and coexistences but can't prove causation.

[Mill cites zoology as a science in which an enormous amount is known about what follows what and what coexists with what, and yet:] on this vast subject. . . .we have made most scanty progress in discovering any laws of causation. In most of the cases of coexistence of animal phenomena

we don't know for sure which is the cause and which the effect (or whether they aren't related as cause and effect but rather are two effects of causes yet to be discovered, complex results of laws hitherto unknown.

Some of what I have said really belongs later, but I thought that a few general remarks on •how sciences of mere observation differ from sciences of experimentation, and on •the extreme disadvantage that inductive inquiry labours under in the former, would be the best preparation for discussing the methods of direct induction. . . ., a discussion to which I now proceed.

Chapter 8. The four methods of experimental inquiry

§1. The aim is to single out from among the circumstances that precede or follow a phenomenon the ones that it is really connected with by an invariable law. Two ways of doing this are simpler and more obvious than any other others. In the **Method of Agreement** we compare different instances in which the phenomenon occurs. In the **Method of Difference** we compare instances in which it occurs with instances in other respects similar in which it doesn't.

In illustrating these methods. . . ., I'll attend to their use both in •inquiring into the cause of a given effect and •inquiring into the effects or properties of a given cause. . . . I'll denote antecedents by upper-case letters and the corresponding consequents by ·italicised· lower-case.

Let A be an agent or cause, and suppose we are trying to ascertain what its effects are. If we can find or produce A in such varieties of circumstances that the different cases have

no circumstance in common except A, then any effect that we find to be produced in *all* our trials is shown to be the effect of A. Suppose, for example, that A is tried along with B and C, and that the effect is *a b c*; and suppose that A is next tried with D and E, but without B and C, and that the effect is *a d e*. Then we may reason thus: *b* and *c* are not effects of A, for they weren't produced by it in the second experiment; nor are *d* and *e*, for they weren't produced in the first. Whatever is really the effect of A must have been produced in both instances, and the only circumstance that's true of is *a*. . . .

For example, let the A be the contact of an alkaline substance and an oil. We try this combination in several varieties of circumstances that resemble each other only in that they all produce a greasy and soap-like substance; so we conclude that the combination of an oil and an alkali causes the production of a soap. That is how we use the

---

[1]   Unless y was generated not by the x but by the means used to produce the x. But these means are •under our power, so there's some probability that they are also sufficiently •within our knowledge to enable us to judge whether that could be the case.

Method of Agreement to inquire into the effect of a given cause.

In a similar way we can inquire into the cause of a given effect. Let *a* be the effect. Here. . . .we have only the resource of observation without experiment; we can't take a phenomenon of which we don't know the origin and try to find how it is produced by producing it!. . . . But if we can observe *a* in two different combinations, *a b c* and *a d e*; and if we know or can discover that the antecedent circumstances in these cases respectively were A B C and A D E, we can conclude by a reasoning similar to that in the 'soap' example that A is the antecedent connected with the consequent *a* by a law of causation. B and C can't be causes of *a* because on its second occurrence they weren't present; nor can D and E, because they weren't present on its first occurrence. A is the only one of the five circumstances that was found among the antecedents of *a* in both instances.

For example, suppose the effect whose cause we want to discover is crystallisation. We compare cases where bodies are known to acquire crystalline structure but have nothing else in common. We find them to have one—and as far as we can see *only* one—antecedent in common, namely the deposition of a solid matter from a liquid state. . . . So we conclude that the solidification of a substance from a liquid state is an invariable antecedent of its crystallisation.

In this example we can go further and say that this is not only •the invariable antecedent of crystallisation but •the cause of it; or at least the immediately preceding event that completes the cause. That's because after detecting the antecedent A we can •produce it artificially, and by finding that *a* follows it •verify the result of our induction. [Mill cites two examples, discoveries about how to produce quartz and how to produce marble. He comments that these are] two admirable examples of the light that can be thrown upon the most secret processes of Nature by well-contrived interrogation of her.

But if we can't artificially produce A, the conclusion that it's the cause of *a* remains very doubtful. Even if it's an invariable antecedent of *a*, preceding it as day precedes night, it may not be *unconditionally* so.

This uncertainty arises from our inability to be sure that A is the only immediate antecedent common to both the instances. If we could be certain of having ascertained *all* the invariable antecedents, we might be sure that the unconditional invariable antecedent—i.e. the cause—must be among them. Unfortunately it's hardly ever possible to ascertain all the antecedents unless the phenomenon is one we can produce artificially. Even then, the difficulty is merely lightened, not removed; men knew how to raise water in pumps long before they learned what was really the operating circumstance in pumping, namely the pressure of the atmosphere on the open surface of the water. It's much easier to analyse completely •a set of arrangements made by ourselves than •the whole complex mass of agencies that nature happens to be exerting at the moment when a given phenomenon is produced. We may overlook some of the relevant circumstances in an experiment with an electrical machine; but at worst we'll be better acquainted with them than with the circumstances of a thunder-storm.

The way of discovering and proving laws of nature that I have just presented is based on the following axiom:

> Whatever circumstances can be absent when the phenomenon is present is not causally connected with it. With such casual circumstances set aside, if only one remains then it is the cause we are searching for; if more than one remains, they either are the cause or contain it among them; and the same thing holds *mutatis mutandis* [see Glossary] for the effect.

192

As this method proceeds by comparing different instances to ascertain what they agree in, I call it the Method of Agreement, and we can adopt as its regulating principle the following:

FIRST CANON.

If two or more instances of the phenomenon under investigation have only one circumstance in common, that one circumstance is the cause (or effect) of the given phenomenon.

I'll return to the Method of Agreement very soon, but first I proceed to a still more powerful instrument in the investigation of nature, the Method of Difference.

§2. The Method of Agreement required instances that agreed in the given circumstance but differed in every other; the present method requires two instances that resemble one another in every other respect but differ in the presence of absence of the phenomenon we wish to study ·and, presumably in the presence or absence of the cause of that phenomenon·. [That addition is needed to save Mill's account from incoherence. Other instances of the same trouble will be left untreated.] If we're trying to discover the effects of an agent A, we must procure A in some set of known circumstances A B C, note the effects of that, and compare them with the effect of the remaining circumstances B C without A. If the effect of A B C is *a b c*, and the effect of B C is *b c*, it is evident that the effect of A is *a*. And if we begin at the other end, wanting to investigate the cause of an effect *a*, we must select an instance *a b c* in which the effect occurs and the antecedents were A B C, and then look for another instance in which *b c* occur without *a*. If in that instance the antecedents are B C, we know that the cause of *a* must be A—either alone or in conjunction with some other circumstances present.

It's scarcely necessary to give examples of a logical process that gives us almost all the inductive conclusions we draw in daily life. When a man is shot through the heart, the Method of Difference shows us that it was the gunshot that killed him: he was in the fullness of life immediately before, all circumstances being the same ·as after· except the wound.

The axioms implied in this method are evidently the following. An antecedent that can't be excluded without preventing the phenomenon is the cause of that phenomenon or a condition of it; a consequent that can be excluded with no other difference in the antecedents than the absence of a particular one x is the effect of x. Instead of comparing different instances of a phenomenon to see how they agree, this method compares an instance of its occurrence with an instance of its non-occurrence to see how they differ. The regulating principle of the Method of Difference may be expressed thus:

SECOND CANON.

If an instance where the phenomenon y under investigation occurs and an instance where it doesn't occur have every circumstance in common except for one x that occurs only in the former, x is the effect or the cause or an indispensable part of the cause of y.

§3. The two methods I have presented are alike in many ways but also unalike in many way. Both are methods of *elimination*. This term (borrowed from the mathematical theory of equations. . . .) is well suited to express the operation that has been understood since the time of Bacon to be the foundation of experimental inquiry—namely the successive exclusion of the various circumstances that are found to accompany a phenomenon in a given instance, in order to

ascertain which of them can be absent consistently with the existence of the phenomenon. The Method of Agreement is based on the thesis that whatever can be eliminated is not connected with the phenomenon by any law. The Method of Difference has for its foundation that whatever *can't* be eliminated is connected with the phenomenon by a law.

Of these two, the Method of Difference is more particularly a method of artificial experiment; while the Method of Agreement is more especially what we use when experimentation is impossible. A few reflections will prove this, and point out the reason of it.

It is inherent in the unique character of the Method of Difference that the nature of the combinations it requires is much more strictly defined than in the Method of Agreement. The two instances that are to be compared must be exactly similar in all circumstances except the one we're trying to investigate; they must inter-relate as A B C relates to B C (·if we're investigating the effects of A·) or as *a b c* relates to *b c* (·if we're investigating the cause of *a*·). This similarity of circumstances needn't be total—it needn't extend to circumstances that we already know to be irrelevant to the result. With most phenomena we learn at once, from the commonest experience, that most of the coexistent phenomena in the universe can be either present or absent without affecting the given phenomenon. . . . Still, even limiting the identity that's required between the two instances A B C and B C to circumstances that aren't already known to be irrelevant, nature very seldom offers two instances that we can be sure are related in that way—·i.e. that the *only* difference between them (apart from ones that we know are irrelevant) is the presence of A in one of them and not the other·. Nature's spontaneous operations are generally so complicated and so obscure—being out of our reach because they are too vast or too tiny—that we're ignorant of a great part of the facts

that really take place, and even the ones we aren't ignorant of are so numerous and thus so seldom exactly alike in any two cases that a spontaneous experiment [= 'a hands-off observation'] of the kind required by the Method of Difference is usually not to be found. On the other hand, when we obtain a phenomenon by an artificial experiment, a pair of instances such as the method requires is obtained almost as a matter of course, provided the process doesn't last a long time. A certain state of surrounding circumstances existed before we started the experiment; this is B C. We then introduce A—e.g. by merely bringing an object from another part of the room—before there has been time for any change in the other elements. Comte was right: it's the very nature of an experiment to introduce into the pre-existing state of circumstances a perfectly definite change. We *choose* a previous state of things that we are well acquainted with, so that it's not likely to change without our noticing; and into this we introduce, as rapidly as possible, the phenomenon x that we want to study; so that in general we're entitled to be quite sure that the state we have produced differs from the pre-existing state only in the presence or the absence of x. If a bird is taken from a cage and instantly plunged into carbonic acid gas, the experimenter can be fully assured (after one or two repetitions) that no circumstance that could cause suffocation had intruded except the change from •immersion in the atmosphere to •immersion in carbonic acid gas. . . . It thus appears that in the study of the various kinds of phenomena that *we* can modify or control, we can in general satisfy the requirements of the Method of Difference; but that those requirements are seldom fulfilled by the spontaneous operations of nature.

With the Method of Agreement the situation is reversed. We don't here require instances of such a special and determinate kind. For the purposes of this method, *any* instances

in which nature presents us with a phenomenon can be examined, and if all such instances agree in *anything*, that's already a useful conclusion. It's true that we usually can't be sure that the one point of agreement is the only one; but this ignorance does not invalidate the conclusion, as it would with the Method of Difference. . . . We have ascertained one invariable antecedent or consequent, however many other invariable antecedents or consequents may still remain unascertained. If A B C and A D E and A F G are all equally followed by *a* then *a* is an invariable consequent of A. If *a b c* and *a d e* and *a f g* all have A among their antecedents, then A is connected as an antecedent with *a*. But to determine whether this invariable antecedent is a cause, or this invariable consequent an effect, we must also be able to produce one of them by means of the other; or at least to obtain an instance in which the effect *a* has come into existence with no change in the circumstances except the addition of A. (That is our only way of being sure that we have *produced* something.) And this, if we can do it, is an application of the Method of Difference, not of the Method of Agreement.

So it seems that the only way direct experience can give us certain results about causes is through the Method of Difference. The Method of Agreement leads only to uniformities which either aren't laws of causation or whose status as causal must for the present remain undecided. (Some writers call these 'laws of phenomena', but that's a bad usage because laws of causation are also laws of phenomena.)

The Method of Agreement is to be used mainly •as a means of suggesting applications of the Method of Difference (as in the last example, where the comparison of A B C and A D E and A F G suggested that A was the antecedent on which to try by experiment whether it could produce *a*); or •as a second-best in cases where the Method of Difference is

impracticable—e.g. because we can't artificially produce the phenomena. So the Method of Agreement—though applicable in theory to either case—is more emphatically the method of investigation in cases where artificial experimentation is impossible, because in them it's usually our only resource of a directly inductive kind, whereas with phenomena that we can produce at will the Method of Difference is generally more effective because it can ascertain •causes as well as •mere laws.

§4. But in many cases our power of producing the phenomenon is complete and yet the Method of Difference can't be used at all, or only with a previous use of the Method of Agreement. This occurs when our only way of producing the phenomenon involves a combination of antecedents that we can't separate from each other and exhibit apart. Suppose, for instance, that we want to investigate the cause of the double refraction of light. We can produce this phenomenon at will, using any one of the many substances that we know to refract light in that special manner—Iceland spar, for example—but we can't use the Method of Difference because we can't find another substance precisely resembling Iceland spar except in some one property. The only way to push this inquiry is the one provided by the Method of Agreement. And that's what *was* used: the physicists compared all the known substances that doubly refract light, and found that they have in common *being crystalline substances*; from which they reasonably inferred. . . . that either •crystalline structure or •the cause of that structure is one of the conditions of double refraction.

[This paragraph will have a good many small omissions not indicated by. . . .ellipses. The reasons are purely aesthetic; you can trust the paragraph's content.] Suppose that by using the Method of Agreement we have discovered that there's a connection

between A and *a*. To convert this evidence of *connection* into proof of *causation* by the direct Method of Difference we would need to do things like:

> having tested A B C and found that it leads to *a*, we then test B C and observe whether that leads to *a* also.

Now, we often can't do this (see the Iceland spar example), but sometimes we can find out what *would* be the upshot if we could test B C, and that's as good as conducting the test. Here's how we do that:

> Having tested a variety of cases where *a* occurred, and found that they all contain A, we now observe a variety of instances where *a* doesn't occur, and find that none of them contains A.

This establishes by the Method of Agreement the same connection between the absence of A and the absence of *a*, which was previously established between their presence. Just as our first work showed that whenever A is present *a* is present, so now we can conclude whenever A is absent *a* is also absent, which means that we have the positive and negative instances that the Method of Difference requires.

  This method—call it the 'Indirect Method of Difference' or the 'Joint Method of Agreement and Difference'—consists in two uses of the Method of Agreement, each independent of the other and corroborating it. But it isn't equivalent to a proof by the direct Method of Difference. The Method of Difference requires us to be quite sure •that the instances leading to *a* have nothing in common except A, or •that the instances that don't lead to *a* have nothing in common but the absence of A. This is never possible; and if it were, we wouldn't need the joint method, because either of the two sets of instances separately would prove causation. This indirect method, therefore, can only be regarded as an extension and improvement of the Method of Agreement, but not as

having any part in the more powerful nature of the Method of Difference. Its canon is this:

THIRD CANON.

If two or more instances in which the phenomenon occurs have only one circumstance x in common, while two or more instances in which it doesn't occur have nothing in common except the absence of x, then x is the effect, or the cause or an indispensable part of the cause, of the phenomenon.

[Mill says that the Joint Method of Agreement and Difference has another advantage over 'the common Method of Agreement', but that he needs to postpone discussing this until later, and will] at once proceed to a statement of the other two methods, which will complete the list of the means we have for exploring the laws of nature by specific observation and experience.

§5. The first of these has been well named 'the Method of Residues'. Its principle is very simple. Remove from any given phenomenon all the parts of it that can by virtue of preceding inductions be assigned to known causes, and what's left will be the effect of antecedents which had been overlooked or whose effect was still an unknown quantity.

  Suppose again that we have the antecedents A B C followed by the consequents *a b c*, and that by previous inductions (based, let's say, on the Method of Difference) we have discovered the causes of some of these effects or the effects of some of these causes; specifically we have learned that the effect of A is *a*, and that the effect of B is *b*. Subtracting the sum of these effects from the total phenomenon, there remains *c*, and we don't need any new experiments to know that *c* is the effect of C. This Method of Residues is in fact a special adaptation of the Method of Difference. If the instance A B C and *a b c* could have been compared with

a single instance A B and *a b*, we would have proved C to be the cause of *c* by using the Method of Difference in the ordinary way. In the present case, though, instead of a single instance A B we have had to study separately the causes A and B, and to infer from the effects they produce separately what effect they must produce in the case A B C, where they act together. Thus, of the two instances that the Method of Difference requires—one positive, the other negative—the negative one (in which the given phenomenon is absent) is not the direct result of observation and experiment, but has been arrived at by deduction. As one of the forms of the Method of Difference, the Method of Residues shares in its rigorous certainty, provided the previous inductions—the ones that gave the effects of A and B—were obtained by the same infallible method, and provided we're certain that C is the only antecedent that the residual phenomenon *c* can be connected with, i.e. the only agent whose effect we hadn't already calculated and subtracted. But we can never be quite certain of this, so the evidence derived from the Method of Residues is not complete unless we can obtain C artificially and test it separately, or unless its agency, when once suggested, can be explained and proved deductively from known laws.

Even with these reservations, the Method of Residues is one of our most important instruments of discovery. Of all the methods of investigating laws of nature, this is the most fertile in unexpected results, often informing us of sequences in which neither the cause nor the effect was conspicuous enough to attract the attention of observers. The agent C may be an obscure circumstance, not likely to have been perceived unless sought for, nor likely to have been sought for until attention had been awakened by the insufficiency of the obvious causes to account for the whole of the effect. And *c* may be so disguised by its intermixture with *a* and *b* that

it would scarcely have presented itself spontaneously as a subject of separate study. I'll soon present some remarkable examples of these uses of the Method of Residues. Its canon is as follows:

FOURTH CANON.

Subtract from any phenomenon the part of it that is known by previous inductions to be the effect of certain antecedents, and the remainder of the phenomenon is the effect of the remaining antecedents.

§6. There remains a class of laws that can't be discovered by any of the three methods I have tried to describe, namely the laws of •permanent causes—i.e. •indestructible natural agents—that we can't exclude or isolate, can't hinder from being present or arrange to have present alone. You might think that we can't possibly separate the effects of these agents from the effects of the other agents that they have to coexist with; but in fact for most of the permanent causes no such difficulty arises: although we can't eliminate them as •coexisting facts, we can eliminate them as •influencing agents by simply conducting our experiment in a place outside the reach of their influence. The swing of a pendulum, for example, is disturbed by a nearby mountain; we move the pendulum far enough away from the mountain, and the disturbance ceases. From these data [see Glossary] we can use the Method of Difference to calculate the amount of effect due to the mountain; and beyond a certain distance everything goes on precisely as it would do if the mountain exercised no influence whatever, and we reasonably enough conclude that it doesn't.

But the picture changes when we can't get ourselves ·or our experimental apparatus· out of reach of the influence of a permanent cause. The pendulum can be moved away

from the influence of •the mountain, but it can't be removed from the influence of •the earth; we can't move the earth and the pendulum away from one another, to discover whether it would continue to swing if the earth's action on it were withdrawn. Then what is our evidence that the pendulum's swing is caused by the earth's influence? It can't be anything supported by the Method of Difference, for one of the two instances is lacking—namely the negative instance where the earth's influence isn't a factor. Nor by the Method of Agreement: when any pendulum swings the earth is always present, but so is the sun! Obviously to establish even such a simple fact of causation as this we needed some method other than those I have so far presented.

For another example, consider *heat*. Independently of any theory about the real nature of heat we can be sure of this much: •we can't deprive any body of the whole of its heat, and •no-one ever perceived heat that wasn't being given off by a body. So we can't separate body and heat, and therefore can't vary the circumstances in the way the foregoing three methods require—we can't ascertain by those methods what portion of the phenomena exhibited by any body is due to the heat contained in it. If we could observe a body with its heat, and the same body entirely divested of heat, the Method of Difference would show the effect of the heat, apart from the effect of the body. If we could observe heat under circumstances agreeing only in heat, and therefore not involving the presence of a body, we could use the Method of Agreement to discover the effects of heat by comparing •an instance of heat with a body and •an instance of heat without a body; or we could use the Method of Difference to discover what effect was due to the body, the remainder due to heat being given by the Method of Residues. But we can't do any of these things, so none of the three methods can help us to solve this problem. . . .

. . . .But there is still something we can try. Even when we can't exclude an antecedent altogether, we may be able to produce—or nature may produce for us—some modification in it, by which I mean a change in it not amounting to its total removal. If some modification in the antecedent A is always followed by a change in the consequent a, the other consequents b and c remaining the same; or *vice versa* if every change in a is found to have been preceded by some modification in A, none being observable in any of the other antecedents, we can safely conclude that a is at least in part causally connected somehow with A. We can't expel heat altogether from any body, but we can modify its amount, increasing or diminishing it; and in doing this we can find by the various methods of experiment or observation that I have discussed that such increase or diminution of heat is followed by expansion or contraction of the body. This brings us to the conclusion that we couldn't have achieved in any other way, that one effect of heat is to make bodies bigger, i.e. to increase the distances between their particles.

A change in a thing that doesn't amount to its total removal—i.e. a change that leaves it still the same thing—must be a change either in •its quantity or in •some of its variable relations to other things; and the main one of these is position in space. We have seen an example depending on quantity; now for one involving spatial position. Question: what influence does the moon exert on the surface of the earth? We can't try an experiment in the absence of the moon. But when we find that all the variations in the moon's position are followed by corresponding variations in the time and place of high tide, the place always being either the part of the earth nearest to the moon or the part furthest from it, this gives us ample evidence that the moon is at least partially the cause that determines the tides. . . .

Similar evidence shows that the swinging of a pendulum is caused by the earth. The swings take place between equidistant points on opposite sides of a line that •is perpendicular to the earth, and therefore •varies with every variation in the earth's position. . . . This method tells us that all terrestrial bodies tend toward the earth, and not towards some unknown fixed point lying in the same direction. In every 24 hours of the earth's rotation, the line drawn from the body at right angles to the earth coincides successively with all the radii of a circle, and in the course of six months the place of that circle changes by nearly $200,000,000$ million miles; yet in all these changes of the earth's position the line in which bodies tend to fall—·the line down the centre of the pendulum's swing·—continues to be directed toward it. This proves that terrestrial gravity is directed towards the earth and not, as some people used to think, towards a fixed point in space.

The method by which these results were obtained may be termed the Method of Concomitant [see Glossary] Variations; it is regulated by the following canon:

FIFTH CANON.
If any phenomenon x varies in some specific way whenever another phenomenon y varies in some specific way, x is either a cause or an effect of y, or is causally connected with it in some other manner.

I add that last clause because when two phenomena match each other in their variations it doesn't follow one is cause and the other effect. If they were two effects of a common cause, they would exhibit concomitant variation; and this method alone can't tell us whether they're related in that way rather than as cause and effect. The only way to answer the question would be—yet again!—by trying to ascertain whether we can produce the one set of variations by means of the other. In the case of heat, for example, by increasing the temperature of a body we make it bigger, but by making it bigger (e.g. by using an air-pump to decrease the air-pressure on it) we don't increase its temperature; on the contrary, in most cases we diminish it. So heat is not an effect of increase in size but a cause of it. If we can't ourselves produce the variations, we must try—though we'll usually fail—to find them produced by nature in some case in which the pre-existing circumstances are perfectly known to us. . . .

You might think that the Method of Concomitant Variations assumes a new axiom, i.e. a new law of causation in general, namely: *Every modification of the cause is followed by a change in the effect.* And it does usually happen that when a phenomenon A causes a phenomenon *a*, any variation in A's quantity or relational properties is uniformly followed by a variation in the quantity or relational properties of *a*. . . . The sun causes a certain tendency to motion in the earth; here we have cause and effect; but that tendency is toward the sun, and therefore varies in •direction as the sun varies in its position relative to the earth; and the tendency also varies in •intensity in a certain numerical correspondence to the sun's distance from the earth—i.e. according to another relation of the sun. So there's not only an invariable connection between the sun and the earth's gravitation, but two of the sun's relational properties—its position relative to the earth and its distance from the earth—are invariably connected as antecedents with the quantity and direction of the earth's gravitation. The cause of the earth's gravitating at all is simply *the sun*; but the cause of its gravitating with a given intensity in a given direction is the existence of the sun at a given distance and in a given direction from the earth. A modified cause is really a different cause, so it's not surprising that it produces a different effect.

But the Method of Concomitant Variations doesn't require as an axiom that

(a) If x is the cause of y, any modification of x is followed by a modification of y.

All it needs is the converse proposition:

(b) If every modification of x is followed by a modification of y, x is the cause of y (or is connected with the cause of y).

It's obvious that **(b)** is true, because if x has no influence over y then modifications of x can't influence y either. If the stars have no power over the fortunes of mankind, then the conjunctions or oppositions of stars can have no such power.

The most striking uses of the Method of Concomitant Variations occur in cases where the Method of Difference, strictly so-called, is impossible; but its use isn't confined to those cases. It is often useful as a follow-up to the Method of Difference, to give additional precision to a solution that the latter method has found. When we know through the Method of Difference that x produces y, the Method of Concomitant Variations can be usefully called in to determine what law governs the match between x's quantity and relational properties and y's.

§7. This method is most widely used in cases where the ·relevant· variations of the cause are variations of *quantity*. It's pretty safe to say that quantitative variations in the cause will be attended by *quantitative* variations in the effect; because the proposition that •more of the cause is followed by •more of the effect follows from the principle of the Composition of Causes, which we saw on page 182 to be the •general rule of causation, whereas counterexamples to it—cases where causes change their properties on being combined—are •special and exceptional. Suppose that when A changes in quantity, a also changes in quantity, and that

we can trace the •numerical relation between parts of the two sets of changes—the parts, that is, that aren't too big or too small for us to observe them. Then with certain precautions we can safely conclude that the same •numerical relation will hold outside those limits. If we find that when A is double, a is double, when A is treble or quadruple, a is treble or quadruple, we can conclude

(i) that if A were a half or a third, a would be a half or a third, and

(ii) that if A were annihilated, a would be annihilated; and thus

(iii) that a is wholly the effect of A or wholly the effect of A's cause.

And we could infer **(iii)** for any other numerical relation according to which **(ii)** A and a would vanish simultaneously—e.g. if a were proportional to A^2. If on the other hand a is not wholly the effect of A, but still varies when A varies, it is probably a mathematical function not of A alone but of A and something else. For example, its changes may be what you would get if some part of it remained constant or varied on some other principle, while the remainder varied in some numerical relation to the variations of A. In that case, as A diminishes, a will be seen to approach not •zero but •some other limit; and when the series of variations indicates what that limit is, the limit will exactly measure how much of a is the effect of some other and independent cause, and the remainder will be the effect of A (or of the cause of A). That is stated for cases where the limit is constant; if it is variable, replace 'indicates what that limit is' by 'indicates what the law of its variation is'.

But these conclusions mustn't be drawn without certain precautions. In the first place, they can't be drawn at all unless we're acquainted not only with •the variations but with •the absolute quantities both of A and a. If we don't

know the total quantities, we can't determine the numerical relation according to which they vary. So it's an error to conclude (as some have concluded) that because increase of heat expands bodies, i.e. increases the distance between their particles, therefore •that distance is wholly the effect of heat, and •if we could entirely deprive the body of its heat the particles would be in complete contact. This is a mere guess, and wildly risky one rather than a legitimate induction. Because we don't know how much heat there is in any body, or what the real distance is between any two of its particles, we can't judge whether the contraction of the distance follows the diminution of the quantity of heat according to a numerical relation such that the two quantities would reach zero simultaneously.

Now consider a case where the absolute quantities *are* known, namely the case addressed in the first law of motion, which says that *all bodies in motion continue to move in a straight line with uniform velocity until acted upon by some new force.* This is in open opposition to first appearances; all moving terrestrial objects slow down and eventually stop; and the ancients—going by *inductio per enumerationem simplicem* [see Glossary]—imagined that to be the law. But every moving body encounters various obstacles—friction, the resistance of the atmosphere, etc.—which we know by daily experience to be causes that can destroy motion. It was suggested that the lessening of motion might come wholly from these causes. How was this inquired into? With the obstacles entirely removed, the Method of Difference could have come into play. But they couldn't be removed, only lessened, so the case had to be handled by the Method of Concomitant Variations. This was used, and it was found that every lessening of the obstacles lessened the slowing of the motion; and this being a case (unlike the case of heat) where the total quantities of both the antecedent and of

the consequent were known, it was possible to get a fairly accurate estimate of the amount of •the slowing and the amount of •the relevant resistances, and to judge how near each was to zero; and it turned out that the effect dwindled as rapidly as the cause did, so that at each step the two were equally near to annihilation. The swinging of a weight suspended from a fixed point and moved a little out of the perpendicular ordinarily lasts for only a few minutes, but Borda got it to continue for more than thirty hours by going as far as possible towards reducing the friction at the point of suspension and making the body move in a vacuum. That left no reason to hesitate to conclude that the whole of the slowing of motion was due to the influence of the obstacles. With the slowing removed from the total phenomenon, the remainder was a uniform velocity, and the result was the proposition known as the first law of motion.

The inference that *the law of variation that the quantities conform to within our limits of observation will hold beyond those limits* is open to another kind of uncertainty. Actually there are two of them, one being obvious: we don't know what happens in the range outside the limits of our observation, and it *might* be that something comes into play there that spoils our conclusion. This kind of uncertainty comes into virtually all our predictions of effects; it's not specially relevant to the Method of Concomitant Variations. I want to talk about an uncertainty that is characteristic of that method; especially in the cases where our *observable* range is very small compared with the *possible* variations in the quantities of the phenomena. If you know anything of mathematics you know that very different laws of variation can produce numerical results that differ only slightly from one another; and in many cases it's only when the absolute amounts of variation are considerable that we can see the difference between the results given by two rival laws. The upshot is

that when the variations in the quantity of the antecedents that we can observe are small in comparison with the total quantities, there's a great danger of our picking the wrong numerical law, and being led to miscalculate the variations that would occur beyond our limits. That miscalculation would invalidate any conclusion about the dependence of the effect on the cause. There are plenty of examples of such mistakes. Herschel writes: 'The formulae that have been empirically deduced for the elasticity of steam (till very recently), and those for the resistance of fluids and other similar subjects', when relied on beyond the limits of the observations from which they were deduced, 'have almost invariably failed to support the theoretical structures based on them'.

Even when we have this uncertainty, the Method of Concomitant Variation can prove that there is some connection between A and a, and....can legitimately satisfy us that the relation we have observed (within our limits) to exist between the variations of A and a will hold true in all cases that fall between those same limits. . . .

The four methods that I have tried to describe are the only possible modes of experimental inquiry—of direct induction *a posteriori* as distinguished from deduction. At any rate, I don't know of any others and can't imagine any others. And the Method of Residues (I remind you) isn't independent of deduction; but I include it among methods of direct observation and experiment because as well as deduction it also requires specific experience.

. . . .In chapter 10 I'll come to certain circumstances that make the use of these methods much more complicated and difficult ·than I have so far indicated·. Before coming to that, though, I shall illustrate the use of the methods by suitable examples drawn from actual physical investigations.

Chapter 9. Examples of the four methods

§1. First example: I'll start with an interesting bit of theory by one of the most eminent theoretical chemists, Baron Liebig. The objective is to discover the immediate cause of the death produced by metallic poisons.

Arsenious acid and the salts of lead, bismuth, copper, and mercury, if introduced into the animal organism in anything but the smallest doses, destroy life. These facts have long been known, as separate and unconnected truths that are as ungeneral as generalisations can be. It was left to Liebig, by an apt employment of the Methods of Agreement and Difference, to connect these truths with one another by a higher induction, revealing the property that •is common to all these harmful substances and •is the operative cause of their fatal effect.

(a) When solutions of these substances are placed in close enough contact with many animal products—albumen, milk, muscular fibre, and animal membranes—the acid or salt leaves the water it was dissolved in and enters into combination with the animal substance; and this substance, after being thus acted upon, is found to have lost its tendency to putrefy [see Glossary].

(b) Observation also shows, in cases where death has been produced by these poisons, that the parts of the body that the poisonous substances have been brought into contact

202

with don't afterwards putrefy.

(c) And, finally, when the poison has been supplied in too small a quantity to destroy life. . . ., certain superficial portions of the tissues are destroyed and afterwards thrown off by the process of recovery in the healthy parts.

These three sets of instances can be handled according to the Method of Agreement. In all of them the metallic compounds are brought into contact with the substances that compose the human or animal body; and the instances seem to have nothing else in common. The remaining antecedents are as different—even opposite—as they could possibly be made; for in some the animal substances exposed to the action of the poisons are in a state of life, in others only in a state of organisation, in others not even in that. And the result in *all* the cases is the conversion of the animal substance (by combination with the poison) into a chemical compound that is held together by force so powerful that it resists the subsequent action of the ordinary causes of decomposition. Now, organic life (the necessary condition of sensitive life) consists in a continual state of decomposition and recomposition of the different organs and tissues, so anything that prevents this decomposition destroys life. Thus the immediate cause of the death produced by poisons of this kind is ascertained, as far as the Method of Agreement can ascertain it.

Let us now use the Method of Difference to test our conclusion. This will involve a comparison. On one hand we have:

> cases where the antecedent is the presence of substances that combine with the tissues to form a compound that can't putrefy (and therefore can't support life), the consequent being death of the whole organism or of some part of it.

We are to compare these with

> cases as much like the former ones as possible except that they don't have the death of anything as their effect.

Many insoluble basic salts of arsenious acid are known not to be poisonous. The substance called 'alkargen', discovered by Bunsen, which contains a great amount of arsenic and is very like the organic arsenious compounds found in the body, hasn't the slightest injurious action upon the organism. Now when these substances are brought into contact with the tissues in any way, they don't combine with them, and don't stop their progress towards decomposition. What these instances seem to show is that when the effect is absent that's because of the absence of the antecedent that we already had good reason to consider as the immediate cause.

But the rigorous conditions of the Method of Difference aren't yet satisfied; for we can't be sure that these unpoisonous bodies differ from the poisonous substances *only* in not combining with animal tissues to form a compound that resists decomposition. To make the method strictly applicable, we need an instance not of a different substance but of one of the very same substances, in circumstances that prevent it from combining with the tissues to form the sort of compound in question; and *then*, if death doesn't follow, our case is made out. Instances of this kind are provided by the antidotes to these poisons. For example, if hydrated peroxide of iron is administered along with poisonous arsenious acid, the destructive agency of the latter is instantly checked. Now, this peroxide is known to combine with the acid to form a compound that is insoluble, and so can't act at all on animal tissues. Thus, sugar is a well-known antidote to poisoning by salts of copper; and sugar turns those salts into something that doesn't combine with animal matter. The disease called 'painter's colic', so common in factories making white lead, is unknown where

the workmen regularly take (as a preservative) a solution of sugar made acid by sulphuric acid. Now, diluted sulphuric acid has the property of •decomposing all compounds of lead with organic matter or •preventing them from being formed.

[Mill then describes a set of facts about 'soluble salts of silver' which, when applied externally, have about the same effect as arsenious acid, but aren't poisonous when ingested. The explanation is that the animal stomach contains common salt and muriatic acid, which turn the soluble salts into something virtually insoluble and therefore unable to combine with the tissues to fatal effect,]

Those instances have shown us a very conclusive induction that illustrates the two simplest of our four methods; though it doesn't rise to the maximum of certainty that a *perfect* example of the Method of Difference can provide. Remember that the positive instance and the negative one strictly ought to differ only in the presence or absence of one single circumstance. And in the foregoing argument they differ in the presence or absence not of a single •circumstance but of a single •substance; every substance has countless properties; so there's no knowing how many real differences are involved in what is apparently only one difference. It is conceivable that the antidote. . . .counteracts •the poison through some property other than that of forming an insoluble compound with •it; and if that were so the theory would collapse so far as it rests on that instance. This source of uncertainty is a serious hindrance to all extensive generalisations in chemistry; but in our present case it is reduced to almost the lowest possible degree when we find that *many* substances can act as antidotes to metallic poisons, and that all these share the property of forming insoluble compounds with the poisons and can't be ascertained to share any other

property whatsoever. So we have in favour of the theory all the evidence that can be obtained by the Joint Method of Agreement and Difference [see page 196]; and though the evidence it produces can't amount to that of the Method of Difference properly so-called, it can approach indefinitely near to that.

§2. Second example: The aim is to discover the law governing 'induced electricity'—i.e. to learn under what conditions a body that is positively or negatively electrified gives rise to the opposite electric state in some other body adjacent to it.[1]

The most familiar kind of example of the phenomenon to be investigated is the following. Around the prime conductors of an electrical machine the nearby atmosphere or any conducting surface suspended in it is found to be in the electric condition opposite to that of the prime conductor itself: near and around the positive prime conductor there's negative electricity, and near and around the negative prime conductor there's positive electricity. When a pith ball (or a human hand) is brought near to one of the conductors, it becomes electrified with the opposite electricity to it—either •receiving a share from the already electrified atmosphere by conduction, or •acted on by the direct inductive influence of the conductor itself—and then it is attracted by the conductor to which it is opposite or by any other oppositely charged body. Now, we have no evidence that a charged conductor can be suddenly discharged except by the approach of a body with the opposite charge. In the case of the electric machine, therefore, it appears that the accumulation of electricity in an insulated conductor is always accompanied by the excitement of the opposite electricity in •the surrounding atmosphere and in things in •it. It does not seem possible,

---

[1] For this bit of theorising as for many of my other scientific illustrations I am indebted to Bain, whose treatise on Logic is full of apt illustrations of all the inductive methods.

in this case, to produce one electricity by itself. [That last sentence is verbatim from Mill.]

Let us now examine all the other instances we can get that resemble this one in the given consequent, namely *the occurrence of an opposite electricity in the neighbourhood of an electrified body.* One remarkable instance is the Leyden jar; another is the magnet, in which it is impossible to produce one kind of electricity by itself, i.e. to charge one pole without charging another pole with the opposite electricity at the same time. (That holds both for natural magnets and for electromagnets. In counting magnets as relevant to my topic, I am relying on Faraday's splendid experiments decisively showing that magnetism and electricity are basically the same thing.) We can't have a magnet with one pole; if we break a natural lodestone into a thousand pieces, each piece will have its two oppositely electrified poles complete within itself. In the voltaic circuit, again, we can't have one current without its opposite. In the ordinary electric machine, the glass cylinder or plate acquires an electrical charge opposite to that of the rubber.

From all these instances, treated by the Method of Agreement, a general law appears to result. The instances cover all the known ways in which a body can get an electric charge; and in all of them there is found, as a concomitant or consequent, the excitement of the opposite electric charge in some other body or bodies. It seems to follow that the two facts are invariably connected, and that a necessary condition of a body's acquiring an electric charge is the simultaneous excitement of the opposite charge in some neighbouring body.

As the two opposite charges can only be produced together, so they can only cease together. This can be shown by an application of the Method of Difference to the Leyden jar. In the Leyden jar electricity can be accumulated and retained in considerable quantity, by the device of having two conducting surfaces of equal extent, and parallel to each other through the whole of that extent, with a non-conducting substance such as glass between them. When one side of the jar is charged positively, the other is charged negatively (which is why I cited the Leyden jar as an instance in our use of the Method of Agreement). Now, it's impossible to discharge one of the coatings unless the other is discharged at the same time. A conductor held to the positive side can't convey away any electricity unless an equal quantity is allowed to pass from the negative side; if one coating is perfectly insulated, the charge is safe. . . .

The law that this strongly indicates can be corroborated by the Method of Concomitant Variations. The Leyden jar can receive a much higher charge than can ordinarily be given to the conductor of an electrical machine. Now, in the Leyden jar the metallic surface that receives •the induced electricity is a conductor exactly like that which receives •the primary charge, and is therefore as capable of receiving and retaining one charge as the opposite surface is of receiving and retaining the other; but in the machine the neighbouring body that is to get the opposite charge is the surrounding atmosphere or a nearby object; and as these can usually hold only a much smaller charge than the conductor itself, their limited power imposes a corresponding limit to the conductor's capacity for being charged. As the neighbouring body's ability to support the opposition increases, a higher charge becomes possible; and that appears to explain the great superiority of the Leyden jar.

One of Faraday's experiments provides a further and most decisive confirmation by the Method of Difference. [Mill's account of the experiment and the conclusion drawn from it is hard to follow. It speaks of 'two opposite ·electric· currents. . . .both accommodated in one wire', and it's hard

to see what Mill has in mind. We can slide past this example without harm to our grasp of the rest of what he has to say.]

§3. Our **third example** will be extracted from Herschel's *Discourse on the Study of Natural Philosophy*, a work full of well-selected examples of inductive processes from almost every branch of physical science. . . . The present example is described by Herschel as 'one of the most beautiful specimens' that can be cited 'of inductive experimental inquiry lying within a moderate compass'—namely, the theory of *dew* that is now accepted by all scientific authorities.

[Mill devotes four pages to this, much of it in direct quotations from Herschel. We can afford to excuse ourselves from going through all the details. Mill shows that the series of tests and experiments make clear use of three of his methods (the exception being the Method of Residues). At a certain point he arrives at this:]

It thus appears that the various instances in which much dew is deposited agree in this (and as far as we can see *only* this): they either •radiate heat rapidly or •conduct it slowly; and those two qualities have nothing in common except that by virtue of either of them the body tends to lose heat from the surface faster than it can be restored from within. And the instances where little or no dew is formed have nothing in common (as far as we can see) except *not* having this same property. So we seem to have detected the characteristic difference between the substances on which dew is produced and those on which it isn't produced. We have done this by using the Joint Method of Agreement and Difference; and the data were prepared for that by the Methods of Agreement and of Concomitant Variations. . . .

Can we be quite sure that the substances on which dew is produced differ from those on which it isn't in *nothing* but the property—·I'll call it R·—of losing heat from the surface

faster than the loss can be repaired from within? No, but this matters less than you might think. Suppose there is an undiscovered property Q that is present in all the substances that contract dew and absent from those that don't, Q must be present in all the substances that have R and in none of the substances that don't. That much match between two properties creates a strong presumption that they have the same cause and therefore will invariably go together. And if that is right, then the property R—being a better radiator than conductor—if it isn't itself the cause almost certainly always *accompanies* the cause, and for purposes of prediction we can safely treating it as if it really were such.

At an earlier stage of the inquiry we found that whenever dew is formed the surface on which it forms is colder than the surrounding air. Was this coldness the cause of dew or an effect of it? We can now answer this. We have found that when dew forms, the substance on which it forms is one which, by its own properties or laws, would if exposed in the night become colder than the surrounding air. •The coldness is accounted for independently of the dew, while it is proved that •there is a connection between the two; so it must be the case that •the dew depends on the coldness, i.e. that the coldness is the cause of the dew.

This law of causation, already so amply established, can be further corroborated in no less than three ways. **(i)** First, by the Deductive Method. I won't be ready to deal with that until chapter 11, but I'll say enough here to firm up the results concerning dew. It is known by direct experiment that only a limited quantity of water can remain suspended as vapour at each degree of temperature, and that this maximum goes down as the temperature falls. From this it follows deductively that if the air already has much vapour as it can contain at its existing temperature, any lowering of that temperature will cause a portion of the vapour to be

condensed. And we also know deductively, from the laws of heat, that the air's contact with a body colder than itself must lower the temperature of the layer of air immediately against its surface, and will therefore cause it to part with some of its water. And this, by the ordinary laws of gravitation or cohesion—·deduction again!·—will attach itself to the surface of the body, constituting dew. This deductive proof has the advantage of proving causation as well as coexistence; and it has the further advantage of explaining the exceptions, the cases where the body is colder than the air but no dew is deposited—by showing that this must be the case when the air has too little vapour to give any of it up. That's why in a very dry summer there are no dews, and in a very dry winter no hoar-frost. This is a condition of the production of dew that wasn't detected by the other methods; it might have remained still undetected if we hadn't set out to *deduce* the effect from the known properties of the agents known to be present.

(ii) The second corroboration is by direct experiment according to the canon of the Method of Difference. By cooling the surface of a body we can find the temperature at which dew begins to be deposited. Here again the causation is directly proved. We can accomplish this only on a small scale, but we have ample reason to conclude that the same operation, if conducted in •nature's great laboratory, would equally produce the effect.

(iii) Even on •that great scale we can verify the result. This

is one of the rare [see page 194] cases where nature works the experiment for us in the same way that we ourselves perform it, introducing into the previous state of things a single perfectly definite new circumstance, and producing the effect so rapidly that there's no time for any other material [see Glossary] change in the pre-existing circumstances. Herschel writes:

> 'It is observed that dew is never copiously deposited in situations much screened from the open sky, and not at all in a cloudy night; but if the clouds withdraw even for a few minutes and leave a clear opening, dew starts to appears almost at once, and goes on increasing. . . Dew formed in clear intervals often evaporates when the sky becomes thickly overcast.'

So we have complete proof that the presence or absence of an uninterrupted communication with the sky causes the deposition or non-deposition of dew. Now, a clear sky is merely the absence of clouds, and we know that clouds. . . .tend to raise or keep up the surface temperature of a nearby object by radiating heat to it; so we see at once that the disappearance of clouds will cause the surface to cool. Thus, in this case nature produces a change in the antecedent by definite and known means, and the consequent follows accordingly—a natural experiment that satisfies the requirements of the Method of Difference![1]

The accumulated proof that has been found for the theory of dew is a striking example of the fullness of assurance that

---

[1]   This example may seem to count against my claim that the Method of Difference doesn't apply well to cases of pure observation ·as distinct from controlled experiments·; but really it doesn't. Nature seems to have imitated man's type of experiment, but has succeeded only in copying man's most imperfect experiments—namely, those in which he succeeds in producing the phenomenon only by using complex •means that he can't perfectly analyse and therefore can't tell what parts of the effects may be due not to the supposed cause but to some unknown agency of the •means by which that cause was produced. In the natural experiment in question here, the •means was the clearing off a canopy of clouds; and we don't know enough about this process. . . .to be certain *a priori* that it couldn't operate upon the deposition of dew independently of any effect on the temperature of the earth's surface. Thus, even in a case as favorable as this to Nature's experimental talents, her experiment is of little value except in corroboration of a conclusion already reached through other means.

the inductive evidence of laws of causation can achieve in cases where the invariable sequence is far from obvious at first glance.

§4. Fourth example: The admirable physiological investigations of Brown-Séquard provide brilliant examples of the use of the inductive methods in a class of inquiries in which—for reasons I'll give soon—direct induction is done under special difficulties and disadvantages. I select his theorising. . . .about the relations between •muscular irritability [see Glossary], •*rigor mortis* [see Glossary], and •putrefaction.

The law that Brown-Séquard's investigation tends to establish, is this:

> The greater the degree of muscular irritability at the time of death, the later the *rigor mortis* sets in, and the longer it lasts, and also the later putrefaction appears, and the more slowly it progresses.'

At first glance you'd think that this must be work for the Method of Concomitant Variations, but that is wrong—it's an illusion arising from the fact that the conclusion to be tested is itself a fact about concomitant variations. For the establishment of *that* fact any of the ·four· Methods may be put to work, and it will turn out that the fourth Method—·the Method of Concomitant variations·—has a real but subordinate place in this investigation.

The items of evidence by which Brown-Séquard establishes the law can be enumerated as follows:

Firstly: **(a)** •Paralysed muscles have greater irritability than healthy muscles. And **(b)** paralysed muscles are later in entering *rigor mortis* than healthy muscles, the *rigor* lasts longer, and putrefaction sets in later and proceeds more slowly.

Brown-Séquard proved both these propositions by experiment. He established **(a)** in various ways, but most

decisively by comparing the duration of irritability in a paralysed muscle and in the corresponding healthy muscle on the opposite side when they are both submitted to the same stimulus. He often found that the paralysed muscle remained irritable up to four times as long as the healthy one. This is induction by the Method of Difference. Because the two limbs were those of the same animal, they were presumed not to differ in any circumstance relevant to the case except the paralysis, so that the presence and absence of paralysis was the source of the difference in the muscular irritability. The assumption that there was only one relevant difference between the legs wasn't safe in any *one* pair of experiments, because the two legs of any given animal might happen to differ in other relevant respects; but if. . . .the experiment was repeated often enough with different animals to exclude the supposition that any abnormal circumstance could be present in them all, the conditions of the Method of Difference were well enough satisfied.

Brown-Séquard also proved the proposition **(b)** concerning *rigor mortis* and putrefaction. Having. . . .cut some nerves so as to produce paralysis in one hind leg of an animal but not the other, he found that muscular irritability lasted much longer in the paralysed limb, *rigor* set in later and ended later, and putrefaction began later and progressed more slowly than on the healthy side. This is a routine use of the Method of Difference, requiring no comment. An important corroboration was obtained by the same method. When the animal was killed not •soon after the nerves were cut but •a month later, the effect was reversed; *rigor* set in sooner and lasted a shorter time in the paralysed limb than in the healthy one. What had happened was this: During the month before death the paralysed muscles were of course •resting, and thereby •losing much of their irritability and eventually becoming *less* irritable than the muscles on the

healthy side. This gives the

A B C — *a b c* and

B C — *b c*

of the Method of Difference. When one antecedent (increased irritability) was changed and the other circumstances kept the same, the consequent didn't follow; and when a new antecedent was provided, contrary to the first, it was followed by a contrary consequent. This has the special advantage of proving that the delay and slowing of *rigor mortis* don't depend directly on the paralysis, because that was the same in both cases, but on one effect of the paralysis, namely the increased irritability—they stopped when it stopped, and were reversed when it was reversed.

Secondly: Lowering the temperature of muscles before death increases their irritability, and also delays *rigor mortis* and putrefaction.

It was Brown-Séquard himself who made these truths known, through experiments that conform to the Method of Difference. There's nothing in the nature of the process that requires comment.

Thirdly: When muscular exercise is continued to exhaustion, that lessens the muscular irritability. This is a well-known truth that depends on the most general laws of muscular action and is proved by constantly repeated experiments using the Method of Difference. Now, observation has shown that if cattle are driven too hard and then killed before they recover from their fatigue, their bodies become rigid and putrefy in a surprisingly short time. The same thing has been observed in animals hunted to death, cocks killed during or shortly after a fight, and soldiers slain in battle. The only thing involving the muscles that all these have in common is their having just been subjected to exhausting exercise. Under the canon of the Method of Agreement, therefore, we can infer that there is a connection between

the two facts. We have seen that the Method of Agreement can't prove causation; but we already know that what we're dealing with here is causation. It's certain that the body's state after death must somehow depend on its state at the time of death; so we are justified in concluding that the single circumstance shared by all the instances is the part of the antecedent that causes that particular consequent.

Fourthly: In proportion as the nutrition of muscles is in a good state, their irritability is high; this is supported also by laws of physiology based on many familiar applications of the Method of Difference. Now, when someone (or some animal) dies from accident or violence, with his muscles in a good state of nutrition, •the muscular irritability continues long after death, •*rigor* sets in late, and •it continues for a long time without putrefaction. On the other hand, in cases of disease where nutrition has been diminished for a long time before death, all these effects are reversed. This satisfies the conditions of the Joint Method of Agreement and Difference. These cases of delayed and long continued *rigor* agree only in being preceded by a high state of nutrition of the muscles; the cases of rapid and brief *rigor* agree only in being preceded by a low state of muscular nutrition; so a connection is inductively proved between •the degree of the nutrition and •the slowness and prolongation of the *rigor*.

Fifthly: Convulsions lessen the muscular irritability, like exhausting exercise but even more. When death follows violent and prolonged convulsions—as in tetanus, hydrophobia, some cases of cholera, and certain poisons—*rigor* sets in very rapidly and after a very little time gives place to putrefaction. This involves the Method of Agreement in the same way as 'Thirdly. . . .'.

Sixthly: The last series of instances that I'll present is more complex and requires a more finely detailed analysis.

It has long been observed that in some cases of death by lightning *rigor mortis* either doesn't occur at all or doesn't last long enough to be noticed, and that in these cases putrefaction is very rapid; whereas in other cases the usual *rigor mortis* appears. There must be some difference in the cause to account for this difference in the effect. [Mill reports the experimental work by Brown-Séquard that located the line between the two kinds of death by lightning (which he brought within experimental reach by substituting artificial galvanic shocks for natural lightning), namely: When and only when the eclectic shock produced muscular convulsions throughout the body, the irritability of the muscles went down, and the duration of the *rigor* went down with it. We can safely spare ourselves the details, and rejoin Mill when he quotes Brown-Séquard's summing up of his findings from all the work described in this section:]

'When the degree of muscular irritability at the time of death is considerable, either because of
  •a good state of nutrition, as in persons who die in full health from an accidental cause, or
  •rest, as in cases of paralysis, or
  •the influence of cold,
rigor mortis sets in late and lasts long, and putrefaction appears late and progresses slowly; but when the degree of muscular irritability at the time of death is slight, either because of
  •a bad state of nutrition, or
  •exhaustion from overexertion, or
  •convulsions caused by disease or poison,
rigor mortis sets in and ceases soon, and putrefaction appears and progresses quickly.'

These facts completely satisfy the conditions of the Joint Method of Agreement and Difference. Early and brief *rigor* takes place in cases that agree only in having a low state of muscular irritability. *Rigor* begins late and lasts long in cases that agree only in the opposite circumstance of high and unusually prolonged muscular irritability. It follows that there's a causal connection between the degree of muscular irritability after death and the tardiness and length of the *rigor mortis.*

This investigation shines a strong light on the value and efficacy of the Joint Method. We have seen that the defect of that Method—as of the Method of Agreement—is that it can't prove causation. But in the present case (as in one of the steps in the argument leading up to it) causation is already proved; because there could never be any doubt that the *rigor* and the ensuing putrefaction are caused by death; the empirical basis for this is too familiar to need analysis, and falls under the heading of the Method of Difference. So we know beyond doubt that the aggregate antecedent, the death, is the actual cause of the whole sequence of consequents; and ·we can get more fine-grained results—'The death's being of *this* kind is the cause of such-and-such a feature of the upshot'·—when variations in the manner of death can be shown to match corresponding variations in the effect we are investigating. . . .

§5. Some more examples: The examples I have presented offer such a clear conception of the use and practical management of three of the four methods of experimental inquiry that there's no need to give further examples of them. There remains the Method of Residues, which hasn't yet made an appearance in this chapter. I shall quote from Herschel some examples of that method, with the remarks by which they are introduced.

'It is by this process that science in its present advanced state is chiefly promoted. Most natural phenomena are very complicated; and when the effects of all known causes

are estimated exactly and set aside, the residual facts are constantly appearing in the form of entirely new phenomena that lead to the most important conclusions.

'For example, the return of the comet predicted by Professor Encke a great many times in succession, and the general good agreement of its calculated place with its observed place during any one of its periods of visibility, would lead us to say that its gravitation toward the sun and planets is the sole and sufficient cause of all the facts about its orbital motion; but when the effect of this cause is strictly calculated and subtracted from the observed motion, there remains a residual phenomenon that would never have been known to exist if this method weren't used. This residue is a small diminution of the comet's periodic time that can't be accounted for by gravity, and whose cause is therefore to be inquired into. Such a diminution would be caused by the resistance of a medium disseminated through the celestial regions; and as there are other good reasons for believing this to be a *vera causa* [see Glossary] it has therefore been ascribed to such a resistance.' [The idea is: resistant medium → slower movement → less propulsive force relative to centripetal force → greater tendency towards the sun → shorter journey → shorter time.]

[Herschel's next example is actually not a use of the Method of Residues, Mill says. There are several more, but we can settle for one more, introduced again by Herschel:]

'Unexpected and striking confirmations of inductive laws frequently occur in the form of residual phenomena, during investigations that are nothing like the ones that led to the inductions themselves. An elegant example is the unexpected confirmation of the law of *the development of heat in elastic fluids by compression*, which is provided by the phenomena of sound. The inquiry into the cause of sound had led to conclusions about its mode of propagation, from which its velocity in the air could be precisely calculated. The

calculations were performed, and the results were near enough to right to show the general correctness of the theory about the cause and the mode of propagation; but this theory couldn't be shown to account for all the sound's velocity. There was still a residual velocity to be accounted for, and for a long time this remained a puzzle. Eventually Laplace had the nice idea that it might come from the heat developed by the condensation that necessarily takes place at every vibration by which sound is conveyed. This matter was subjected to exact calculation, and the immediate result was the complete explanation of the residual phenomenon, and a striking confirmation of the general law of the development of heat by compression, under circumstances beyond artificial imitation.'

§6. Whewell has expressed an unfavourable opinion of the utility of the Four Methods, as well as of the aptness of the examples by which I have tried to illustrate them. He writes:

> 'The obvious thing to say about these methods is that they take for granted the very thing that it's hardest to discover, the reduction of the phenomena to formulae such as are here presented to us. When we have any set of complex facts offered to us. . . ., and we want to discover the law of nature that governs them—or, if you want to put it this way, the feature in which all the cases agree—where are we to look for our A, B, C, and *a*, *b*, *c*? Nature doesn't present the cases to us in this form; and how are we to reduce them to this form? You say when we find the combination of A B C with *a b c* and A B D with *a b d*, then we may draw our inference. Granted; but when and where are we to find such combinations? Even now that the discoveries are made, who will point out to us what are the A, B, C, and *a*, *b*, *c*, elements of

the cases that have just been enumerated? [He has cited ones from astronomy, mechanics, optics, and chemistry.] Who will tell us which of the methods of inquiry those historically real and successful inquiries exemplify? Who will carry these formulae through the history of the sciences, as they have really grown up, and show us that these four methods have been operative in their formation; or that any light is thrown upon the steps of their progress by reference to these formulae?' He adds that in this work ·of mine· the methods haven't been applied 'to a large body of conspicuous and undoubted examples of discovery, extending along the whole history of science'; which ought to have been done if the methods were to be shown to have the advantage. . . .of being those 'by which all great discoveries in science have really been made'.

These objections against the Canons of Induction are strikingly like the 18th century objections, by men as able as Whewell, against the acknowledged Canon of Ratiocination. Those who protested against the Aristotelian logic said of the syllogism what Whewell says of the inductive methods, namely that it 'takes for granted the very thing that is most difficult to discover, the reduction of the argument to formulae such as are here presented to us'. The great difficulty, they said, is to obtain your syllogism, not to judge its correctness when obtained. On the matter of fact, they and Whewell are right. The greatest difficulty in both cases is •obtaining the evidence and then •reducing it to the form that tests its conclusiveness. But if we try to reduce it without knowing what it's to be reduced *to* we're not likely to make much progress. It's harder to solve a geometrical problem than to judge whether a proposed solution is correct; but if people couldn't judge the solution when it was found, they would have little chance of finding it. And it can't be maintained that to judge an induction once it has been found

is perfectly easy, a thing for which aids and instruments are superfluous; for erroneous inductions, •false inferences from experience, are quite as common as—and on some subjects much commoner than—•true ones. The business of inductive logic is to provide rules and models (such as the syllogism and its rules are for ratiocination) such that inductive arguments are conclusive if, and only if, they conform to them. That's what the four methods claim to be, and what I believe they are considered to be by all experimental philosophers, who had practised all of them long before anyone tried to reduce the practice to theory.

The assailants of the syllogism also anticipated Whewell in the other branch of his argument. They said that no discoveries were ever made by syllogism; and Whewell seems to say, that none were ever made by the four methods of induction. To the former objectors Whately gave a good answer, namely that if their argument was any good it was good against the reasoning process altogether; for whatever can't be reduced to syllogism isn't reasoning. And Whewell's argument, if good at all, is good against all inferences from experience. In saying that no discoveries were ever made by the four methods, he affirms that none were ever made by observation and experiment; for assuredly if any were, it was by processes reducible to one or other of those methods.

This difference between us accounts for his dissatisfaction with my examples, which I didn't select with a view to showing that observation and experiment are ways of acquiring knowledge. In choosing them I was thinking only of •illustration, and of •making methods easier to grasp by examples. If I had wanted to justify the processes themselves as means of investigation, I wouldn't have needed to look far off or use recondite or complicated instances. As a specimen of a truth ascertained by the Method of Agreement, I could have chosen the proposition 'Dogs bark'. This dog and that

dog and the other dog answer to A B C and A D E and A F G. The circumstance of being a dog answers to A. Barking answers to *a*. As a truth made known by the Method of Difference, 'Fire burns' might have sufficed. Before I touch the fire I am not burned; this is B C; I touch it, and am burned; this is A B C and *a* B C.

Whewell doesn't regard such familiar experimental processes as inductions; but they are perfectly homogeneous with the ones on which, even on his own showing, the pyramid of science is based. He tries to escape from this conclusion by arbitrarily restricting the range of examples that can serve as instances of induction: they must not be

•things that are still matters of discussion,
•drawn from mental and social subjects, or
•drawn from ordinary observation and practical life.

They must all concern generalisations by which scientific thinkers have ascended to great and comprehensive laws of natural phenomena. Now it is seldom possible in these complicated inquiries to go much beyond the first steps without making use of deduction and the temporary aid of hypotheses—this being something that Whewell and I have maintained against the purely empirical school—so that such cases wouldn't serve well as illustrations of the principles of mere observation and experiment. Whewell is misled by their absence into representing the experimental methods as serving no purpose in scientific investigation, forgetting that if those methods hadn't supplied the first generalisations there would have been no materials for his own conception of induction to work on.

But it's easy to answer his challenge to say which of the four methods are involved in certain important scientific developments. The planetary paths, as far as they are a case of induction at all [see page 147], involves the Method of Agreement. The law of 'falling bodies', namely that they cover distances proportional to the squares of the times, was historically a deduction from the first law of motion; but the experiments that verified it and could have led to its discovery involved the Method of Agreement; and the apparent variation from the true law caused by air-resistance was cleared up by experiments *in vacuo*, involving the Method of Difference. . . . The movements of comets were determined by highly complex processes of thought in which deduction was predominant, but the Methods of Agreement and of Concomitant Variations had a large part in establishing the empirical laws. Every case. . . .is a well-marked example of the Method of Difference. To anyone acquainted with the subjects—to Whewell himself—there wouldn't be the slightest difficulty in setting out 'the A B C and *a b c* elements' of these cases.

If discoveries are ever made by observation and experiment without deduction, the four methods are methods of •discovery; but even if they weren't, they would still be the sole methods of •proof; and they could serve as proofs even of the results of deduction. The great generalisations that begin as hypotheses must end by being proved, and in due course I'll show that they are in fact proved by the four methods. Now logic is principally concerned with proof as such. This approach has no chance of finding favour with Whewell, because his system has the special feature that it doesn't recognise any need for proof in cases of induction. If an hypothesis is carefully collated with facts, and nothing inconsistent with it turns up—i.e. if experience doesn't disprove it—Whewell is content, at least until we find a simpler hypothesis that is equally consistent with experience. If this is induction, doubtless there is no need for the four methods. But to suppose that it is induction seems to me a radical misunderstanding of the nature of the evidence for physical truths.

There's a real practical *need* for a test for induction, like the syllogistic test of ratiocination. Inferences that defy the most elementary notions of inductive logic are confidently presented by persons eminent in physical science, as soon as they are off the factual ground that they know. . . . As for educated persons in general, I doubt that they are better judges of a good or a bad induction than they were before Bacon wrote. The improvement in the •results of thinking has seldom extended to the •processes; and if it *has* reached any process it has been that of investigation only and not that of proof. No doubt a knowledge of many laws of nature has been arrived at by forming hypotheses and finding that the facts corresponded to them; and many errors have been cured by coming to know facts that were inconsistent with them, but not by discovering that the mode of thought that led to the errors was itself faulty and could have been known to be faulty independently of the facts that disproved the specific conclusion. The upshot is that while mankind's thoughts on many subjects have worked out well in practice, the thinking power remains as weak as ever. In all subjects where the facts that would check the result are not accessible—e.g. in what relates to the invisible world, and even. . . .to the visible world of the planetary regions—men with the greatest scientific acquirements argue as pitiably as the merest ignoramus. They have made many sound inductions, but they haven't learned from them—and Whewell thinks there is no need for them to learn—the principles of inductive *evidence.*

Chapter 10. Plurality of causes, and the intermixture of effects

§1. In my account of the four methods of observation and experiment by which we contrive to sort out among a mass of coexistent phenomena the particular effect of a given cause, or the particular cause of a given effect, I have had to suppose for simplicity's sake that this analytical operation doesn't run into difficulties other than the ones that are essentially inherent in its nature. So I have represented every effect as connected exclusively with a single cause, and as incapable of being confusingly mixed in with any other coexistent effect. I have regarded *a b c d e*, the aggregate of the phenomena existing at any moment, as consisting of dissimilar facts—*a* and *b* and *c* and *d* and *e*—for each of which we need to look for just one cause; the difficulty being only that of singling out this one cause from the multitude of antecedent circumstances A, B, C, D, and E. The cause may indeed not be simple; it may consist of an assemblage of conditions; but I have supposed that there's only one possible assemblage of conditions from which the given effect could result.

If that were right, it would be comparatively easy to investigate the laws of nature. But the supposition is false in both its parts. **(i)** It's not true that the same phenomenon is always produced by the same cause; the effect *a* may sometimes arise from A, sometimes from B. **(ii)** And the effects of different causes are often not dissimilar but homogeneous, and not demarcated by any assignable boundaries; A and B, instead of producing *a* and *b*, may produce different parts of an effect *a*. Investigation of the laws of phenomena is made much harder and darker by the need to take account of these two circumstances: intermixture of effects, and plurality of causes. I'll take the latter first, because it is the simpler of

the two. ·I'll start on the intermixture of effects in section 4·.

·Here's the situation that we face·. It's not true that one effect must be connected with only one cause or assemblage of conditions; i.e. that each phenomenon can be produced in only one way. There are often several independent ways in which the same phenomenon could have originated. . . . Many causes can produce mechanical motion; many causes can produce some kinds of sensation; many causes can produce death. It can happen that a given effect was produced by a certain cause but could perfectly well have been produced without it.

§2. One of the principal consequences of this fact of plurality of causes is to bring uncertainty into the Method of Agreement. I illustrated that method by supposing two instances:

•A B C followed by *a b c*, and

•A D E followed by *a d e*.

To avoid a difficulty that isn't relevant to my present theme, let us suppose that we know for sure that the two cases have no antecedent in common except A. Then it might seem that we have a basis for concluding that A is an invariable antecedent of *a*, and even that it is its unconditional invariable antecedent, i.e. its cause. But the moment we admit the possibility of a plurality of causes, that conclusion fails. Why? Because it tacitly assumes that *a* must have been produced in both instances by the same cause. If there could have been two causes, they might have been (for example) C and E; with C causing *a* in the former of the instances and E in the other, and A having no influence in either case.

Suppose we investigate the circumstances of the upbringing and history of two great artists (or it could be two great philosophers, two extremely selfish men, or two extremely generous men) and find that their antecedents agree only in one circumstance x; would it follow that x was what caused each to be a great artist (or a great philosopher or. . .)? Not at all! The causes that can produce any type of character are very numerous; and the two persons could have been just as alike in character without there being *any* resemblance between their previous histories.

This is a **characteristic imperfection** of the Method of Agreement, from which the Method of Difference is free. For if we have two instances A B C and B C, of which B C gives *b c*, and the addition of A converts it into *a b c*, it's certain that at least in this instance A was either the cause of *a* or an indispensable portion of its cause, even if in other instances *a* is produced by entirely different causes. Plurality of causes, therefore, doesn't make the Method of Difference less reliable, and doesn't even require a greater number of observations or experiments; two instances, one positive and the other negative, are still enough for a complete and rigorous induction. Not so with the Method of Agreement. The conclusions that *it* yields when the number of instances is small are of no real value unless they function as *suggestions* that may lead either to •experiments bringing them to the test of the Method of Difference or •to reasonings that can explain and verify them deductively.

When the instances are indefinitely multiplied and varied and *still* suggest the same result, then (and only then) we have an independently valuable result. If the only instances ·of production of *a*· are A B C and A D E, though these instances have nothing in common except A, the effect *a* may have been produced in the two cases by different causes so that there's at most only a slight probability in favour of A; there *may be* causation but it's almost equally probable that there was only a coincidence. But the oftener we repeat the observation, varying the circumstances, the more we advance toward a solution of this doubt. For if we try A F G, A H K, etc., all unlike except in containing A, and if we find

the effect a appears in all these cases, we must suppose one of two things: •that a is caused by A, or •that it has as many different causes as there are instances. With each addition, therefore, to the number of instances, the presumption is strengthened in favour of A. The inquirer will take any chance he gets to exclude A from one of these combinations—let's say from A H K—and by trying H K separately bring the Method of Difference to the aid of the Method of Agreement. Only the Method of Difference can show us that A is the cause of a; but the Method of Agreement, provided the instances are numerous and sufficiently various, can put it beyond any reasonable doubt that A is either the cause of a or an effect of the cause of a.

How many varied instances with only A in common does it take to •rule out the supposition of a plurality of causes and •make it virtually certain that a is connected with A? We mustn't dodge this question, but the consideration of it belongs to the theory of probability, which I'll come to in chapter 17. Still, we can see right away •that the conclusion does amount to a practical certainty after a sufficient number of instances, and thus •that the method isn't radically discredited by the **characteristic imperfection**. There are two upshots to these considerations, **(1)** We see a new source of inferiority in the Method of Agreement, and new reasons for never resting content with results obtained by it without trying to confirm them either by the Method of Difference or by connecting them deductively with some law already ascertained by that superior method. **(2)** We learn the true theory of the value of mere number of instances in inductive inquiry. The plurality of causes is the only reason why mere number is of any importance. Unscientific inquirers tend to rely too much on number, without analysing the instances—without looking into their nature closely enough to see what circumstances are or aren't eliminated by means

of them. Most people hold their conclusions with a degree of assurance proportioned to the mere mass of the experience they appear to rest on, overlooking the fact that by adding instances to instances, differing from one another only in details already recognised as immaterial, nothing whatever is added to the force of the conclusion. A single instance eliminating some antecedent that existed in all the other cases is of more value than the greatest multitude of instances that are reckoned by their number alone. We do of course have to assure ourselves, by repetition of the observation or experiment, that we haven't committed any error concerning the individual facts observed; and until we are sure about this our primary need is not to vary the circumstances but to repeat the same experiment or observation, very carefully, without any change. But once we have this assurance, the multiplication of instances that don't exclude any more circumstances is entirely useless, provided there have been already enough to exclude the supposition of plurality of causes.

This is important: . . . .the Joint Method of Agreement and Difference is not affected by the characteristic imperfection of the Method of Agreement. In the joint method it is supposed not only that •the instances in which a is ·an effect· agree only in containing A, but also that •the instances in which a is not ·an effect· agree only in not containing A. If that's how things stand, A must be not only •the cause of a but •the only possible cause; for if there were another—say, B—then in the instances in which a is not ·an effect· B must have been absent as well as A, and it wouldn't be true that these instances agree only in not containing A. This is an immense advantage of the •joint method over the •simple Method of Agreement. It may seem, indeed, that the advantage belongs to the negative part of the joint method rather than to the method as a whole. The Method of Agreement, when

applied to negative instances (i.e. ones where a phenomenon does not take place), is certainly free from the characteristic imperfection which affects it in the affirmative case. So you might think that the negative premise could be worked as a simple case of the Method of Agreement, with no need for an affirmative premise to go with it. But though this is true in principle, it's usually impossible to work the Method of Agreement by negative instances without positive ones, because it's so much harder to exhaust the field of negation than the field of affirmation. For example: if we are inquiring into what makes bodies transparent, what are our chances of success if we try to discover what the various substances that *aren't* transparent have in common. We are more likely to succeed in seizing some point of resemblance among the comparatively few and definite kinds of things that *are* transparent; and when we've done this our natural next task is to look into whether the absence of this one circumstance isn't precisely the respect in which all opaque substances will be found to be alike.

So the Joint Method of Agreement and Difference. . . .is, after the Direct Method of Difference, the most powerful of the instruments of inductive investigation that I haven't yet discussed; and in the sciences that depend on pure observation with little or no aid from experiment, this method—so well illustrated by the theorising about the cause of dew—is the primary resource, so far as direct appeals to experience are concerned.

§3. Up to here I have treated plurality of causes only as a *possible* supposition that makes our inductions uncertain until we have eliminated it; and have considered how we can eliminate it in cases where there isn't in fact a plurality of causes. But we must also consider it as something that actually occurs in nature, and find ways for our methods of

induction to be able to identify the cases where it does occur. We don't need any special method for doing this. When an effect really could be produced by either of two (or more) causes, the process for detecting them is exactly the same as the process for discovering single causes. They may (first) be discovered as separate sequences, by separate sets of instances—i.e. of observations or experiments—showing that the causes of heat include

- •the sun,.
- •friction,
- •percussion,
- •electricity,
- •chemical action,

with each of these being shown by its own special set of instances. Or (secondly) the plurality may come to light when we are collating a number of instances in an attempt to find something that they all have in common. A *failing* attempt: we can't find anything that is common to all instances of heat; we find that no one antecedent is present in all the instances, no one of them indispensable to the effect. But when we look harder we find that although no *one* is always present, *one or other of several* always is. If on further analysis we can detect any common element in these, we may be able to ascend from them to some one cause that is the really operative circumstance in them all. Thus it is now thought that a single *ultimate* source is at work in the production of heat by friction, percussion, chemical action, etc. But if (as continually happens) we can't take this further step, the different antecedents must be noted provisionally as distinct causes each of which is sufficient, unaided, to produce the effect.

I now move from the plurality of causes to the still more special and more complex case of •the intermixture of effects and •the interference of causes with one another. This is the

principal source of complication and difficulty in the study of nature; and we'll soon see that the four inductive methods that I have presented—the only *possible* methods of directly inductive investigation by observation and experiment—are for the most part quite unable to cope with it. Our only means for unravelling the complexities proceeding from the intermixture of effects and the interaction amongst causes is *deduction*; and the four methods can't do much more than supply premises for our deductions and check their conclusions.

§4. A concurrence of two or more causes, not separately producing each its own effect but interfering with or altering one another's effects, happens in two ways. **(1)** In one, exemplified by the joint operation of different forces in mechanics, the separate effects of all the causes continue to be produced, but are compounded with one another and make one total. **(2)** In the other, already mentioned on page 184 and illustrated by the case of chemical action, the separate effects cease entirely and are succeeded by phenomena that are altogether different and governed by different laws.

Of these **(1)** is by far the more frequent, and also the more likely to elude the grasp of •our experimental methods. The exceptional case **(2)** is basically open to being handled by •them. When

> •the laws of the original agents cease to be applicable because a new phenomenon appears that doesn't offer a hand-hold for those laws, e.g. when
> •two gaseous substances, hydrogen and oxygen, are brought together and throw off their special properties and produce water,

in such cases the new fact can be subjected to experimental inquiry, like any other phenomenon; and the elements that are said to compose it can be regarded as the mere agents of its production—the conditions on which it depends, the facts that make up its cause.

The *effects* of the new phenomenon—e.g. the *properties* of water—are as easily found by experiment as the effects of any other cause. But to discover the *cause* of it, i.e. the particular conjunction of agents from which it results, is often difficult enough. **(a)** The origin and actual production of the phenomenon are usually out of reach of our observation. If we couldn't have learned the composition of water until we *found* instances where it was actually produced from oxygen and hydrogen, we'd have been forced to *wait* until someone had the random idea of passing an electric spark through a mixture of the two gases, or inserting a lighted taper into it, merely to see what would happen. **(b)** Many substances can be analysed but can't be recompounded by any known artificial means. **(c)** Even if we could have learned by the Method of Agreement that oxygen and hydrogen are both present when water is produced, no experiments with oxygen and hydrogen separately—no knowledge of their ·separate· laws—could have enabled us to infer deductively that they would produce water. For that we need a specific experiment on the two combined.

Given these difficulties, you might expect that our knowledge of the causes of this class of effects comes either from accident or from the gradual progress of experimentation on the different combinations that the producing agents are capable of. But ·we can often do better than that, because· effects of this kind have the special feature that under certain combinations of circumstances they reproduce their causes. Water results from putting hydrogen and oxygen really closely and intimately together, and correspondingly hydrogen and oxygen result from placing water in certain situations. When that happens the new laws—·i.e. the laws of water·—abruptly

cease and the two gases re-appear separately with their own properties. What is called 'chemical analysis' is the process of searching for the causes of a phenomenon among its effects, or rather among the effects of subjecting it to some other causes.

Lavoisier, by heating mercury to a high temperature in a closed vessel containing air, found that the mercury increased in weight and became 'red precipitate', while the air when examined after the experiment turned out to have •lost weight and •lost its ability to support life or combustion. When red precipitate was exposed to a still greater heat it became mercury again and gave off a gas which *did* support life and flame. . . .

Where two phenomena between whose laws or properties no connection can be traced are thus cause-effect and effect-cause, each capable in its turn of being produced from the other, and each when it produces the other ceases itself to exist (as water is produced from oxygen and hydrogen, and oxygen and hydrogen are reproduced from water); this causation of the two phenomena by one another, each being generated by the other's destruction, is strictly *transformation*. The idea of chemical composition is an idea of transformation, but of a transformation that is *incomplete* because we consider the oxygen and hydrogen to be present in the water as oxygen and hydrogen, discoverable in it if our senses were keen enough. That's a mere a supposition, based solely on the fact that the weight of the water is the sum of the separate weights of the two ingredients. This fact about the weights is an exception to the entire disappearance in the compound of the laws of the separate ingredients. . . .

In these cases, where the heteropathic effect (as I called it on page 184) is merely a transformation of its cause—i.e. where an effect and its cause are also a cause and its effect, and are mutually convertible into each other—the problem of finding the cause resolves itself into the far easier one of finding an effect, which is the kind of inquiry that *can* be performed by direct experiment. But in some cases of heteropathic effects this can't be done. Consider for instance the heteropathic laws of mind; the part of the phenomena of our mental nature that are analogous to •chemical rather than to •dynamical phenomena; as when a complex passion is formed by the coalition of several elementary impulses, or a complex emotion by several simple pleasures or pains of which it is the result without being the aggregate or in any way homogeneous with them. in these cases the product is generated by its various factors, but the factors can't be reproduced from the product. . . . We can't ascertain what simple feelings any of our complex states of mind are generated from, in the way we ascertain the ingredients of a chemical compound by making it generate them. So our only way to discover these laws is the slow process of studying the simple feelings themselves, and learning by experimenting on the various combinations they're capable of, what they can generate by their interactions.

§5. One might have thought that the other and apparently simpler sort of the mutual interference of causes, where each cause continues to obey the laws that it conformed to in its separate state, would have presented **fewer** difficulties to the inductive inquirer than does the one I have been discussing. In fact, however, it presents—so far as direct induction without help from deduction is concerned—infinitely **greater** difficulties. When a concurrence of causes gives rise to a new effect that has no relation to the separate effects of those causes, the resulting phenomenon stands out undisguised. . . ., presenting no obstacle to our recognising its presence or absence among any number of surrounding phenomena. So it can easily be brought under the canons

of induction if instances of the right kind can be obtained. And the non-occurrence of such instances, or the lack of means to produce them artificially, is the real and only difficulty in such investigations—not a *logical* difficulty but in some way a *physical* one. What I (in chapter 5) called the 'composition of causes' is not like that. There, the effects of the separate causes don't terminate and give place to others, thereby ceasing to form any part of the phenomenon to be investigated; on the contrary, they hold their place but are intermingled with, and disguised by, the homogeneous and closely allied effects of other causes. They are no longer

   a, b, c, d, e

existing side by side and continuing to be separately discernible; rather, they are

   $+a$, $-a$, $\frac{1}{2}b$, $-b$, $2b$, etc..

some of which cancel one another, while many others don't appear separately but merge in one sum. Between •their over-all result and •the causes by which it was produced there's often an insurmountable difficulty in seeing any fixed relation whatever.

We have seen that according to the general idea of the composition of causes:

> Two or more laws interfere with one another, apparently frustrating or modifying one another's operation, yet in reality all are fulfilled, the collective effect being the exact sum of the effects of the causes taken separately.

As a familiar example, think about a body in equilibrium by two equal and opposite forces. One on its own would carry the body in an hour one mile westward, the other on its own would carry it in an hour one mile eastward; and the result ·of the equilibrium· is just the same as if the body had first been carried westward by one force and then back eastward by the other, being finally left where it was at first.

Every causal law L_1 is liable to be counteracted—seemingly frustrated—by coming into conflict with another law L_2 (or more than one) the separate result of which is opposite to L_1's, more or less inconsistent with it. The result of that is that many instances in which L_1 really is entirely fulfilled don't at first sight seem to involve its operation at all. An example of that is the west-east one that I just offered: a 'force' in mechanics means precisely a 'cause of motion', but it can happen that the sum of the effects of two causes of motion is rest ·= motionlessness·. Another example: a body subjected to two forces in different directions moves in the diagonal; and it seems a paradox to say that motion in the diagonal is the sum of two motions in two other lines. [The 'diagonal' referred to here is, as Mill explained on page 161, the diagonal of a parallelogram whose sides represent the direction and strength of those two forces.] Motion, however, is merely change of place, and at every instant the body is in the exact place it would have been in if the forces had acted during alternate instants instead of acting in the same instant (except that we must of course allow the forces double the time if they're to do successively what they in fact do simultaneously). So it's clear that each force has had during each instant its own full effect, and that the *modifying* influence that C_2 is said to exercise with respect to C_1 can be seen as exerted not over the action of C_1 itself but over the effect after C_1 has done its work. For all purposes of predicting, calculating, or explaining their joint result, causes that compound their effects can be treated as if •they produced their own separate effects simultaneously, and •all these effects coexisted visibly.

Because the laws of causes are just as completely fulfilled •when the causes are 'counteracted' by opposing causes as they are •when they are left to their own undisturbed action, we must take care not to express the laws in terms that would make the assertion of their being fulfilled in those

cases a contradiction. For example, if we said that there's a law of nature according to which

> any body to which a force is applied moves in the direction of the force with a velocity directly proportional to the force and inversely proportional to its own mass,

when in fact

> some bodies to which a force is applied don't move at all, and the ones that do move (at least in the region of our earth) are from the very first held back by the action of gravity and other resisting forces, and eventually stopped altogether,

it's clear that the general proposition—·the supposed 'law'·—though true under a certain hypothesis doesn't express the facts as they actually occur. To get the expression of the law to fit the real phenomena we must say not that the object *moves* in the direction and with the velocity specified but that it *tends* to move in that way. We could guard our expression in a different way by saying that the body moves in that manner *unless prevented*, or *except to the extent that it is prevented*, by some counteracting cause. But ·that's less good, because· the body doesn't merely

> move in that manner unless counteracted;

it also

> tends to move in that manner even when it is counteracted.

It still exerts, in the original direction, the same energy of movement as if its first impulse had been undisturbed, and produces by that energy an exactly equivalent quantity of effect. This is true even when the force leaves the body as it found it, in a state of absolute rest. Suppose we are trying to raise a body weighing three tons with a force equal to one ton; if while we're doing this wind or water or any other agent supplies an additional force of just over two tons, the body will be raised—proving that the force we applied exerted its full effect by neutralizing an equivalent part of the total weight. And if, while we're exerting this force of one ton on the object in a direction contrary to that of gravity, it is put onto a scale and weighed, it will be found to have lost a ton of its weight—i.e. to press downward with a force equal to only the difference between the two forces.

These facts are correctly indicated by the term 'tendency'. Because laws of causation can be counteracted, they should all be stated in terms of tendencies only, not actual results. . . .

The habit of neglecting this needed element in the precise expression of the laws of nature has given rise to the popular [see Glossary] prejudice that all general truths have exceptions; and this has brought much unmerited distrust to the conclusions of science when they have been submitted to the judgment of minds insufficiently disciplined and cultivated [see Glossary]. The rough generalisations suggested by common observation usually do have exceptions; but principles of science—i.e. laws of causation—don't. Let me quote from an earlier work of mine [from here to the end of this section]: 'What is thought to be an exception to a principle is always some *other* principle cutting into the former, some other force that impinges against the first force and pushes it off-course. We do *not* have this:

> a law and an exception to it, the law acting in 99 cases and the exception in one.

What we do have is this:

> Two laws, each possibly acting in the whole 100 cases, bringing about a common effect by their joint operation.

If in a single case the less conspicuous force—called the "disturbing" force—prevails sufficiently over the other force to create what is commonly called an "exception", the same

disturbing force probably acts as a *modifying* cause in many other cases that no-one will call exceptions.

'Thus if it were said to be a law of nature that **all heavy bodies fall** to the ground, it would probably be said that the resistance of the atmosphere that prevents a balloon from falling makes the balloon an "exception" to that supposed law of nature. But the real law is that **all heavy bodies tend to fall**; and there are no exceptions to this, not even the sun and moon, because (as every astronomer knows) they tend toward the earth with a force exactly equal to that with which the earth tends toward them. . . .' [From Mill's *Essays on Some Unsettled Questions of Political Economy*, Essay 5.]

§6. We now have to face the question: *How* are we to study these complex effects made up of the effects of many causes? *What* enable us to trace each effect back to the concurrence of causes in which it originated, and learn what the circumstances are in which it may be expected to recur?

The conditions of a phenomenon that arises from a composition of causes can be investigated either •deductively or •experimentally.

It's obvious that the deductive mode of investigation is appropriate to this kind of case. The law governing an effect of this sort, x, is an upshot of the separate laws governing the causes that jointly produced x, so it is in itself capable of being deduced from these laws. This is called the *a priori* method. The *a posteriori* method claims to proceed according to the canons of experimental inquiry. Considering the whole assemblage of causes that jointly produced x as one single cause, it tries to ascertain the cause in the ordinary manner, by a comparison of instances. There are two varieties of this second method. If it merely assembles and compares instances of the effect, it's a method of pure observation. If it operates on the causes and tries different combinations of them in hopes of eventually hitting the precise combination that will produce the given total effect, it is a method of experiment.

So we have three methods: deductive, observational, and experimental. In order get clearer about the nature of each, and determine which of them deserves preference, I shall 'clothe them in circumstances' (Lord Eldon's phrase). I'll select for this purpose a case that hasn't yet provided a brilliant example of the success of any of the three methods, but does illustrate the difficulties inherent in them. Let's suppose we are inquiring into. . . .the conditions of recovery from a given disease x; and for a start let our question be limited to: 'Is mercury a remedy for x?'

•The deductive method would set out from known properties of mercury and known laws of the human body, and by reasoning from these would try to discover whether mercury will act on an x-afflicted body in a manner that would tend to restore health. •The experimental method would simply administer mercury in as many cases as possible, noting the age, sex, temperament, and other details of bodily constitution, the particular form or variety of x, the particular stage of its progress etc., noting in which of these cases it led to a salutary effect, and what circumstances it was combined with on those occasions. •The method of simple observation would compare instances of recovery, to find whether they agreed in having been preceded by the administration of mercury; or would compare instances of recovery with instances of failure, to find cases that were like the others except that in them mercury had been administered, or. . . .that it hadn't.

§7. No-one has ever seriously contended that the last of these three methods of investigation could work with composite causes. No useful conclusions on a subject of such intricacy were ever obtained in that way. The most one could

get would be •a vague general impression for or against the efficacy of mercury, and •that would be practically useless unless it were confirmed by one of the other two methods. The results that this method tries to obtain would be of the utmost possible value *if* they could be obtained. If in an examination of a great number of instances all recoveries were cases in which mercury had been administered, we could generalise with confidence from this experience, and would have obtained a conclusion of real value. But in a case of this sort we have no chance of getting a basis for such a generalisation. Why not? because of what on page 215 I called the 'characteristic imperfection' of the Method of Agreement, namely the plurality of causes. Even if mercury does tend to cure the disease, so many other natural and artificial causes also tend to cure it that there are sure to be abundant instances of recovery in which mercury has not been administered. . . .

When an effect results from the union of many causes, no one of them can have a large role in determining •whether the effect follows or, if it does, •what it is like in detail. Recovery from a disease is an event that always comes from many influences acting together. Mercury may be one such influence; but there are bound to be cases where it is administered but the patient doesn't recover because other needed influences aren't at work, or where mercury isn't given but the patient recovers because the other favourable influences are powerful enough to do this in its absence. . . . About the best that this method could do for us is to show—by multiplied and accurate hospital records and the like—that there are rather more recoveries and rather fewer failures when mercury is administered than when it isn't. But that result would have little value as a guide to practice, and virtually none as a contribution to the theory of the subject.[1]

§8. Having recognised the inapplicability of the method of simple observation to ascertain the conditions of effects that have many concurring causes, let us now ask whether any greater benefit can be expected from the other branch of the *a posteriori* method—the one that directly *tries* different combinations of causes. . . .and takes note of their effects; e.g. trying the effect of mercury in as many different circumstances as possible. This method differs from the previous one in turning our attention directly to the causes or agents, instead of turning it to the effect, recovery from the disease. As a general rule the effects of causes are easier to study than the causes of effects, so it's natural to think that this method has a better chance of succeeding than the previous one.

The method now under consideration is called the Empirical Method; and to estimate it fairly we must take it to be *completely* empirical, without any input from any deductive operation. We might do this:

> Try experiments with mercury on a healthy person in order to ascertain the general laws of its action upon the human body, and then reason from these laws to determine how it will act on persons affected with a particular disease,

and this might be a really effectual method; but it is deduction. The experimental method doesn't derive the law

---

[1]   Bain rightly says that though the Methods of Agreement and Difference are not applicable to these cases, the Method of Concomitant Variations is of some use with them: 'If a cause happens to vary alone, the effect will also vary alone: a cause and effect may be thus singled out under the greatest complications. . .' [Mill says that this is correct in theory, but:] when there are many influencing causes, no one of them greatly predominating over the rest, and especially when some of them are continually changing, it is scarcely ever possible to trace a relation between •the variations of the effect and •those of any one cause in a way that would enable us to assign to that cause its real share in the production of the effect.

of a complex case from the simpler laws that jointly produce it, but experiments directly on the complex case. We must set aside entirely all knowledge of the simpler tendencies of mercury in detail. Our experimentation must try to get a direct answer to the specific question: 'Does mercury tend to cure the particular disease or doesn't it?'

Let us see how far the rules of experimentation that have to be followed in other cases can be followed in the ·multiple-cause· case. [Mill's handling of this question is long, detailed and demanding, and leads him to conclude that the rules *can't* be obeyed in this 'case'. At every turn we encounter possibilities of error that we can't exclude because of the complexities of the multiple-cause situation. This sums it up:]

Anything like a scientific use of the method of experiment in these complicated cases is out of the question. We can generally, even in the most favourable cases, only discover by a series of trials that a certain cause is *very often* followed by a certain effect. Anything like a scientific use of the method of experiment is therefore out of the question in these complicated cases. Even in the most favorable cases we can generally only discover by a series of trials that a certain cause is *very often* followed by a certain effect. . . .

If so little can be done by the experimental method to understand multiple-cause situations in **medical science**, still less is this method applicable to a class of phenomena even more complicated than those of physiology—the phenomena of **politics and history**. In that region plurality of causes exists in almost boundless excess, and most effects are inextricably interwoven with one another. To make things still worse, most inquiries in political science relate to the production of very large-scale effects such as •the public wealth, •public security, •public morality and the like; and these items are open to being affected—directly or indirectly,

helped or hindered—by nearly every fact that exists or event that occurs in human society. The vulgar notion that the safe methods on political subjects are those of Baconian induction—that the true guide is not general reasoning but specific experience—will some day be referred to as one of the clearest signs of a low state of theoretical thinking in any age in which it is accepted! Nothing can be more ludicrous than the parodies of experimental reasoning that we encounter not only in popular discussion but also in solemn treatises about the affairs of nations:

> •'How can an institution be bad, when the country has prospered under it?'
> •'How can such-and-such a cause have contributed to the prosperity of one country, when another has prospered without it?'

Anyone who argues like this, not intending to deceive, should be sent back to learn the elements of some one of the easier physical sciences! Such reasoners ignore the fact of plurality of causes in the very case that provides the most obvious example of it. [Mill adds that there's little 'reason for regret' in our inability to perform experiments in this area; because even if we could perform them, we would be comprehensively defeated by the scope and complexity of the material.] The nearest approach to an *experiment*—in the philosophical [here = 'scientific'] sense of the term—in politics is the introduction of a new operative element into national affairs by some particular identifiable measure of government, such as the enactment or repeal of a particular law. But where there are so many influences at work, it takes time for the influence of a new cause on national phenomena to become apparent; and as the causes operating in such an extensive sphere are not only •infinitely numerous but •in a state of perpetual alteration, it is always certain that before the effect of the new cause becomes conspicuous enough to be a subject of

induction, many of the other influencing circumstances will have changed, wrecking the experiment.[1]

Thus, two of the three possible methods for the study of phenomena resulting from a combination of many causes are, from the very nature of the case, inefficient and illusory.

That leaves us with the third—the method that considers the causes separately and infers the effect from the balance of the different tendencies that produce it; i.e. the deductive or *a priori* method. A detailed consideration of this intellectual process requires a chapter to itself.

Chapter 11. The deductive method

§1. Given that the direct methods of observation and experiment can't help us to grasp the conditions and laws of recurrence of the more complex phenomena, our main source of knowledge of those phenomena has to be the Deductive Method. It consists of three operations; •direct induction, •ratiocination, and •verification.

First operation, Induction: I call the first step in the process an 'inductive' operation, because there must be a direct induction as the basis of the whole. The role of the induction may often be played by •a prior deduction, but the premises of •this must have been derived from induction.

The problem of the Deductive Method is to find the law of an •effect from the laws of the different tendencies of which •it is the joint result. What is needed first, then, is to know the laws of those ·separate· tendencies, i.e. the law of each of the concurrent causes; and this requires, for each cause separately, a previous process of observation or experiment or else a previous deduction whose ultimate premises come from observation or experiment. If we're investigating social or historical phenomena, the premises of the Deductive Method must be the laws of the causes that determine such phenomena; and those causes are •human actions together with •the general external circumstances by which mankind are influenced.... So the Deductive Method as applied to social phenomena must start by investigating....the laws of human action, and the properties of external things that determine the actions of human beings in society. Some of these general truths will be obtained by observation and experiment, others by deduction (e.g. deducing the more complex laws of human action from the simpler ones); but the simple or elementary laws must have been obtained by a directly inductive process.

---

[1] Though Bain generally agrees with the views expressed in this chapter, he seems to estimate more highly than I do the scope for specific experimental evidence in politics. He is right when he says that there are some cases 'when an agent suddenly introduced is almost instantaneously followed by some other changes, as when the announcement of a diplomatic rupture between two nations is followed the same day by a derangement of the money-market'. But this 'experiment' would be quite inconclusive merely as an experiment. It can only serve, as any experiment can, to verify the conclusion of a deduction. Unless we already knew from our knowledge of business men's motives that the prospect of war *tends* to derange the money-market, we would never have been able to prove a connection between those two facts. What if we ascertained throgh historical study that one followed the other in a great number of instances? Anyone who has carefully examined any of the attempts—they're continually being made—to prove economic doctrines by such a recital of instances knows very well how futile they are. It turns out that the circumstances of the cases have hardly ever been fully stated, and that the records have omitted as many or even more instances that would have tended to an opposite conclusion.

Learning what the separate causes are that must be studied in this way is sometimes hard, sometimes easy. In the social-phenomena case it is easy. There could never have been any doubt that social phenomena depend on *the acts and mental impressions of human beings*, however little may have been known about what laws govern those impressions and actions, or what social consequences their laws naturally lead to. ·Another easy case:· After physical science had achieved a certain development, there was no real doubt about where to look for the laws on which *the phenomena of life* depend: they had to be the mechanical and chemical laws of the solid and fluid substances composing the organised body and the medium in which it lives, together with the special life-laws of the various tissues constituting the organic structure. ·A hard case:· With celestial phenomena it was much less obvious in what direction the causes were to be looked for (although the relevant causal structures were far simpler than those of society and of life). What happened was this: scientists combined the laws of certain causes and discovered that those laws •explained all the facts that experience had proved concerning the heavenly motions, and •led to predictions that always turned out to be true. It was only then that mankind knew that those *were* the causes. But whether we can put the question before we can answer it (society, life) or can't state it until we have become able to answer it (celestial phenomena), either way it must be answered, and the laws of the different causes ascertained, before we can deduce from them the conditions of the effect.

The mode of ascertaining those laws is the fourfold method of experimental inquiry that I already discussed—no other way is even possible. All I need add are a few remarks on the application of that method to cases of the composition of causes.

Obviously, we can't expect to find the law of a tendency

through an induction from cases where the tendency is counteracted. The laws of motion couldn't have been brought to light from the observation of bodies kept motionless by the equilibrium of opposing forces. Even when the tendency is not •counteracted but merely •modified by having its effects combined with the effects of some other tendency, we are still not well placed to extract from the tangle the law of the tendency itself. It would hardly have been possible to discover the law that *every body in motion tends to continue moving in a straight line* by an induction from instances in which the motion is deflected into a curve by some external force. In such cases the Method of Concomitant Variations can give some help; but still the principles of a judicious experimentation prescribe that the law of each of the tendencies should be studied, if possible, in cases where that tendency operates •alone or •in combination only with agencies whose effects can be calculated and allowed for.

Accordingly, in cases where the causes can't be separated and observed apart it's *very* hard to lay down with due certainty the inductive foundation needed to support the deductive method. (It's bad luck that such cases are numerous and important.) This difficulty is especially conspicuous in the case of *physiological phenomena*, because it's seldom possible to separate the different agencies that collectively compose an organised body, without destroying the very phenomena we are trying to investigate:

> —following life, in creatures we dissect,
> We lose it, in the moment we detect.
> <div align="right">(Alexander Pope)</div>

For this reason I'm inclined to think that •physiology (greatly and rapidly progressive though it now is) is troubled by greater natural difficulties, and is probably capable of less ultimate perfection, than even •the social sciences. We can

make a better job of studying the laws and operations of one human mind apart from other minds than we can of studying the laws of one organ or tissue of the human body apart from the other organs or tissues.

Pathological facts—i.e. in common language, *diseases*—in their different forms and degrees provide physiologists with the most valuable equivalent to experimentation strictly so-called, because they often show us a definite disturbance in some one organ or organic function, the remaining organs and functions being unaffected, at least for a while. It's true that. . . .there can't be a prolonged disturbance in any one function without eventually involving many of the others; and as soon as this happens the experiment loses most of its scientific value. Everything depends on observing the early stages of the disturbance, which unfortunately are bound to be the least conspicuous. But if organs and functions that aren't disturbed at first become affected in a fixed order of succession, that throws *some* light on one organ's influence over another; and we occasionally get a series of effects that we can with some confidence attribute to the original local disturbance; but to get this benefit we have to know that the original disturbance *was* local. If instead it was 'constitutional' (as they say)—i.e. if we don't know where in the animal's system it started, or exactly what it consisted of—we can't determine which of the various upsets was cause and which was effect—which of them were produced by one another, and which by the direct (perhaps delayed) action of the original cause.

. . . .We can also produce pathological facts artificially. We can try *experiments*, even in the popular sense of the term, by subjecting the living being to some external agent such as the mercury of my former example or cutting a nerve so as to ascertain the functions of different parts of the nervous system. This experimentation isn't intended to obtain a direct solution of any practical question, but rather to discover general laws from which the conditions of any particular effect can be obtained later by deduction; so it's best to select cases whose circumstances can be best ascertained; and those are usually not ones in which there's any practical object in view. The experiments are best tried not in a state of disease, which is essentially a changeable state, but in the condition of health, which is comparatively stable. In sickness, unusual agencies are at work and we can't predict their results; in health the usual course of the physiological phenomena would remain undisturbed if it weren't for the disturbing cause that we introduce.

Such are our inductive resources for ascertaining the laws of the causes considered separately when we ·confront them only in complexes and· don't have any way of separating them out and then investigating them separately. (Actually, the Method of Concomitant Variations sometimes comes to our aid; but it is as burdened as the more elementary methods are by the special difficulties of the subject.

These resources are so glaringly inadequate that the backward state of the science of physiology is no surprise. Indeed our knowledge of causes ·in that science· is so imperfect that it's no surprise we can't explain, and couldn't without specific experience have predicted, many of the facts that we know about from ordinary observation. Fortunately, we're much better informed than this concerning the empirical laws of the phenomena, i.e. the uniformities that we can't yet decide how to classify—whether as •cases of causation or merely •results of it. Not only do we have this:

> The order in which the facts of organisation and life successively manifest themselves, from the first germ of existence to death, has been found to be uniform and very accurately ascertainable

but also

By a great application of the Method of Concomitant Variations to the entire facts of comparative anatomy and physiology, the characteristic organic •structure corresponding to each class of •functions has been determined with considerable precision.

We are quite ignorant as to whether these organic conditions are *all* the conditions—and in many cases we don't even know whether they are conditions at all rather than mere collateral effects of some common cause. And we're not likely ever to know this unless we can construct an organised body and try whether it would live.

Those are the obstacles we encounter in cases of this ·complex-cause· sort when we try to take the first step, the inductive step, in applying the Deductive Method to complex phenomena. But fortunately things are usually not as bad as that. In general, the laws of the causes on which the effect depends can be obtained by an induction from comparatively simple instances.... By 'simple instances' I mean of course ones in which the action of each cause is not much intermixed or interfered with other causes whose laws we don't know. The use of the Deductive Method to ascertain the laws of a complex effect has sometimes had brilliant results, but only when the induction supplying the premises for the Deductive Method has rested on simple instances of that kind.

§2. **Second operation, Ratiocination:** When the laws of the causes have been ascertained, and the first stage of our great logical operation satisfactorily completed, the second part follows: determining from the laws of the ·separate· causes what the effect will be of any given combination of those causes. This is a process of *ratiocination*, and it often involves processes of *calculation* in the narrow sense in which it = *numerical calculation*. When our knowledge of

the causes is so perfect as to extend to the exact numerical laws that they conform to in producing their effects, the ratiocination may include among its premises the theorems of the science of number—and I'm speaking of the whole immense extent of that science. We often need the most advanced truths of mathematics to be able to compute an effect of which we already know the numerical law; and even with the help of those advanced truths we can't get very far. Here's a simple problem:

Given the locations and masses of three bodies that are gravitating toward one another, with a force directly proportional to their mass and inversely proportional to the square of the distance, how do we calculate what their locations will be after n seconds?

All the resources of the calculus haven't yet been able to provide a general solution—only approximations. [And in 2012 there is *still* no complete general solution to the 'three-body problem'.] A slightly more complex case, though still one of the simplest that arise in practice, is that of plotting the motion of a projectile. Even if we know and have numerical values for all the causes that affect the velocity and range of a cannon-ball—the force of the gunpowder, the angle of elevation, the density of the air, the strength and direction of the wind—it's an extremely difficult mathematical problem to put these together so as to calculate their combined effect.

Besides the theorems of number, those of geometry also come in as premises, when we are trying to solve problems in mechanics, optics, acoustics, or astronomy—where the effects take place in space and involve motion and extension. But when the complication increases, and the effects depend on so many and such shifting causes that there's no place for fixed numbers or for straight lines and regular curves, the laws of number and extension are applicable, if at all, only on a large scale where precision of details becomes

unimportant. I'm thinking here of physiology, and even more of mental and social phenomena. Although mathematical laws play a conspicuous part in the most striking examples of the investigation of nature by the Deductive Method—e.g. in the Newtonian theory of the celestial motions—they are by no means an indispensable part of every such process. All that is essential is reasoning from a general law to a particular case—i.e. determining by means of the particular circumstances of that case what result is required in that instance to fulfill the law. Thus in the Torricellian experiment [in effect, the discovery of the barometer], if the fact that air has weight had been previously known it would have been easy, without any numerical data, to deduce from the general law of equilibrium that the mercury would stand in the tube at a height such that the column of mercury would exactly balance a column of the atmosphere of equal diameter. . . .

By such ratiocinations from the separate laws of the causes we can to some extent answer either of the following questions:

- •Given a certain combination of causes, what effect will follow?
- •What combination of causes, if it existed, would produce a given effect?

In the one case, we determine the effect to be expected in any complex circumstances of which the different elements are known; in the other case we learn according to what law—under what antecedent conditions—a given complex effect will occur.

§3. Third operation, Verification: You may want to say:

Those arguments that you used to dismiss as illusory the methods of direct observation and experiment when applied to the laws of complex phenomena— don't they apply with equal force against the Method of Deduction? When in every single instance a multitude (often an unknown multitude) of agencies are clashing and combining, how can we be sure that in our *a priori* computation we have taken them all into account? Aren't there certain to be many that we don't know anything about? Aren't we likely to have overlooked some that we *do* know of? And even if we did take account of them all, that would be useless unless we knew the precise numerical law of each, which we usually don't. And if we did, we would need to make a calculation which, in any but very simple cases, surpasses the utmost power of mathematical science with all its most modern improvements.

These objections have real weight, and would be unanswerable if it weren't for the fact that when we are using the Deductive Method there's a *test* that enables us to judge whether we have committed any of those errors. The application of this test constitutes Verification, the third essential component part of the Deductive Method, without which all the results the method can give amount to little more than conjectures. We aren't entitled to rely on the general conclusions arrived at by deduction unless careful comparison shows us that they fit the results of direct observation wherever that can be had. If we have relevant experience and it confirms them, we may safely trust to them in other cases of which we don't *yet* have specific experience. But if our deductions have led to the conclusion that from a particular combination of causes C a given effect E would result, then in all known cases where C is not followed by E we must be able to show (or at least to make a probable surmise about) what *blocked* E; and if we can't do that the theory is imperfect and not yet to be relied upon. And the verification isn't complete unless some of the cases where the theory is confirmed by the observed result are at least as

complex as any where its application could be called for.

If direct observation and the assembling of instances have provided us with any relevant empirical laws (whether true in all observed cases, or only true for the most part), the best verification the theory could have would be its leading deductively to those empirical laws, so that the complete or incomplete uniformities that were observed among the phenomena were *accounted for* by the laws of the causes. . . . It was very reasonably thought to be an essential requirement of any true theory of the causes of the celestial motions that it should lead by deduction to Kepler's laws—which the Newtonian theory did.

Something else that is important for the verification of theories obtained by deduction. . . .is that the phenomena should be described in the most comprehensive and accurate manner possible;. . . .as when the series of the observed places of a planet was first expressed by a circle, then by a system of epicycles, and subsequently by an ellipse.

Complex instances would have been no use for the discovery of the simple laws into which we ultimately analyse their phenomena, but when they have served to verify the analysis they become additional evidence for the laws themselves. Although we couldn't have discovered the law from complex cases, still when the law—discovered in some other way—is found to be in accordance with the result of a complex case, that case becomes a new experiment on the law, and helps to confirm something that it didn't help to discover. . . . This was strikingly conspicuous in the example [page 211] in which the difference between the observed and the calculated velocity of sound was found to result from the heat developed by the condensation that happens in each sound-vibration. This was a trial, in new circumstances, of the law of the development of heat by compression, and it added materially to the proof of the universality of that law. Accordingly, any

law of nature is regarded as having become more certain by being found to explain some complex case that hadn't previously been thought of in connection with it; and this indeed is a consideration that scientific inquirers customarily value too much rather than too little.

To the Deductive Method—with its three constituent parts, Induction, Ratiocination, and Verification—the human mind is indebted for its most conspicuous triumphs in the investigation of nature. To it we owe all the theories by which vast and complicated phenomena are embraced under a few simple laws that could never have been detected by the direct study of those great phenomena. To get a sense of what the method has done for us, consider the case of planetary motions. Of the greater instances of the composition of causes this is one of the simplest, because (except in a few not very important cases) each heavenly body x can be considered (without material inaccuracy) to be never attracted by more than two bodies at once, •the sun and •one other planet or satellite. So x's motions depend on only four different agents:

  •the sun,
  •the other planet or satellite,
  •the reaction of x itself, and
  •the force generated by x's own motion and acting in
    the direction of the tangent.

This is surely a much smaller number than any of the other great phenomena of nature is determined or modified by. Yet how could we ever have discovered the combination of forces on which the motions of the earth and planets depend by merely comparing the orbits or velocities of different planets, or the different velocities or positions of the same planet ·at different times·? Despite the usual regularity of those motions, and although the periodical recurrence of exactly the same effect shows that all the combinations of

causes that occur at all recur periodically, we wouldn't have known what the causes were if the existence of precisely similar agencies on our own earth hadn't brought the causes themselves within the reach of experimentation under simple circumstances. I'll have occasion in chapter 14 to analyse this great example of the method of deduction, so I shan't spend time on it here. My next topic is a secondary application of the deductive method, the result of which is not to •prove laws of phenomena but to •explain them.

Chapter 12. Explaining laws of nature

§1. When we use the deductive operation to derive the law of an effect from the laws of the causes that jointly give rise to it, we may be engaged in either of two things: •discovering the law or •explaining a law already discovered. The word 'explanation' occurs so continually in philosophy, and has such an important place in it, that a little time spent in fixing its meaning will be well spent.

An individual fact is said to be *explained* when someone points out its cause, i.e. states the law or laws of causation of which its production is an instance. A fire is explained when it is proved to have arisen from a spark falling onto a heap of dry leaves. Similarly a law or uniformity in nature L is said to be explained when someone points out another law or laws •of which L is a special case and •from which it could be deduced.

§2. There are three sets of circumstances in which a law of causation can be explained from other laws—or, as it also is often expressed, *resolved into* other laws.

The **first** is a case that I have already fully considered: a mixture of laws producing a joint effect equal to the sum of the effects of the causes taken separately. The law of the complex effect is explained by being resolved into the separate laws of the causes that contribute to it. For example, •the law of the motion of a planet is resolved into •the law of the acquired force that tends to produce a uniform motion in the tangent and •the law of the centripetal force that tends to produce an accelerating motion toward the sun, the real motion being a compound of those two.

In this resolution of the law L of a complex effect, the laws L is compounded of aren't the only elements. It is resolved into the laws of the separate causes *and the fact of their coexistence*. This is as essential as the other ingredients, whether we are discovering L or only explaining it. To deduce the laws of planetary motions, we have to know not only
  •the law of a rectilineal force and
  •the law of gravitative force, but also
  •the fact that both these forces exist in the celestial regions, and even their relative amount.
The complex laws of causation are thus resolved into two distinct *kinds* of elements: •simpler laws of causation and •collocations, the latter consisting in the existence of certain agents or powers in certain places at certain times. Later on I'll need to return to this distinction and discuss it at some length—enough to remove any need to go on about it here. So: the first kind of explanation of laws of causation occurs when the law of an effect is resolved into the various

231

tendencies of which it is the result, together with the laws of those tendencies. . . .

§3. A **second** kind of explanation of laws occurs when what seemed to be an immediate cause-effect pair turns out to have an intermediate link, a fact caused by the antecedent and in its turn causing the consequent. A seemed to be the ·immediate· cause of C, but we later found that A was the ·immediate· cause only of B and a remote cause of C, and that B was the ·immediate· cause of C. We knew that touching an outward object caused a sensation. We discovered later that •after we have touched the object and •before we experience the sensation •some change occurs in a kind of thread called a 'nerve' that extends from our external organs to the brain. Thus, touching the object is only the remote cause of our sensation—i.e. not *the cause* properly speaking, but *the cause of the cause*—and the real cause of the sensation is the change in the nerve. Future experience may not only •increase our knowledge of the nature of this change, but also •interpolate another link. It may be (for example) that between the contact of the object with our external organs and the change in the nerve there is some electric phenomenon, or some phenomenon unlike anything we now know. No such intermediate link has been discovered up to now, so the touch of the object must be regarded *provisionally* as the immediate cause of the event in the nerve. Thus the sequence

contact with an object → sensation of touch

is discovered not to be an ultimate law; it is 'resolved' (as they say) into two other laws:

contact with an object → event in the nerve, and
event in the nerve → sensation of touch.

Another example: the more powerful acids corrode or blacken organic compounds; this is causation, but ·only· remote causation; and it's said to be explained when it is shown that there's an intermediate link, namely an event in which chemical elements in the organic structure separate from the rest and combine with the acid. The acid causes this separation of the elements, and the separation of the elements causes the disorganisation and often the charring of the structure. . . .

§4. This is important: when a sequence of phenomena is thus resolved into other laws, they're always laws more general than itself. The law that A is followed by C is less general than either of the laws connecting B with C and A with B. Some very simple points will show that this is so.

All laws of causation are liable to be counteracted or frustrated by the non-fulfillment of some negative condition; so B's tendency to produce C may be defeated. Now the law that A produces B is equally fulfilled whether or not B is followed by C; but the law that •A produces C by means of B is of course fulfilled only when B really is followed by C; so it is less general than the law that •A produces B. It is also less general than the law that B produces C. [Mill's defence of this is the same, *mutatis mutandis*, as the one he has just given. And then he has a paragraph applying all this to the touch-nerve-sensation case, including this:] The law that an event in a nerve produces sensation is more general than the law that contact with an object produces sensation, because the sensation equally follows the change in the nerve when it is produced not by contact with an object but by some other cause. . . .

The laws of •more immediate sequence that the •law of a remote sequence is resolved into are not only more general than that law is but also more to be relied on. . . . The tendency of A to produce C can be defeated by whatever can defeat either the tendency of A to produce B or the tendency

of B to produce C; so it is twice as liable to failure as either of those more elementary tendencies; and the generalisation that A is always followed by C is twice as likely to be found erroneous. . . .

The resolution of one generalisation into two others not only •shows that there are possible failures of the former from which its two elements are exempt, but also •shows where these are to be looked for. As soon as we know that B intervenes between A and C we also know that if there are cases where A → C doesn't hold they are most likely to be found by studying the causes and the effects of B.

So we see that in the second of the three ways in which a law can be resolved into other laws, the explaining laws are more general (i.e. cover more cases) and less likely to collide with subsequent experience than is the law they explain. They are
    •more nearly unconditional,
    •defeated by fewer contingencies, and
    •a nearer approach to universal truths of nature.
And all this is still more obviously true of first of the three modes of resolution. When the law of an effect of combined forces is resolved into the separate laws of the causes, the law of the effect must be less general than the law of any of the causes because it only holds when they are combined; whereas the law of any one of the causes holds good both in that combination and out of it. . . .

Here's another strong reason why the law of a complex effect must be less general than the laws of the causes that collaborate to produce it. If two complexes involve the same causes acting according to the same laws, they can still differ in the proportions in which the causes are combined; and that can lead to their having effects that differ not merely in •quantity but in •kind. The combination of •a centripetal force with •a projectile force, in the proportions they have

in all the planets and satellites of our solar system, gives rise to an elliptical motion; but if the ratio between the two forces were slightly different the motion they produced would be in a circle or parabola or hyperbola. . . . The law of each of the concurrent causes remains the same, however their collocations may vary; but the law of their joint effect varies with every difference in the collocations. . . .

§5. There is also a **third** mode in which laws are resolved into one another; and in this it's self-evident that they are resolved into laws more general than themselves. This third mode is the subsuming (as they say) of one law under another, i.e. the gathering up of several laws into one more general law that includes them all. The most splendid example of this occurred when •terrestrial gravity and •the central force of the solar system were brought together under •the general law of gravitation. It had already been proved that the earth and the other planets tend toward the sun; and it had been known from the earliest times that terrestrial bodies tend toward the earth. These were similar phenomena; for them both to be subsumed under one law all that was needed was to prove that as well as being alike in •quality they conform to the same rules as to •quantity. This was first shown to be true of the moon: it resembled terrestrial objects in tending to a centre and indeed in tending toward the earth. After it had been discovered that the moon's tendency toward the earth varied inversely with the square of the distance between them, it was directly calculated that if the moon were as near to the earth as terrestrial objects are, and if the acquired force in the direction of the tangent were suspended, the moon would fall toward the earth through exactly as many feet per second as those objects do by virtue of their weight. So the inference was irresistible •that the moon also tends toward the earth by virtue of its weight, and

•that these two phenomena. . . .are cases of one and the same law of causation. But the tendency of the moon to the earth, and the tendency of the earth and planets to the sun, were already known to be cases of the same law of causation; and thus the law of all these tendencies and the law of terrestrial gravity were recognised as identical, and were subsumed under one general law, that of gravitation.

In a similar manner the laws of magnetic phenomena have more recently been subsumed under known laws of electricity. That's how the most general laws of nature are usually arrived at—we climb up to them by successive steps. Here is why. To arrive by correct induction at laws holding under such an immense variety of circumstances, laws so general as to be independent of any changes of space or time that we can see, requires many sets of experiments or observations, conducted at different times by different people. One set of observations teaches us that the law holds good under conditions C_1, another that it holds good under conditions C_2, and by combining these we find that it holds good under much more general conditions or even holds universally. The general law is literally the *sum* of all the partial ones: it recognises the same sequence in different sets of instances, and can in fact be regarded as merely one step in the process of elimination. The tendency of bodies toward one another that we now call 'gravity' was at first observed only on the earth's surface, where it shows up only as a tendency of all bodies toward the earth; it might have been regarded as a special property of the earth itself, because one of the circumstances—namely **proximity to the earth**—hadn't been eliminated. To eliminate this required a fresh set of instances in other parts of the universe; we couldn't create these ourselves, and though nature had created them for us, we weren't well-placed to observe them. The making of these observations came within the province of a different set of

scientists from those who studied terrestrial phenomena; and it was a matter of great interest back at a time when the idea of explaining •celestial facts by •terrestrial laws was regarded as the confounding of an indefeasible distinction [= 'the crossing of an uncrossable line']. But when the celestial motions were accurately ascertained and the deductions performed, showing that their laws corresponded with the laws of terrestrial gravity, those celestial observations became a set of instances that precisely eliminated the circumstance of **proximity to the earth**. This proved that in the case of terrestrial objects the cause of the downward motion or pressure was not

> •the earth as such, but
> •the presence of some great body within certain limits of distance,

this being the circumstance common to the terrestrial and the celestial instances.

§6. There are, then, three ways of *explaining* laws of causation, i.e. *resolving them into other laws*. **(1)** The law of an effect of combined causes is resolved into the separate laws of the causes together with the fact of their combination. **(2)** The law that connects two links (not immediate neighbours) in a chain of causation is resolved into the laws that connect each with the intermediate links. **(3)** After a law has been shown to hold good in several classes of cases we decide that what is true in each of these is true under some more general supposition consisting of what all those classes of cases have in common. The first two involve resolving one law into two or more; the third resolves two or more into one. . . .

In all three processes, laws are resolved into laws more general than themselves—laws extending to all the cases that the former extended to, and others besides. In the first two

they are also resolved into laws that are more certain—i.e. more universally true—than themselves. They aren't proved to be •laws of nature, because that would require them to be universally true; what they are proved to be is •results of laws of nature. With that status, they're only conditionally true, usually true. Not so with the third process, because here the partial laws are in fact the very same law as the general one, so any exception to them would be an exception to it too. . . .

I'm using 'explanation' in its philosophical sense. Explaining one law of nature by another is merely substituting one mystery for another, and does nothing to make the general course of nature other than mysterious. We can no more assign a *why* for the more extensive laws than for the partial ones. In ordinary talk about these matters, an 'explanation' replaces a mystery that is still strange by one that has become familiar and come to seem not mysterious. But the process I have been discussing here often does the exact opposite: it resolves •a phenomenon that we are familiar with into •one of which we previously knew little or nothing; as when the common fact that heavy bodies fall was resolved into the tendency of all particles of matter toward one another. Don't forget this: in science those who speak

of 'explaining' a phenomenon mean (or should mean) to be pointing out not some more *familiar* phenomenon but merely

- •some more *general* phenomenon of which it is a partial exemplification, or
- •some laws of causation that produce it by their combined action, and from which its conditions can therefore be determined deductively.

Every such operation brings us a step nearer toward answering the question that I said on page 157 includes the whole problem of the investigation of nature, namely: What are the fewest and simplest assumptions, which being granted the whole existing order of nature would result? What are the fewest general propositions from which all the uniformities existing in nature could be deduced?

. . . .In minds that aren't used to accurate thinking there is often a confused notion that the general laws are the causes of the partial ones, e.g. that the law of general gravitation causes the phenomenon of the fall of bodies to the earth. But that's a misuse of the word 'cause'; terrestrial gravity isn't an •effect of general gravitation but a •case of it, i.e. one kind of the particular instances in which that general law obtains. . . .

Chapter 13. Examples of the explanation of laws of nature

§1. The most striking example of the kind of explanation I have been talking about—explaining causal laws and regularities among •special phenomena by resolving them into laws that are simpler and more •general—is the great Newtonian generalisation. So much has already been said about this ·that you don't need me to expound it to you·; it's enough to call attention to the great number and variety of the special observed uniformities that it accounts for as particular cases or consequences of one very simple law of universal nature. The simple fact that

> every particle of matter tends toward every other particle, with the tendency varying inversely as the square of the distance between them

explains •the fall of bodies to the earth, •the revolutions of the planets and satellites, •the motions (as far as we know them) of comets, and •all the regularities that have been observed in these special phenomena, such as the

> •elliptical orbits, and the variations from exact ellipses,
> •the relation of the planets' distances from the sun to the duration of their revolutions,
> •the precession of the equinoxes [see Glossary],
> •the tides,

and a vast number of minor astronomical truths.

[Mill reminds us of the explanation of magnetism in terms of electricity; and mentions the explanation—not yet complete, but already powerful—of the properties of the bodily organs in terms of the elementary properties of the tissues making them up.]

Another striking instance is Dalton's generalisation, commonly known as the 'atomic theory'. It had been known from the start of detailed chemical observation that any

two bodies combine chemically with one another in only a certain number of proportions; but those proportions were always expressed in percentages by weight. . . .; and those formulations didn't let the chemists see any relation between •the proportion in which a given element combines with one substance and •the proportion in which it combines with others. Dalton's great step consisted in perceiving that a unit of weight—known now as 'the atomic weight'—might be established for each substance, such that by supposing the substance to enter into all its combinations in the ratio of that unit (or of some low multiple of it) all the different proportions that had previously been expressed by percentages would result. Thus taking 1 to be the atomic weight of hydrogen and 8 to be the atomic weight of oxygen,

> •the combination of one unit of hydrogen with one unit of oxygen would produce the exact proportion (by weight) between the two substances that is known to exist in water;
> •the combination of one unit of hydrogen with two units of oxygen would produce the proportion that exists in the other compound of those two elements, hydrogen peroxide; and
> •and the combinations of hydrogen and of oxygen with all other substances would fit the supposition that •those two enter into combination by single units, or twos, or threes of their atomic weight = 1 and 8, and •the other substances enter the combinations by ones or twos or threes of *their* atomic weights.

The result is that a table of the atomic weights of all the elementary substances comprises in itself, and scientifically explains, all the proportions in which any substance,

elementary or compound, can enter into chemical combination with any other substance whatever.

§2. [Mill praises the work of Thomas Graham, who highlighted the difference between the *crystalloid* and *colloidal* states of matter, and discovered many of their properties. Crucially:] Whereas colloidal substances are easily penetrated by water and by the solutions of crystalloid substances, they are very little penetrable by one another. That enabled Graham to introduce a highly effective process (called 'dialysis') for separating the crystalloid substances contained in any liquid mixture, by passing them through a thin wall of colloidal matter that allows through little if any colloidal material. This enabled Graham to account for a number of special results of observation that hadn't previously been explained. [Mill sketches three of them and then this fourth:] Much light is thrown on the observed phenomena of *osmosis* (the passage of fluids outward and inward through animal membranes) by the fact that the membranes are colloidal. The result of that is that the water and saline solutions contained in the animal body pass easily and rapidly through •the membranes, whereas the substances directly applicable to nutrition, which are mostly colloidal, are detained by •them.

Salt's property of preserving animal substances from putrefaction is resolved by Liebig into two more general laws: •salt's strong attraction for water, and •the need for water if putrefaction is to occur. The intermediate item interpolated here between the remote cause and the effect isn't merely inferred but can be seen; for we've all seen that flesh on which salt has been thrown is soon swimming in brine.

The need of water for putrefaction itself provides an additional example of the explanation of laws. The law itself is proved by the Method of Difference: flesh completely dried

and kept in a dry atmosphere doesn't putrefy. . . . A deductive explanation of this same law results from Liebig's speculations. The putrefaction of animal and other nitrogen-containing bodies is a process in which they are gradually converted into (mainly) carbonic acid and ammonia. Now,

> •to convert the carbon of the animal substance into carbonic acid requires oxygen, and
> •to convert the nitrogen into ammonia requires hydrogen,

and these two are the elements of water. . . .

§3. Among the many important properties of the nervous system that were first discovered or strikingly illustrated by Brown-Séquard, I select *the reflex influence of the nervous system on nutrition and secretion.* By reflex nervous action is meant

> action that one part of the nervous system exerts over another part ·of the body· independently of the will and probably without passing through the brain and thus without consciousness.

Many experiments have shown that irritation of a nerve in one part of the body can in this way start up powerful action in another part; for example,

> •food injected into the stomach through a divided oesophagus ·and by-passing the tongue· nevertheless produces secretion of saliva;
> •warm water injected into the bowels, and various other irritations of the lower intestines, excite secretion of the gastric juice,

and so on. The reality of the power being thus proved, its agency explains a great variety of apparently anomalous phenomena, of which I select the following from Brown-Séquard's *Lectures on the Nervous System*:

•The production of tears by irritation of the eye, or of the mucous membrane of the nose;

•The secretions of the eye and nose increased by exposure of other parts of the body to cold;

•Inflammation of one eye, especially when cause by trauma, often excites a similar state in the other eye, which can be cured by cutting the intervening nerve;

•Loss of sight is sometimes produced by neuralgia, and has been known to be immediately cured by the extraction of a diseased tooth;

•A cataract has been produced in a healthy eye by a cataract in the other eye, or by neuralgia, or by a wound of the frontal nerve;

•The well-known phenomenon of a sudden stoppage of the heart's action, and consequent death, produced by irritation of some of the nerve-ends—e.g. by drinking very cold water, or by a blow on the abdomen or other sudden excitation of the abdominal sympathetic nerve, though this nerve can be irritated to any extent without stopping the heart's action if the nerves connecting them are cut;

•An extensive burn on the surface of the body can produce extraordinary effects on the internal organs—violent inflammation of the tissues of the abdomen, chest, or head; when death ensues from this kind of burn this internal disturbance is one of the most frequent causes of it;

•Paralysis and anaesthesia of one part of the body from neuralgia in another part; and muscular atrophy from neuralgia, even when there is no paralysis;

•Tetanus produced by cutting a nerve. Brown-Séquard thinks it highly probable that hydrophobia is a phenomenon of a similar nature;

•changes in the nutrition of the brain and spinal cord, manifesting themselves by epilepsy, chorea, hysteria, and other diseases, occasioned by lesion of some of the nerve-ends, e.g. worms, stones, tumours, diseased bones, and in some cases even by slight irritations of the skin.

§4. From these and similar instances we can see that when a previously unknown law of nature is brought to light or when new light has been thrown on a known law by experiment, it's important to examine all cases that include the conditions needed to bring that law into action. This process leads to demonstrations of •previously unsuspected special laws and of •explanations of laws that are already empirically known.

For example, Faraday discovered by experiment that if a conducting body is set in motion at right angles to the direction of a natural magnet, voltaic electricity is generated; and he found this to hold not only of small magnets but of that great magnet the earth. With that law established experimentally, we can now watch out for fresh instances in which a conductor moves or revolves at right angles to the direction of the earth's magnetic poles. In each of these we can expect electricity to be generated. In the northern regions where the polar direction is nearly perpendicular to the horizon, all horizontal motions of conductors will produce electricity: horizontal wheels made of metal, for example, and all running streams will generate an electric current that circulates round them; and the air thus charged with electricity may be one cause of the Aurora Borealis. In the equatorial regions, on the other hand, upright wheels placed parallel to the equator will create a voltaic circuit, and waterfalls will naturally become electric.

For a second example, it has been proved (mainly by Graham's researches) that gases have a strong tendency to permeate animal membranes, and diffuse themselves

through the spaces that such membranes enclose, even if there are already other gases in those spaces. [Mill uses this to 'demonstrate or explain' six 'more special laws'.]

§5. . . . .There are countless examples of new theories agreeing with and explaining old empiricisms [Mill's phrase]. All the sound things that experienced persons have said about human character and conduct are simply special laws that the general laws of the human mind explain and resolve. The empirical generalisations on which the operations of the arts [see Glossary] have usually been based are continually •justified and confirmed on the one hand, or •corrected and improved on the other, by the discovery of the simpler scientific laws that those operations depend on for their effectiveness. The effects of the rotation of crops, of the various manures, and other processes of improved agriculture, have been for the first time resolved in our own day into known laws of chemical and organic action, by Davy, Liebig, and others. The processes of the medical art are even now mostly empirical; the effectiveness of each is inferred from a special and most precarious experimental generalisation; but as science advances in discovering the *simple* laws of chemistry and physiology, progress is made in discovering •the intermediate links in the series of phenomena and •the more general laws they depend on; and thus, while the old processes are either exploded or. . . .explained, better processes, based on the knowledge of immediate causes, are continually being suggested and brought into use. [In a footnote Mill gives an example of a surgical improvement born of an *explanation* of the partial success of an 'old' procedure.] Many of the truths of geometry, even, were generalisations from experience before they were deduced from first principles. The quadrature of the cycloid [see Glossary] is said to have been first effected by weighing a cycloidal card and comparing its weight with that of a piece of similar card of known dimensions.

§6. To the foregoing examples from physical science I'll add another from mental science. Here is one of the simple laws of mind:

> Ideas of a pleasurable or painful sort form associations more easily and strongly than other ideas, i.e. they become associated ·with other ideas· after fewer repetitions, and the association is more durable.

This is an experimental law based on the Method of Difference. It is possible by deduction from this law to demonstrate and explain many of the more special laws that experience shows to exist among particular mental phenomena, for example:

> •how fast and easily thoughts connected with our passions or our more cherished interests are aroused, and how durably they stick in our memory;
> •the vivid recollection we retain of tiny circumstances that accompanied any object or event that deeply interested us, and of the times and places in which we have been very happy or very miserable;
> •the horror with which we view the accidental instrument of any occurrence that shocked us or the place where it happened, and the pleasure we get from any reminder of past enjoyment;

all these effects being proportional to the sensibility of the individual mind, and to the consequent intensity of the pain or pleasure from which the association originated. James Martineau has suggested that this same elementary law of our mental constitution, suitably followed out, would explain a variety of previously unexplained mental phenomena, and in particular some of the basic differences among human characters and mental abilities. Associations are of two

sorts—between synchronous impressions and between suc-cessive impressions—and the law that makes associations stronger in proportion to the pleasurable or painful char-acter of the impressions operates with special force in the synchronous class of associations. Martineau remarks that in minds with a strong organic sensibility, synchronous associations are likely to predominate, producing a tendency to conceive things in pictures and in concrete detail, richly clothed in attributes and circumstances, a mental habit that is commonly called 'imagination' and is one of the special qualities of the painter and the poet; while persons who are more moderately susceptible to pleasure and pain will tend to associate facts chiefly in the order of their succession; and such persons, if they have good intellects, will devote themselves to history or science rather than to creative art. I have tried (in my *Dissertations and Discussions*, vol. 1, fourth paper) to pursue this interesting speculation further, and to examine what help it can give towards explaining the poetical temperament. It at least serves as an example to show what scope there is for deductive investigation in the important and hitherto so imperfect science of mind.

§7. I have presented many examples of the discovery and explanation of special laws of phenomena by deduction from simpler and more general ones, because I wanted to characterise the Deductive Method clearly and give it the prominence its importance deserves. The Deductive Method is destined from now on to predominate in the course of scientific investigation. A revolution is peaceably and progressively going on in philosophy [here = 'the philosophy of science'], the reverse of the revolution to which Bacon attached his name. That great man changed the method of the sciences from •deductive to •experimental, and it is now rapidly reverting from •experimental to •deductive. But the

deductions that Bacon abolished were from premises hastily snatched up or arbitrarily assumed. The axioms weren't established by legitimate canons of experimental inquiry, nor were the results tested by that indispensable element of a rational Deductive Method, verification by specific experience. Between the primitive method of deduction ·that Bacon opposed· and the one I have tried to characterise there is all the difference that separates Aristotelian physics from the Newtonian theory of the heavens.

But don't think that all—or even most—of the great generalisations from which the subordinate truths of the more backward sciences will some day be deduced by rea-soning. . . .are truths that are *now* known and accepted. We can be sure that many of the most general laws of nature are as yet entirely unthought-of; and that many others that will eventually qualify as general laws of nature are now known only as laws or properties of some limited class of phenomena. (Just as electricity, now recognised as one of the most universal of natural agencies, was once known only as an odd property that certain substances acquired by friction, of first attracting and then repelling light bodies.) If the theories of heat, cohesion, crystallisation, and chemical action are destined—as surely they are—to become deductive, the truths that will then be regarded as the premises of those sciences would probably strike us *now* as being as novel as the law of gravitation appeared to Newton's contemporaries; perhaps even more novel than that. because Newton's law was an extension of the law of weight—i.e. of a generalisation familiar from of old and already covering a considerable body of natural phenomena. The general laws of a similarly commanding kind that we still look forward to the discovery of may not always find so much of their foundations already laid!

These general truths will doubtless make their first appearance as hypotheses; not proved or even provable at first but assumed as premises for the purpose of deducing from them the known laws of concrete phenomena. But this initial state can't be what they end up with. To entitle an hypothesis to be accepted as a truth of nature, and not as a mere technical help to the human faculties, it must be testable by the canons of legitimate induction, and must actually have been submitted to that test. When this is done *successfully*, premises will have been obtained from which all the other propositions of the science will from then on be presented as conclusions, and the science will by means of a new and unexpected induction be made deductive.

Chapter 14. The limits to the explanation of laws of nature. Hypotheses

§1. We have been led to recognise a distinction between two kinds of laws or observed uniformities in nature: •ultimate laws and what we could call •derivative laws. Derivative laws are those that can be deduced from other and more general ones, and can indeed be resolved into them. Ultimate laws are those that can't. We aren't sure that any of the uniformities we're now acquainted with are ultimate laws; but we know that there must be ultimate laws, and that every resolution of a derivative law into more general laws brings us nearer to them.

Since we are continually discovering that

   •uniformities we thought were ultimate are only derivative, and resolvable into more general laws,

or to put the same thing in different words, since we are continually discovering

   •the explanation of some sequence that we previously knew only as a ·brute unexplained· fact,

the question arises: Are there any necessary limits to this philosophical operation, or might it proceed until all the uniform sequences in nature are resolved into some one universal law? This does seem at first sight to be where the progress of induction by the Deductive Method....is heading. Projects of this kind were universal in the infancy of philosophy; any theoretical ideas that held out a less brilliant prospect were regarded in those early times as not worth pursuing. And the idea seems plausible in the light of the remarkable achievements of modern science, so that even now theorists frequently turn up either claiming to have solved the problem or suggesting ways in which it may one day be solved. Even when such large claims aren't being made, the nature of the solutions that are given or sought for particular classes of phenomena often involves conceptions of what explanation *is* that would make the notion of explaining *all* phenomena by means of *one* cause or law perfectly admissible.

§2. So it's useful to remark that the number of laws of nature can't possibly be smaller than the number of distinguishable sensations or other feelings of our nature—I mean distinguishable from one another in quality and not merely in quantity or degree. For example: there's a phenomenon *sui generis* [see Glossary] called **colour** that our consciousness tells us isn't a particular degree of some other phenomenon such as **heat** or **odour** or **motion**, but intrinsically unlike all

241

others; and it follows from this that there are ultimate laws of colour—that though it may be possible to explain the facts of colour, they can never can be explained from laws of heat or odour alone, or of motion alone, but that however far the explanation goes it will always contain a law of colour. I'm not denying *this*:

> It might be shown that some other phenomenon—e.g. some chemical or mechanical action—invariably precedes and causes each phenomenon of colour.

If this were proved, it would be an important extension of our knowledge of nature; but it wouldn't explain how or why a motion or chemical action produces a sensation of colour. However hard we studied the phenomena, and however many hidden links we detected in the chain of causation terminating in the colour, the last link would still be a law of *colour* and not a law of motion or of any other phenomenon. This applies not only to colour as compared with any other of the great classes of sensations, but also to each particular colour as compared with other colours. White colour can't possibly be explained exclusively by the laws of the production of red colour! In any attempt to explain it, we can't help including as one element of the explanation the proposition that some antecedent or other produces the sensation of white.

So the ideal limit of the explanation of natural phenomena (towards which we are constantly tending, while knowing that we can't ever completely attain it) would be to show that each distinguishable variety of our sensations or other states of consciousness has only one sort of cause; e.g. that there is some one condition or set of conditions that is always present whenever we perceive a white colour, and that always produces that sensation in us. As long as there are several known modes of production of a phenomenon (e.g. several substances that have the property of whiteness but no other

resemblance that we can find) it's always possible that one of these modes of production is resolved into another, or that all of them are resolved into some more general newly discovered mode of production. But when the modes of production are reduced to •one, we can't simplify things any further. This •one may not after all be the ultimate mode; there may be other links to be discovered between the supposed cause and the effect; but the only way we can we can resolve the known law is by introducing some other law that wasn't previously known, which won't reduce the number of ultimate laws.

In what cases has science been most successful in explaining phenomena by resolving their complex laws into laws of greater simplicity and generality? [Mill answers that the greatest success is with 'mechanical motion', and says that that's what might be expected: Motion occurs everywhere, it is produced in countless different ways, and the differences between different instances of motion don't bring in anything that looks like an uncrossable line, like that between colour and odour. He continues:] So there's no absurdity in supposing that all motion may be produced in one way, by the same kind of cause. And the greatest achievements in physical science have consisted in resolving one observed law of the production of motion into the laws of other known modes of production, or the laws of several such modes into one more general mode; as when

> •the fall of bodies to the earth and the motions of the planets were brought under the one law of the mutual attraction of all particles of matter;
> •when the motions said to be produced by magnetism were shown to be produced by electricity;
> •when the motions of fluids in a lateral direction, or even contrary to the direction of gravity, were shown to be produced by gravity;

and so on. There are many causes of motion that aren't

yet resolved into one another—gravitation, heat, electricity, chemical action, nervous action, and so on—but the attempt of the present generation of scientists to resolve all these different modes of production into one is perfectly legitimate, whether or not it ultimately succeeds. . . .

I needn't extend this illustration to other cases—the propagation through space of light, sound, heat, electricity, etc. or any of the other phenomena that have been explained by resolving their observed laws into more general laws. I have said enough to display the difference between •the kind of 'explanation' and 'resolution' of laws that is chimerical and •the kind that it's science's great aim to accomplish; and to show into what sort of elements the resolution must be effected, if at all.[1]

§3. From opposing the view that there is *only one* ultimate law of nature I now turn to the view that there are *enormously many* of them. (Almost every principle of a true method of doing science needs to be guarded against errors on both sides!) [Comte committed the latter error, Mill writes. His account of how is hard to follow, and his reply to it is omitted here.]

The really weak point in the attempts that have been made to account for colours by the vibrations of a fluid is not that the attempt itself is unscientific but that the existence of the fluid, and the thesis that it vibrates, are simply *assumed*, purely because they are supposed to help with the explanation of the phenomena. This leads to the important question of the proper use of scientific hypotheses. You don't need me to explain the connection between •this topic and •the topic of the explanation of natural phenomena and of the unavoidable limits to that explanation.

§4. An hypothesis is a supposition that we make (on admittedly insufficient evidence, or on none) in an attempt to deduce from it conclusions that conform to facts that we know to be real [= 'to factual propositions that we know to be true']. The idea is that if •the hypothesis leads to known truths then •it either must be—or at least is likely to be—true. If the hypothesis concerns the cause or mode of production of a phenomenon, it will serve (if accepted) to explain any facts that can be deduced from it; and *that* is the purpose behind many hypotheses, perhaps most of them. To 'explain' something—in the scientific sense of the word—is to resolve [see Glossary]

---

[1]   [In this note Mill approvingly quotes a long passage from Bain, •saying that similarities between phenomena offer hope of uniting their laws; •pointing out that gravitational attraction is strikingly similar to the cohesion or holding-together of bodies; and then •insisting that there is nevertheless no chance of theoretically uniting those two kinds of force. The quotation continues:] 'The two kinds of force agree in the one point, attraction, but they agree in no other; indeed in the manner of the attraction they differ widely. . . . Gravity is common to all matter, and equal in amount in equal masses of matter, whatever be the kind; it follows the law of the diffusion of space from a point (the inverse square of the distance); it extends to distances unlimited; it is indestructible and invariable. Cohesion is special for each separate substance; it decreases according to distance much more rapidly than the inverse square, vanishing entirely at very small distances. Two such forces aren't alike enough to be generalised into one force; the generalisation is only illusory; the statement of the difference would still make two forces, while the consideration of one wouldn't in any way simplify the phenomena of the other, as happened in the generalisation of gravity itself.' To the impassable limit of the explanation of laws of nature that I expounded in the text we must therefore add a further limitation. When the phenomena to be explained are not in their own nature generically distinct ·like colour and odour·, the attempt to refer them to the same cause is scientifically legitimate; but for the attempt to succeed, the cause *must* be shown to be capable of producing the phenomena according to the same law. Otherwise the unity of cause is a mere guess, and the generalisation only a nominal one which, even if accepted, wouldn't lower the number of ultimate laws of nature.

•a uniformity that isn't a law of causation into the laws of causation from which it follows, or

•a complex law of causation into simpler and more general ones from which it follows.

If we don't know any laws that fulfill this requirement, we can invent or imagine some that would fulfill it; and this is making an hypothesis.

Because an hypothesis is a mere supposition, the only limits to hypotheses are the limits of the human imagination; if we want to, we can offer to explain some effect by imagining some cause of an utterly unknown kind acting according to a perfectly fictitious law. But hypotheses of this sort •wouldn't have any of the plausibility of the ones that ally themselves by analogy with known laws of nature, and wouldn't meet the desire that made-up hypotheses are generally invented to satisfy, namely enabling the imagination to represent to itself an obscure phenomenon in a familiar light. So there has probably been no hypothesis in the history of science in which both the agent and the law of its operation were fictitious. ·In every actual hypothesis·, either **(i)** the supposed the cause is real but the law according to which it acts is merely supposed, or **(ii)** the cause is fictitious but the laws it is supposed to operate by are similar to the laws of some known class of phenomena. An instance of **(i)** is provided by the different suppositions made regarding the law of the planetary central force before the true law was discovered. That law, namely that the force varies as the inverse square of the distance, first suggested itself to Newton as an hypothesis, and was verified by proving that it led deductively to Kepler's laws. Hypotheses of kind **(ii)** include

•the vortices [see Glossary] of Descartes, which were fictitious but were supposed to obey the known laws of rotatory motion; and

•the two rival hypotheses regarding the nature of light,

one ascribing the phenomena to a fluid emitted from all luminous bodies, the other (now generally received) attributing them to vibratory motions among the particles of an ether—·a super-thin fluid·—pervading all of space.

Of the existence of either fluid there is no evidence except the explanation they offer for some of the phenomena; but they're supposed to produce their effects according to known laws—in one case •the ordinary laws of continued locomotion, and in the other •the laws of the propagation of waves among the particles of an elastic fluid.

According to what I have been saying, hypotheses are invented to enable the Deductive Method to be applied to phenomena *earlier*. But, as I said on page 225, there are three parts to the process of discovering the cause of a phenomenon by the Deductive Method:

(1) induction, to ascertain the laws of the causes;

(2) ratiocination, to compute from those laws how the causes will operate in the particular combination known to exist in the case in hand;

(3) verification, by comparing this calculated effect with the actual phenomenon.

None of these can be dispensed with (though the role of induction may be played by a previous deduction). All the three are found in the deduction proving that gravity is the central force of the solar system. **(1)** First, it is proved from the moon's motions that the earth attracts her with a force varying as the inverse square of the distance. This (though partly dependent on previous deductions) corresponds to the first step, the purely inductive one—the ascertainment of the law of the cause. **(2)** Secondly, from this law together with previously obtained knowledge of the moon's average distance from the earth and of the actual amount of her deflection from the tangent, it is ascertained how fast the

moon would be caused to fall if she were no further off, and no more acted upon by extraneous forces, than terrestrial bodies are; that is the second step, the ratiocination. **(3)** Finally, this calculated velocity is compared with the observed velocity with which all heavy bodies fall by mere gravity toward the surface of the earth (sixteen feet in the first second, forty-eight in the second, and so forth. . . .) and the two quantities are found to agree. The order in which I have presented the steps was not the order of their discovery, but it's their correct logical order. . . .

Now the Hypothetical Method suppresses step **(1)**, the induction to ascertain the law; and contents itself with **(2)** ratiocination and **(3)** verification; the law that is reasoned from being assumed rather than proved.   [This is the first occurrence of 'Hypothetical Method' in this work.]

If this process is to be legitimate, the nature of the case must be such that **(3)** the verification amounts to, and fulfills the conditions of, **(1)** a complete induction. We want to be assured that the law L that we have hypothetically assumed is a true one; and L's leading deductively to true results will give this assurance provided the case is such that a false law can't lead to a true result; provided that no law except L can lead deductively to the conclusions that L leads to. And that is often how things stand. For example, in the deduction that I have just cited the original major premise of the ratiocination, the law of gravitation, was ascertained in this way by this legitimate use of the Hypothetical Method. Newton began by assuming that

> the force that at each instant deflects a planet from
> its straight-line course and makes it curve around the
> sun is a force tending directly toward the sun.

He then proved that if this is right the planet will mark out (as we know by Kepler's first law that it does) equal areas in equal times; and lastly he proved that if the force acted in *any*

other direction the planet would not mark out equal areas in equal times. Because this shows that no other hypothesis would square with the facts, the assumption was proved; the hypothesis became an inductive truth. Not only did Newton use this hypothetical process to ascertain the direction of the deflecting force; he also used it to ascertain the law of variation of the strength of that force. He assumed that the force varied inversely as the square of the distance; showed that the remaining two of Kepler's laws could be deduced from this assumption; and finally showed that any other law of strength-variation would give results inconsistent with those laws, and therefore inconsistent with the real motions of the planets, which Kepler's laws were known express correctly.

I have said that in this case the verification fulfills the conditions of an induction; but an induction of what sort? On examination we find that it conforms to the canon of the Method of Difference. It presents the two instances

> A B C leading to *a b c*
> B C leading to *b c.*

A represents central force; A B C represents the planets plus a central force; B C represents the planets with no central force. The planets with a central force give *a*, areas proportional to the times; the planets without a central force give *b c* (a set of motions) without *a* or with something instead of *a.* This is the Method of Difference in all its strictness. It's true that the two instances required by the method are obtained not by experiment but by a previous deduction. But that doesn't matter. It doesn't matter what the nature is of the evidence from which we derive the assurance that A B C will produce *a b c*, and that B C will produce only *b c*; all that matters is that we *have* that assurance. In this case a process of reasoning provided Newton with the very instances that he would have sought by experiment if the

nature of the case had allowed experiments.

So it's perfectly possible—and in fact quite common—for something that was an hypothesis at the beginning of the inquiry to become a proved law of nature before its close. But for this to happen we must be able, by deduction or experiment, to obtain *both* the instances that the Method of Difference requires. We can deduce the known facts from the hypothesis, and that gives us the affirmative instance

A B C leading to *a b c*.

We also have to obtain the negative instance

B C leading to *b c*,

as Newton did by showing that no antecedent except the one assumed in the hypothesis would in conjunction with B C produce *a*.

It seems to me that this assurance can't be obtained if the hypothesis assumes an unknown cause that is imagined solely to account for *a*. When we are only trying

(i) to determine the precise law of a cause that we have already ascertained or

(ii) to pick out the actual cause from among several agents of the same kind, where we know that one or other of them is the cause

then we can get the negative instance ·that is needed for the Method of Difference·. An example of **(ii)** would be an inquiry into which of the bodies of the solar system causes by its attraction some particular irregularity in the orbit or periodic time of some satellite or comet. Newton's inquiry was an example of **(i)**. If it hadn't already been known that

(ia) the planets were prevented from moving in straight lines by some force tending toward the interior of their orbit, though the exact direction was doubtful;

or if it hadn't already been known that

(ib) the force increased in some proportion or other as the distance diminished, and diminished as it increased,

Newton's argument wouldn't have proved his conclusion. But these facts *were* already certain; so the range of admissible suppositions was limited to **(ia)** the various possible directions of a line and **(ib)** the various possible numerical relations between distance and attractive force. And it was easy to show that different suppositions drawn from *this* pool couldn't lead to identical consequences.

So Newton couldn't have performed his second great scientific operation—identifying terrestrial gravity with the central force of the solar system—by the same hypothetical method. When the law of the moon's attraction **had been proved from the data of the moon itself**, then on finding the same law to square with the phenomena of terrestrial gravity he was justified in adopting it as the law of those phenomena likewise; but it wouldn't have been permissible for him, without any data relating to the moon, to **assume** that the moon was attracted toward the earth with a force as the inverse square of the distance, merely because that ratio would enable him to account for terrestrial gravity. . . .

So it seems that a really genuinely scientific hypothesis mustn't be destined always to remain an hypothesis; it must be capable of being either proved or disproved by comparison with observed facts. This is the case •when the effect is already known to depend on the cause that is supposed, and the hypothesis concerns only the precise mode of dependence. . . .; and •when the hypothesis doesn't concern causation but only the law of correspondence between facts that accompany each other in their variations though there may be no cause-effect relation between them. Kepler's various false hypotheses about the law of the refraction of light were like that. It was known that the angle at which light came out of the transparent medium varied with every variation in the angle at which it went in, but it wasn't known *what* changes in the one corresponded to

the different changes of the other. In this case any law different from the true one had to lead to false results. And lastly I should add to these all the hypothetical modes of merely representing or describing phenomena—such as •the hypothesis of the ancient astronomers that the heavenly bodies moved in circles; •the various hypotheses postulating eccentrics, deferents, and epicycles, that were added to that original hypothesis; the nineteen false hypotheses that Kepler made and abandoned concerning the shape of the planetary orbits; and even the doctrine that he finally settled for, that those orbits are ellipses. This was also merely an hypothesis like the rest until it was verified by facts.

In all these cases verification is proof; if the hypothesis squares with the phenomena there's no need for any other evidence for it. But for that to be the case when the hypothesis relates to causation, I think the supposed cause has not only to be a real phenomenon, something actually existing in nature, but also to be already known to exercise, or at least to be capable of exercising, an influence of some sort over the effect. If that's not so, the mere fact that we can deduce the real phenomena from •the hypothesis is not sufficient evidence of •its truth.

What if an hypothesis merely assumes a cause, rather than ascribing an assumed law to a known cause? Am I saying that it isn't scientifically permissible? No. All I'm saying is that it shouldn't be accepted as true merely because it explains the phenomena. Without being accepted as true, it may usefully suggest a line of investigation that could lead to a real proof; though it can't even do that (as Comte rightly says) unless the cause it suggests is in its own nature susceptible of being proved by other evidence. This seems to be the philosophical import of Newton's maxim (so often cited with approval by later writers) that the cause assigned for any phenomenon must not only be one that would explain

the phenomenon (if we accepted it) but must also be a *vera causa* [see Glossary]. Newton didn't very explicitly define *vera causa*; and. . . .it's easy to show that his conception of it was neither precise nor consistent with itself—his optical theory was a striking instance of the violation of his own rule. The cause assigned in an hypothesis certainly doesn't have to be a cause already known; otherwise we would lose our best opportunities of becoming acquainted with new causes. But what is true in Newton's maxim is that the cause, though not known previously, should be capable of being known later on—that its existence should be capable of being detected, and its connection with the effect ascribed to it should be capable of being proved by independent evidence. By suggesting observations and experiments, the hypothesis puts us on the road to that independent evidence, if it really is attainable; and until it is actually attained, the hypothesis should be regarded merely as a more or less plausible conjecture.

§5. This function of hypotheses, however, is absolutely indispensable in science. When Newton said *Hypotheses non fingo* he didn't mean that he deprived himself of the aid to investigation provided by assuming at first what he hoped eventually to be able to prove. [That is Latin for 'I don't make (or invent, make up, contrive, fake) hypotheses'. *Fingo* is the Latin source for the English word 'fiction'.] Science could never have reached its present state without such assumptions. They are necessary steps in the progress to something more certain; and nearly everything that is now •theory was once •hypothesis. Even in purely experimental science, there must be some inducement to try one experiment rather than another. It is abstractly possible that all the experiments that have been tried were motivated by the mere desire to discover what would happen in certain circumstances, with no previous conjecture as to

the result; but •the experiments that have thrown most light on the general constitution of nature have been unobvious, delicate, and often cumbrous and tedious; •they wouldn't have had much chance of being undertaken unless there were people who thought that •they could decide whether some general doctrine or theory that had been suggested but not yet proved should be accepted as true. If this is true even of merely experimental inquiry, the conversion of experimental truths into deductive ones was even further from being feasible without large temporary assistance from hypotheses. The process of picking out a regularity in any complicated and seemingly confused set of appearances is bound to be tentative; we begin by making some supposition, even a false one, to see what consequences follow from it; and by seeing how these differ from the real phenomena we learn how to correct our assumption. The simplest supposition that accords with the more obvious facts is the best to begin with, because its consequences are the most easily traced. This rough hypothesis is then roughly corrected, and the operation repeated; and the comparison between •the consequences deducible from the corrected hypothesis and •the observed facts suggests still further corrections, until the deductive results are finally made to tally with the phenomena.... As Comte rightly said, neither induction nor deduction would enable us to understand even the simplest phenomena 'if we didn't often start by anticipating the results—by making a provisional supposition, at first essentially conjectural, involving some of the very notions that constitute the final object of the inquiry'. Watch how *you* unravel a complicated mass of evidence; consider, for instance, how you elicit the truth about some event from the involved statements of many witnesses. You'll find that you don't take all the items of evidence into your mind at once and try to weave them together; rather, you quickly

take a few of the particulars as your basis for a first rough theory about what happened, and then look at the other statements one by one, checking for whether they can be reconciled with your provisional theory, or what alterations or additions it requires to make it square with them. By this procedure, which has been rightly compared to the Methods of Approximation of mathematicians, we arrive through hypotheses at conclusions that aren't hypothetical.

·START OF A LONG FOOTNOTE·

....The now universally accepted doctrine that the earth is a natural magnet was originally an hypothesis of the celebrated Gilbert.

Another hypothesis (suggested by several recent writers) that isn't open to any objections and seems likely to light the path of scientific inquiry is that the brain is a kind of electric battery, and that each of its pulsations is a discharge of electricity through the system. It has been noted that the sensation felt by the hand from the pulsing of a brain is very like an electric shock. If this hypothesis is followed to its consequences, it might yield a plausible explanation of many physiological facts, and there's nothing to discourage the hope that some day we'll understand electricity well enough to make the truth of the hypothesis checkable against observation and experiment.

When Joseph Gall tried to localise in different regions of the brain the physical organs of our different mental faculties and propensities, this was a legitimate scientific hypothesis; so we ought not to blame him for the extremely slight grounds on which he often proceeded in a scientific project that could only be tentative. We may, however, regret that materials barely sufficient for a first rough hypothesis were hastily worked up into the vain semblance of a science [see 'Phrenology' in Wikipedia]. If there really is a connection

between the scale of mental endowments and the various degrees of complication in the cerebral system, by far the most likely way to discover that connection is to start with an hypothesis like Gall's. But because of the special nature of the phenomena, the verification of any such hypothesis faces difficulties that the phrenologists haven't shown themselves competent even to appreciate, much less to overcome.

Darwin's remarkable speculation on the Origin of Species is another unimpeachable example of a legitimate hypothesis. What he terms 'natural selection' is not only a *vera causa* but one that has been proved to be capable of producing effects like those that the hypothesis ascribes to it. . . . It is unreasonable to accuse Darwin (as some have) of violating the rules of induction. The rules of induction are concerned with the conditions of proof. Darwin has never claimed that his doctrine was proved. He was bound not by the rules of Induction but by the rules of Hypothesis. And the latter rules have seldom been more completely fulfilled. He has opened a path of inquiry full of promise, the results of which no-one can foresee. And isn't it a wonderful feat of scientific knowledge and ingenuity to have made such a bold suggestion admissible and discussible when everyone's first impulse had been to reject it at once, even as a conjecture?

·END OF FOOTNOTE·

§6. It is perfectly consistent with the spirit of the Hypothetical Method to assume in this provisional manner not only an hypothesis •concerning the law of something that we already know to be the cause but an hypothesis •about what the cause *is*. It is permissible, useful, and often even necessary to begin by asking ourselves what cause may have produced the effect, so that we can know which way to look for evidence to determine whether it actually did. The Descartes's vortices [see Glossary] would have been a perfectly

legitimate hypothesis if there had been the faintest chance that we could ever have a mode of exploration that would enable us to bring it conclusively to the test of observation. The defect of the hypothesis was that it couldn't lead to any course of investigation that might convert it from an hypothesis into a proved fact. It might be *dis*proved, either by some lack of correspondence with the phenomena it purported to explain or (as actually happened) by some extraneous fact. As Whewell wrote: 'The free passage of comets through the spaces that these vortices were supposed to inhabit convinced men that the vortices didn't exist.' But the hypothesis would have been false even if no such direct evidence of its falsity had been available. Direct evidence of its truth there could not be.

The prevailing hypothesis of a luminiferous ether [see Glossary] is not entirely cut off from the possibility of direct evidence in its favour (that being the main difference between it and Descartes's hypothesis about vortices). It's well known that the difference between •the calculated times of the periodical return of Encke's comet and •and the observed times has led to a conjecture that something that can resist motion is diffused through space. If this surmise were confirmed by the gradual accumulation, through the centuries, of a similar variance in the case of the other bodies in the solar system, the luminiferous ether would have gone a long way toward being a *vera causa*, because we'd have established that there is a great cosmical agent that has some of the attributes assumed by the hypothesis; though many ·of the •old· difficulties would remain, and I imagine that there would also be •new ones arising from the identification of the ·previously hypothesised· ether with the ·more recently discovered· resisting medium. At present, however, this hypothesis can't be regarded as more than a conjecture; the existence of the ether still rests on the possibility of deducing

from its assumed laws a considerable number of actual phenomena; and I can't regard this evidence as conclusive, because we can't be sure that if the hypothesis is false it must lead to results at variance with the true facts.

Accordingly, most sober thinkers accept that an hypothesis of this kind isn't to be accepted as probably true because it accounts for all the known phenomena. Sometimes two conflicting hypotheses *account for all the known phenomena*; and there are probably many others that are equally possible though they don't come into our minds because there's nothing analogous to them in our experience. But here's something that many people seem to think:

> An hypothesis of the kind we're considering is entitled to a more favourable reception if, besides •accounting for all the facts previously known, it •leads to the anticipation and prediction of others that experience later verifies—as the wave theory of light led to the prediction, subsequently confirmed by experiment, that two luminous rays might meet each other in such a way as to produce darkness.

You might expect that from a layman; but people with scientific attainments also—strangely!—lay stress on the fulfillment of this kind of prediction. If the laws of the propagation of light square with the laws of vibration in an elastic fluid in as many respects as is needed to make the hypothesis provide a correct expression of most of the phenomena known at the time, it's not surprising if they agree in one respect more. Even twenty such agreements wouldn't prove the reality of an ether in which waves occur; it wouldn't follow •that the phenomena of light were results of

the laws of elastic fluids, but at most •that they're governed by laws that overlap with these. . . . [1] Even in our imperfect acquaintance with nature we can cite cases where agencies that we have good reason to consider as radically distinct produce some or all of their effects according to laws that are identical. The law of **the inverse square of the distance** is the measure of the intensity not only of •gravitation but also (we think) of •illumination and of •heat diffused from a centre. Yet no-one thinks that because these three kinds of phenomena •obey the same law they are therefore •produced by the same mechanism.

[Mill quotes Whewell •disagreeing with the line Mill has been taking here and •illustrating his position with a peculiar example; Mill's response to this is also peculiar, and we can afford to by-pass this exchange. After it, Mill gets back on track:] The agreement of •the phenomena of light with •the theory of light-waves must arise from ·overlap, i.e. from· the actual identity of some of the laws of waves with some of those of light. . . . But from the fact that some of the laws ·of light· agree with the laws of waves it doesn't follow **that there are any actual light-waves**; any more than it followed from the fact that some (though not so many) of the laws of light agreed with the laws of the projection of particles that **there was actual emission of particles**. Even the light-waves hypothesis doesn't account for all the phenomena of light. •The natural colours of objects, •the compound nature of the solar ray, •the absorption of light, and •its roles in chemical and vital action—the hypothesis leaves these as mysterious as it found them. And some of these facts *seem* to fit better with particle theory than with the ·wave· theory of Young

---

[1]   What has contributed most to the acceptance of the hypothesis of a physical medium for the conveyance of light is a trio of facts: •that light travels (which can't be proved of gravitation), •that its communication is not instantaneous, but takes time; and •that it is intercepted by intervening objects (which gravitation is not). These are respects in which the phenomena of light *fit* those of the mechanical motion of a solid or fluid substance. But we aren't entitled to assume that mechanical motion is the only power in nature that can exhibit those attributes.

and Fresnel. For all we know, some third hypothesis will in time leave the wave theory as far behind as it has left the ·particle· theory of Newton and his successors.

I have said that 'Hypothesis H accounts for all the known phenomena' is often equally true of two conflicting hypotheses; and Whewell has remarked that he knows 'of no such case in the history of science, where the phenomena are at all numerous and complicated'. . . . But a few pages earlier he carefully *refuted* this by maintaining that all or most exploded scientific hypotheses could have been modified so as to make them correct representations of the phenomena. The hypothesis of vortices, he tells us, went through a series of modifications until its results coincided with •those of the Newtonian theory and with •the facts. Actually, the vortices didn't explain all the phenomena that the Newtonian theory was eventually found to account for—e.g. they didn't explain •the precession of the equinoxes—but •this phenomenon was not something that either side in the dispute had in mind as needing to be accounted for. We can believe on Whewell's authority that all the facts those people *did* have in mind accorded as accurately with the Cartesian hypothesis, in its finally improved state, as with Newton's.

But even if hypothesis H accounts for the facts and we can't imagine any other that does so, that isn't a valid reason for accepting H. There's no need to suppose that the true explanation must be one that we—with our limited experience so far—could imagine. Among the natural agents

we're acquainted with, the vibrations of an elastic fluid may be the only one whose laws are like the laws of light; but for all we know there may be an unknown cause that •is not an elastic ether diffused through space yet •produces effects identical in some respects with the effects that waves in such an ether would produce. To assume that no such cause can exist ·because we can't at present conceive it· appears to me an extreme case of assumption without evidence. And. . . . I can't help expressing astonishment that a philosopher of Whewell's abilities and attainments should have written an elaborate treatise on the philosophy of induction in which he recognises absolutely no mode of induction except that of

> trying hypothesis after hypothesis until one is found that fits the phenomena; which one, when found, is to be assumed as true, with only one reservation, namely if on re-examination it turns out to assume more than is needed for explaining the phenomena, the superfluous part of it should be cut off.

And this without *any* distinction between cases where •it can be known in advance that two hypotheses can't lead to the same result, and cases where •for all we know to the contrary there may be an infinity of hypotheses that are consistent with the phenomena.[1]

But I don't join Comte in condemning those who labour to work out in detail the application of these hypotheses to the explanation of ascertained facts, provided they bear in mind that the most they can prove is not that the hypothesis

---

[1]   Whewell has recently made a concession regarding the medium of the transmission of light that removes the difference between us, but I can't make sense of it in the context of the rest of his doctrine on this subject. Arguing that all matter has weight, he cites Hamilton's reference to the luminiferous ether and the calorific and electric fluids 'which we can neither •denude of their status as substance nor •clothe with the attribute of weight'. Whewell comments: 'My reply is that precisely because I can't clothe these agents with the attribute of weight I *do* denude them of the status of substance. They aren't substances, they are agencies. These weightless agents aren't properly called weightless fluids! I think I have proved this.' Nothing can be more philosophical. But if the luminiferous ether isn't matter—indeed if it isn't *fluid* matter—what is the meaning of the waves in it? Can an *agency* undulate? Can there be alternate motion forward and backward of the particles of an *agency*? And doesn't the whole mathematical theory of waves imply that they are material?. . . .

is true but that it *may be* true. The ether hypothesis has a very strong claim to be followed out in that way, a claim that was greatly strengthened when it was shown to provide a mechanism that would explain the mode of production of heat as well as of light. Indeed, the theory has a smaller element of hypothesis in its application to heat than in the case for which it was originally formed. We have proof by our senses that there is. . . .movement among the particles of all heated bodies, while we have nothing analogous to that in the case of light. Thus, when heat is communicated from the sun to the earth across apparently empty space, the chain of causation has. . . .motion at the beginning and at the end. The hypothesis only makes the motion continuous by extending it to the middle. Now, we know that motion in a body can be passed on to another body contiguous to it; and the intervention of a hypothetical elastic fluid occupying the space between the sun and the earth provides the contiguity that is the only thing missing—and can't be supplied *without* an intervening medium. Still, the supposition is at best a probable conjecture, not a proved truth. For there's no proof that contiguity is absolutely required for motion to be passed from one body to another. Contiguity doesn't always exist, to our senses at least, in cases where motion produces motion. The forces that go under the name of 'attraction', especially the greatest of all, gravitation, are examples of motion producing motion apparently without contiguity. When a planet moves, its distant satellites move with it. The sun carries the whole solar system with it in the progress it is making through space. Some theorists have come up with the geometrical reasonings (like the ones the Cartesians used to defend their vortices) by which they have tried to show that the motions of the ether *can* account for gravitation itself; but even if we accepted this as conclusive, it wouldn't follow that this *is* the mechanism of gravitation.

§7. Before leaving the topic of hypotheses, I should guard against the appearance of questioning the scientific value of several branches of physical inquiry which, though only in their infancy, I regard as strictly inductive. There's a great difference between **(i)** inventing agencies to account for classes of phenomena, and **(ii)** trying in conformity with known laws to conjecture what earlier collocations [see Glossary] of known agents may have given rise to individual facts that are still in existence. Of these, **(ii)** is the legitimate operation of inferring from an observed effect E the past existence of a cause similar to the cause that we know produced E in all the cases where we have actual experience of its origin. That's what goes on in the inquiries of geology; and they are no more illogical or fanciful than judicial inquiries that also aim at discovering a past event by inference from its present effects. Just as

> we can ascertain whether a man was murdered or died a natural death, from the state of the corpse, the presence or absence of signs of struggle, the marks of blood, the footprints of the supposed murderers and so on, relying all the way on uniformities ascertained by a perfect induction with no hypothesis stirred into the mix,

so also

> if we find on and beneath the surface of our planet masses exactly like deposits from water, or like results of the cooling of matter that has been melted by fire, we're entitled to conclude that *that* was their origin; and if the effects, though similar in kind, are on a far larger scale than any which are now produced, we may rationally—and without hypothesis—conclude that the causes existed formerly with greater intensity or operated during an enormous length of time.

Since the rise of the present enlightened school of geological

theorising, no geologist of authority has tried to go further than this.

In many geological inquiries it doubtless happens that though the laws to which the phenomena are ascribed are known laws, and the agents are known agents, those agents are not known to have been present in the particular case. In the theory that granite began as molten lava, there's no direct proof that this substance ever was actually subjected to intense heat. But the same thing could be said of all judicial inquiries that go by circumstantial evidence. We can conclude that a man was murdered, without its being proved by the testimony of eye-witnesses that someone who had intended to murder him was present on the spot. For most purposes it's enough if no other known cause could have generated the effects that have been found.

Laplace's famous theory about the origin of the earth and planets is essentially based on inductive procedures like those of modern geological theory. The theory is this:

> The sun's atmosphere originally extended to the present limits of the solar system; by cooling, it contracted to its present size; that shrinkage of the sun and its atmosphere made them spin ever faster (this is guaranteed by the general principles of mechanics); the increased centrifugal force generated by the faster rotation counteracted gravity and caused the sun to abandon successive rings of vapourous matter; these condensed by cooling and became the planets.

This theory doesn't hypothetically •introduce any unknown substance or •attribute any unknown property or law to a known substance. The known laws of matter authorise us to suppose that a body that constantly gives out as much heat as the sun does must grow steadily cooler, which must make it contract; so the present state of affairs requires us to suppose that the sun's atmosphere used to extend much further than it does now; and we're entitled to suppose that it extended as far out as we can find effects of the sort it might naturally leave behind it on shrinking; and that's what the planets are. [Mill continues to spell out all the steps needed by Laplace's theory, and concludes:] So Laplace's theory contains nothing that is strictly speaking *hypothetical*; it's an example of legitimate reasoning from a present effect to a possible past cause, according to the known laws of that cause. Although I have likened this to the theories of geologists, it is considerably less secure than them..... There is a much greater chance of error in assuming that the present laws of nature are the same ones that operated at the origin of the solar system than in merely presuming (with geologists) that those laws have lasted through a few revolutions and transformations of a single one among the bodies of which that system is composed.

Chapter 15. Progressive effects. The continued action of causes

§1. In chapters 11–14 I have traced the general outlines of the theory of the generation of derivative laws from ultimate ones. In this chapter I'll deal with one particular kind of derivation of laws from other laws—a kind that is so general and so important that it demands a separate examination.

The topic is: a complex phenomenon resulting from one simple law by the continual addition of an effect to itself.

Some phenomena, e.g. some bodily sensations, are essentially instantaneous; their existence can be prolonged only by prolonging the existence of the cause that produces

them. Most phenomena, however, are in their own nature permanent: having begun to exist, they would exist forever unless some cause intervened with a tendency to alter or destroy them. Such, for example, are all the facts of phenomena that we call 'bodies'. Once water has been produced, it won't of itself relapse into a state of hydrogen and oxygen; such a change requires some agent that can decompose the compound. Similarly with bodies' positions and movements in space, No object at rest starts moving without the intervention of something external to itself; and no object, once it is moving, returns to a state of rest or alters its direction or velocity unless some new external factor comes into play. So it perpetually happens that *a temporary cause gives rise to a permanent effect.* **(a)** A few hours of contact between iron and moist air produces a rust that may last for centuries; **(b)** a force that launches a cannon-ball into space produces a motion that would continue forever if no other force counteracted it.

Between those two examples there's a difference worth pointing out. In **(a)** (in which the effect is a substance and not a motion of a substance), since the rust remains unaltered unless some new cause intervenes, we can speak of the contact of moist air a century ago as the *immediate* cause of the rust that has existed from then until now. But when the effect is motion, which is itself a change, we must use a different language. The permanence of the effect is now only the permanence of a series of changes. The second inch or foot or mile of motion is not the mere prolonged duration of the first inch or foot or mile; it is *another* fact that follows the other and may in some respects be very unlike it because it carries the body through a different region of space. The original force that set the body moving is the remote cause of all its motion, however long that is continued; but it is the immediate cause only of the motion that occurred at

the first instant. The motion at any subsequent instant is immediately caused by the motion that occurred at the preceding instant, and not on the original moving cause.... This is recognised by mathematicians when they include the force generated by the motion of a body at t_1 among the causes of its motion at t_2. This would be absurd if it meant that this 'force' was an intermediate link between the cause and the effect. What it in fact refers to is only the previous motion itself, considered as a cause of further motion. So if we want to speak with perfect precision, we should consider each •link in the succession of motions as the effect of the •link preceding it. But if we find it convenient to speak of the whole series as one effect, it must be as an effect produced by the original push: a permanent effect produced by an instantaneous cause, and having the property of self-perpetuation.

Now consider the situation when the original agent or cause is itself permanent. Whatever effect has been produced up to a given time would (unless prevented by the intervention of some new cause) exist permanently, even if the cause were to perish. But the cause *doesn't* perish but continues to exist and to operate; so it must go on producing more and more of the effect; and instead of •one uniform effect we have •a growing series of effects arising from the accumulated influence of the permanent cause. Iron's contact with the atmosphere causes part of it to rust; and if the iron were then protected from the atmosphere that rust would be permanent but no more would be added. If the iron continues to be exposed to moist air, rusting continues until all the exposed iron is converted into a red powder.... Another example: the existence of the earth at a given instant causes an unsupported body to move towards it at the next instant; and if the earth were annihilated the effect already produced would continue—the object would

move in the same direction with its acquired velocity until intercepted by some body or deflected by some force. But the earth isn't annihilated, so it goes on producing in each 'next instant' an effect similar (in kind and quantity) to the effect in the preceding instant. The addition of these two effects to one another results in an accelerated velocity; and as this operation is repeated at each successive instant, the mere permanence of the cause—without any increase of it—gives rise to a continual increase of the effect, so long as all the conditions, negative and positive, of the production of that effect continue to exist.

Obviously this state of affairs is merely a case of the composition of causes. A *cause that continues in action* must on a strict analysis be considered as

> a number of exactly similar causes, successively introduced and jointly producing the sum of the effects that they would separately produce if they acted singly.

Strictly speaking, the progressive rusting of the iron is the sum of the effects of many particles of air acting in succession on corresponding particles of iron. The earth's continued action on a falling body is equivalent to a series of forces applied in successive instants, each tending to produce a certain constant quantity of motion; and the motion at t_2 is the sum of •the effects of the new force applied at t_1 and •the motion that had already been acquired before that. . . . The effect produced by the earth's influence at the most recent instant is added to the sum of the effects whose remote causes were the influences exerted by the earth at all the previous instants since the motion began. So this case comes under the principle of *a concurrence of causes producing an effect equal to the sum of their separate effects.* But because

> •the causes come into play successively, and
> •the effect at each instant is the sum of the effects of

only the causes that have come into action up to that instant,

the result takes the form of an ascending series—a series of sums, each greater than its immediate predecessor—and this gives us a progressive effect from the continued action of a cause.

The continuance of the cause influences the effect only by adding to its quantity, and the addition conforms to a fixed law (equal quantities in equal times); so the result can be calculated mathematically. In fact, this case of infinitesimal increments is precisely what the differential calculus was invented to meet. The questions

> •what effect will result from the continual addition of a given cause to itself?
> •what amount of the cause, being continually added to itself, will produce a given amount of the effect?

are obviously mathematical questions, and therefore to be treated deductively. We have seen that compositions of causes are seldom fit for anything but deductive investigation, and this is especially true in our present case of the continual composition of a cause with its own previous effects. Why? Well, this is especially amenable to the deductive method, and is bound to elude experimental treatment because of how *gradually* the effects are blended with one another and with the causes.

§2. I come now to a more intricate case of the composition of causes, namely the case where the cause doesn't merely •continue in action but •undergoes a continuous change in respects that are relevant to the effect. Here as before, the total effect goes on accumulating by the continual addition of fresh effects to those already produced, but now it's not by adding equal quantities in equal times; the •quantities added are unequal, and even the •quality may now be

different. If the change in the state of the permanent cause is progressive, the effect will go through a double series of changes—•partly from the accumulated action of the cause, and •from the changes in its own action. The effect is still a progressive effect, but this time produced not by the mere continuance of a cause but by its continuance and its progressiveness combined.

A familiar example: the increase of the temperature as summer advances, i.e. as the sun draws nearer to a vertical position and remains for more hours above the horizon.... When the sun has come near enough to the zenith, and remains above the horizon long enough, to give more warmth during one daily rotation than the earth's radiation can remove, the mere continuance of the cause would progressively increase the effect, even if the sun came no nearer and the days grew no longer; but in addition to this a change takes place because of the increase in the amount of heat the sun sends to us because of its changing position in the sky. When the summer solstice has passed, the progressive change in the cause begins to go in the opposite direction, but for a while the accumulating effect of the mere continuance of the cause exceeds the effect of the changes in it, and the temperature continues to rise.

A planet's motion is a progressive effect, produced by causes that are both permanent and progressive. The planet's orbit is determined....by two causes:

(i) the action of the sun, a permanent cause that •alternately increases or diminishes as the planet comes to be nearer to or further from the sun, and •acts in a different direction at every point; and

(ii) the planet's tendency to continue moving in the direction and with the velocity that it has already acquired. This force also grows greater as the planet draws nearer to the sun because it speeds up as it

does so and slows down as it recedes from the sun; and it also acts in a different direction at each point, because at every point the sun's action in deflecting the planet from its previous direction alters the line in which it tends to continue moving.

The planet's motion at t_2 is determined by •the amount and direction of its motion, and •the amount and direction of the sun's action on it, at t_1; and if we speak of the planet's entire journey around the sun as one phenomenon (which....we often find it convenient to do), that phenomenon is the progressive effect of two permanent and progressive causes, the sun's force and the acquired motion. Those causes happen to be progressive in the special way that we call 'periodical', so the effect has to be periodical too....

Another feature of this example is worth thinking about. Though the causes themselves are permanent and independent of all conditions known to us, the *changes* in the quantities and relations of the causes are actually caused by changes in the effects. [Mill explains this in more detail than we need. The point is just that the difference between

•the strength and direction of the sun's pull on the planet at time t_1 and •the strength and direction of its pull at t_2

and also the difference between

•the strength and direction of the planet's tendency to move in a straight line at t_1 and •the strength and direction of its tendency to move in a straight line at t_2

are both caused by facts about how the planet moves between those two times.]

§3. In all cases of progressive effects, whether arising from the accumulation of unchanging or of changing elements, there is a uniformity of succession not merely between the cause and the effect, but between the first stages of the effect

and its subsequent stages. . . . The sequence of spring and summer is regular and invariable. . . ., but we don't consider spring to be the cause of summer; it's evident that both are successive effects of the heat received from the sun, and that spring considered merely in itself could continue for ever without having the slightest tendency to produce summer. As I have so often remarked, the cause is the *unconditional* invariable antecedent. . . .

This is how most uniformities of succession are generated—I mean ones that aren't cases of causation. When a phenomenon goes on increasing, or periodically increases and diminishes, or goes through any continued and unceasing process of variation reducible to a uniform rule or law of succession, we don't infer from this that any two neighbours in the series are cause and effect. We presume the contrary; we expect to find that the whole series originates either from •the continued action of fixed causes or from •causes that are themselves continuously changing. A tree grows from half an inch high to a hundred feet; and trees of some species will generally grow to that height unless prevented by some counteracting cause. But we don't call the seedling the cause of the full-grown tree; it certainly is the invariable antecedent, and we don't know much about what other antecedents the sequence depends on, but we're convinced that it depends on something. Why? Because. . . .the close resemblance of the seedling to the tree in all respects but

size, and the gradualness of the growth, so exactly resemble the progressively accumulating effect produced by **the long action of some one cause** that we can't possibly doubt that the seedling and the tree are two terms in a series of that sort, the first term of which we haven't yet found. The conclusion is further confirmed by the fact that we can prove by strict induction that the tree's growth, and even its continued existence, depend on the continued repetition of certain processes of nutrition—the rise of the sap, the absorptions and exhalations by the leaves, etc.—and the same experiments would probably prove to us that the growth of the tree is the accumulated sum of the effects of these continued processes if it weren't for the fact that our eyesight isn't microscopic enough for us to observe correctly and in detail what those effects are.

In such a case the effect may during its progress undergo many modifications besides those of quantity, and may sometimes appear to undergo a very marked change of character. This could be because •the unknown cause consists of several component elements whose effects, accumulating according to different laws, are compounded in different proportions at different times; or because •at certain points in the effect's progress fresh causes or agencies come in, or are evolved, which intermix their laws with those of the primary agent.

Chapter 16. Empirical laws

§1. When observation or experiment has shown that a uniformity U exists, but scientists can't see any reason why U exists and therefore hesitate to rely on it in cases varying much from those that have been actually observed, they call U an *Empirical Law.* In calling something an empirical law we imply that it's not an ultimate law—that if it is true, its truth can be and should be accounted for. It is a derivative law, the derivation of which is not yet known. To state the explanation, the *why*, of the empirical law would be to state the laws from which it is derived—the ultimate causes on which it depends; and if we knew these we would also know what its limits are, i.e. under what conditions it would cease to be fulfilled.

The periodic return of eclipses, as originally ascertained by the early Eastern astronomers' many observations, was an empirical law until the general laws of the celestial motions had accounted for it. The following are empirical laws still waiting to be resolved into the simpler laws from which they are derived:
- the local laws of the rise and fall of the tides in different places;
- the relation between certain kinds of weather and certain appearances of sky;
- the apparent exceptions to the almost universal truth that bodies expand by increase of temperature;
- the law that animal and vegetable species are improved by cross-breeding:

and also the fact that

- gases have a strong tendency to permeate animal membranes;
- substances with a very high proportion of nitrogen (such as hydrocyanic acid and morphia) are powerful poisons;
- when different metals are fused together the alloy is harder than the various elements;
- the number of atoms of acid required to neutralise one atom of any base is equal to the number of atoms of oxygen in the base;
- the solubility of substances in one another depends, at least in some degree, on the similarity of their elements.[1]

An empirical law, then, is *an observed uniformity, presumed to be resolvable into simpler laws but not yet resolved into them.* Empirical laws are often discovered *long* before they are explained by the Deductive Method; and the verification of a deduction usually consists in comparing its results with empirical laws previously ascertained.

§2. A small number of ultimate laws of causation generates a vast number of derivative uniformities, both of succession and of coexistence. **(a)** Some are laws of succession or coexistence between different effects of the same cause; I gave examples of these it chapter 15. **(b)** Some are laws of succession between effects and their remote causes, resolvable into the laws connecting each with the intermediate link. **(c)** When causes act together and compound their effects,

[1]   Water, of which eight-ninths in weight is oxygen, dissolves most bodies that contain a high proportion of oxygen. . . .; bodies largely composed of combustible elements. . . .are soluble in bodies of similar composition. . . . This empirical generalisation is far from being universally true; no doubt because it is a remote and therefore easily defeated result of general laws that are too deep for us at present to penetrate; but it will probably in time suggest lines of inquiry that will lead to the discovery of those laws.

the laws of those causes generate the fundamental law of the effect, namely that it depends on the coexistence of those causes. **(d)** Finally, the order of succession or of coexistence that holds among effects necessarily depends on their causes. If they are effects of a single cause, it depends on the laws of that cause; if they're effects of different causes, it depends on the laws of those causes separately and on the circumstances that determine their coexistence. If we investigate when and how the causes will coexist, that depends on *their* causes; and we may thus trace back the phenomena higher and higher until

> •the different series of effects meet in a point, and the whole thing is shown to have depended ultimately on some common cause;

or until

> •instead of converging to one point they terminate in different points, and the order of the effects is proved to have arisen from the collocation of some of the ultimate causes.

For example, the order of succession and of coexistence among the heavenly motions that Kepler's laws express is derived from the coexistence of two primeval causes, •the sun and •each planet's original impulse or projectile force. Kepler's laws are resolved into the laws of these causes and the fact of their coexistence.

So derivative laws don't depend solely on the ultimate laws into which they are resolvable; they mostly depend on •those ultimate laws and •an ultimate *fact*, namely the mode of coexistence of some of the elements of the universe. The ultimate laws of causation could be just what they actually are and yet the derivative laws completely different, if the causes coexisted in different proportions or with any difference in such of their relations as influence the effects. If, for example, •the sun's attraction and •the

original projectile force had existed in some other **ratio** to one another than they did (and we know of no reason why this couldn't have been the case), the derivative laws of planetary motions could have been quite different from what they are. The **ratio** that does exist happens to be such as to produce regular elliptical motions; any other ratio ·of sun's attraction to original projectile force· would have produced different ellipses, or circles, parabolas, or hyperbolas, but still regular trajectories because the effects of each of the agents accumulate according to a uniform law; and two regular series of quantities, when their corresponding terms are added, must produce a regular series of some sort. . . .

§3. Now this last-mentioned element in the resolution of a derivative law—the element that is not a law of causation but a collocation of causes—can't itself be reduced to any law. As I remarked on page 170, no uniformity or norm or principle or rule is perceivable in the distribution of the primeval natural agents through the universe. The different substances composing the earth stand in no constant relation to the powers that pervade the universe. One substance is more abundant than others, one power acts through a larger extent of space than others, without any pervading analogy that we can discover. We don't know why •the sun's attraction and •the force in the direction of the tangent coexist in the exact proportion they do, and we can't trace any match between the sun's attraction and the proportions in which any other elementary powers in the universe are intermingled. The utmost disorder in the combination of the causes is consistent with the most regular order in their effects; because when each agent acts according to a uniform law even the most capricious combination of agencies will generate a regularity of some sort; as we see in the kaleidoscope, where any casual arrangement of coloured bits of

glass produces through the laws of reflection a beautiful regularity in the effect.

§4. This justifies the attitude of scientists in not relying much on empirical laws.

A derivative law that results wholly from the operation of some one cause will be as universally true as the laws of the cause are—i.e. it will always be true except where some one of its effects is defeated by a counteracting cause. But when the derivative law results not from •different effects of one cause but from •effects of several causes, we can't be certain that it will remain true if there's some re-arrangement of those causes or of the primitive natural agents on which the causes ultimately depend. The proposition that *coal-beds always rest on strata of kind K*, though true on the earth as far as we know, can't be extended to the moon or the other planets, supposing that they have coal, because we can't be sure that the initial constitution of any other planet was such as to lay down geological deposits in the same order as on our globe. The derivative law in this case depends not only on laws but also on a collocation [see Glossary]; and collocations can't be reduced to any law.

If EL is an empirical law—i.e. a derivative law that hasn't *yet* been resolved into its elements—then of course we don't know whether it results from •the different effects of one cause, or from •effects of different causes. We can't tell whether it depends wholly on laws, or partly on laws and partly on a collocation. If EL depends on a collocation, it will be true in every case where that particular collocation exists. But we don't know, supposing it *does* depend on a collocation, what the collocation is; so we aren't safe in extending EL beyond the limits of time and place where we have actual experience of its truth. Since it has always been found true within those limits, we have evidence that the

relevant collocations, whatever they are, really do exist within those limits. But we have no basis for inferring that because a collocation is proved to exist within certain limits of place or time it will exist beyond those limits. So empirical laws can be accepted as true only within the limits of time and place in which they have been found true by observation; indeed, only within the limits of time and place *and circumstance*; for we don't know the ultimate laws of causation on which EL depends, so we can't foresee, without actual trial, how the introduction of any new circumstance may affect it.

§5. How are we to know that a uniformity ascertained by experience is only an empirical law? We haven't been able to resolve it into any other laws, so how do we know that it isn't itself an ultimate law of causation?

I answer that no generalisation amounts to more than an empirical law if the only support for it comes from the Method of Agreement. We have seen that we can never arrive at causes by that method alone. The utmost that the Method of Agreement can do is to ascertain all the circumstances common to all cases in which a phenomenon P is produced; and this aggregate includes not only •the cause of P but all the phenomena P is connected with by any derivative uniformity, whether as collateral effects of a single cause or as effects of some other cause that has coexisted with it in all the instances we have observed. The Method of Agreement doesn't offer any way of determining which of these uniformities are laws of causation and which are merely derivative laws resulting from the laws of causation and the collocation of the causes. So none of them can be accepted as anything but derivative laws whose derivation hasn't been traced—i.e. empirical laws. And that's the status we must assign to all results obtained by the Method of Agreement (and therefore almost all truths obtained by simple observation without

experiment), until they are confirmed by the Method of Difference or explained deductively.

These empirical laws may have **(i)** more or **(ii)** less authority, depending on whether there's reason to think that they are resolvable into **(i)** laws only or into **(ii)** laws and collocations together. **(i)** The sequences that we observe in the production and subsequent life of an animal or a vegetable, resting purely on the Method of Agreement, are mere empirical laws; but though the antecedents in those sequences may not be the causes of the consequents, all the stages in the sequences are doubtless *successive stages of a progressive effect of a common cause*, and are therefore independent of collocations. **(ii)** On the other hand, the uniformities in the top-to-bottom order of strata on our planet are empirical laws of a much weaker kind, because as well as not being laws of causation there's no reason to believe that they depend on any common cause; all appearances are in favour of their depending on a particular collocation of natural agents that existed on our globe at some time or other—a collocation that there's no reason to think supports any inference about what collocation does or did exist in any other part of the universe.

§6. My definition of 'empirical law' made that phrase applicable not only to uniformities that *aren't* known to be laws of causation, but also to ones that *are*, provided there's reason to presume that they aren't ultimate laws. Now is the time to ask: By what signs can we judge, of an observed uniformity that ·we are satisfied· is a law of causation, that it is a derivative and not an ultimate law? ·I shall present two such signs·.

(1) The first sign is one we get if there's evidence that between the antecedent *a* and the consequent *b* there's some intermediate link, some phenomenon that we can guess exists there, though our senses or our instruments aren't sharp enough for us to ascertain its precise nature and laws. If there is such an intermediate phenomenon IP, it follows that even if *a* is the cause of *b*, it is only the remote cause, and that the law *a causes b* is resolvable into at least two laws, *a causes IP* and *IP causes b*. This is a very common case, because the operations of nature are mostly on such a minute scale that many of the successive steps can't be clearly perceived if indeed they are perceived at all.

Consider the laws of the chemical composition of substances—e.g. that when hydrogen and oxygen are combined water is produced. All we see of the process is

- the two gases are mixed in certain proportions,
- heat or electricity is applied,
- an explosion takes place,
- the gases disappear, and
- water remains.

There's no doubt about the law, or about its being a law of causation. But between the antecedent (the gases in a state of mechanical mixture, heated or electrified) and the consequent (the production of water) there must be an intermediate process that we don't see. For if we analyse *any* portion of the water, we find that it always contains hydrogen and oxygen—indeed, in the very same proportions. This is true of a single drop; it's true of the smallest portion our instruments can evaluate. And since the smallest perceptible portion of the water contains both those substances, portions of hydrogen and oxygen smaller than the smallest perceptible must have come together in every minute portion of space; must have come closer together than when the gases were merely mechanically mixed since (to mention just one reason) the water occupies far less space than the gases. Now, we can't *see* this contact or close approach of the tiny particles, so we can't observe what circumstances accompany it or

what laws are at work when it produces its effects. The production of water. . . .may be a very remote effect of those laws. There may be countless intervening links, and we are sure that there must be *some*. We have full proof that each of the great transformations in the sensible properties of substances is preceded by some kind of corpuscular [see Glossary] action; so we can't doubt that the laws of chemical action, as at present known, are not ultimate laws but derivative ones—even if we will never know the nature of the laws of corpuscular action they are derived from.

Similarly, all the processes of vegetative life, whether in plants or in animals, are corpuscular processes. Nutrition is the addition of particles to one another, sometimes merely replacing other particles that have been separated and excreted, sometimes adding to the organism's size or weight, but doing this so gradually that it isn't perceptible until it has gone on for a long time. Various organs have their own special vessels in which they store fluids whose component particles must have been in the blood, though they are utterly unlike blood in their mechanical properties and in their chemical composition. Here's an abundance of unknown links to be filled in; and there can't be any doubt that the laws of the phenomena of living organisms are derivative laws, dependent on properties of •corpuscles and of •elementary tissues that are comparatively simple combinations of corpuscles.

(2) We encounter the second sign that a law isn't ultimate when its antecedent A is an extremely complex phenomenon, which makes it likely that A's effects are at least partly compounded out of the effects of A's different elements. Cases where the effect of the whole is not made up of the effects of its parts are rare ·and therefore unlikely·, so the Composition of Causes is by far the more ordinary ·and thus more probable· case.

I'll illustrate this by two examples. In the first, the antecedent is the sum of many homogeneous parts. The weight of a body is made up of the weights of its tiny particles; and astronomers avail themselves of this when they say that bodies at equal distances gravitate towards one another in proportion to their quantity of matter—·implying that what holds for the big things also holds for the little ones·. So all true propositions concerning gravity are derivative laws; the ultimate law into which they are all resolvable is that every particle of matter attracts every other. In my second example, the antecedent is the sum of many heterogeneous parts. Let it be any one of the sequences observed in meteorology—e.g. the fact that a lessening of atmospheric pressure (indicated by a fall of the barometer) is followed by rain. The antecedent is here a complex phenomenon, made up of heterogeneous elements: the column of the atmosphere over any particular place consists of two parts—a column of air and a column of water-vapour mixed with it—and the change in these two together that is •shown by a fall of the barometer and •followed by rain must be a change in the air or a change in the water-vapour or a change in both. So even if that's all we have to go on, we can reasonably suppose—given the invariable presence of both these elements in the antecedent—that the sequence is probably not an ultimate law, but a result of the laws of air and the laws of water-vapour. If we come to know those laws so well that we're in a position to say that they couldn't, unaided, produce the observed results in the barometer and the weather, then of course we must give up this supposition. But not until then.

In almost all known cases in which a very complex antecedent A is regularly followed by a state of affairs S, we can either

> •actually account for the sequence A → S in terms of simpler laws, or

•infer with great probability (from our knowledge that there are intermediate causal links though we don't know what they are) that A → S could be accounted for in that way.

So it's highly probable that all sequences from complex antecedents are resolvable like that, and that ultimate laws are all comparatively simple. If we didn't have the reasons that I gave on page 183 for believing that the laws of organised nature are resolvable into simpler laws, it would be almost a sufficient reason that the antecedents in most of the sequences are so very complex.

§7. I have recognised two kinds of empirical laws—•those known to be laws of causation, but presumed to be resolvable into simpler laws, and •those not known to be laws of causation at all. These two have several things in common:

•They both agree in the demand they make for being explained by deduction.

•They are both appropriate means of verifying such a deduction, because they represent the experience that the result of the deduction must be compared with.

•Until they are explained and connected with the ultimate laws from which they result, they both fall short

of the highest certainty that laws are capable of.

I showed on page 232 that laws of causation that are derivative, and compounded of simpler laws, are not only •less general than the simpler laws from which they result, but also •less certain, less entitled to be relied on as universally true. But the certainty-gap between •simpler laws and •the less general laws derived from them, is trifling compared with the certainty-gap between •simpler laws and •uniformities that aren't known to be laws of causation at all. Until these are resolved, we can't tell how many collocations as well as laws their truth may depend on; so we can never confidently extend them to cases where we haven't assured ourselves (by trial) that the required collocations of causes actually exist. The property that philosophers usually regard as characteristic of empirical laws—namely, being unfit to be relied on beyond the limits of time, place, and circumstance in which the observations have been made—really and strictly belongs only to laws in this class. They are 'empirical laws' in a stronger and more direct sense; and except where the context plainly indicates otherwise I shall use the phrase 'empirical laws' only to refer to uniformities—whether of succession or of coexistence—that aren't known to be laws of causation.

Chapter 17. Chance and its elimination

§1. Empirical laws, then, are observed uniformities concerning which the question 'Are they laws of causation?' must remain undecided until •they are explained deductively or •some means are found of applying the Method of Difference to the case; and I showed in chapter 16 that until a uniformity can in one of these ways be removed from the class of

empirical laws and classified either as a law of causation or a demonstrated results of laws of causation, we can't be sure that it is true beyond the spatial and other limits within which it has been found true by actual observation. There remains the question: How are we to sure that it is true even *within* those limits? How much experience is

needed for a generalisation that rests solely on the Method of Agreement to be considered sufficiently established, even as an empirical law? On page 216 I explicitly set this question aside, and now it's time to try to solve it.

We found that the Method of Agreement doesn't prove •causation, and can therefore only be used for ascertaining •empirical laws. But we also found that it has a second characteristic imperfection, namely tending to make uncertain even conclusions of the sort that it is in itself adapted to prove. That's because of plurality of causes. Although two or more cases where the phenomenon *a* has been met with have no common antecedent except A, this doesn't prove that there is any connection between *a* and A, because *a* may have many causes, and may have been produced in these different instances not •by anything that the instances had in common but •by a variety of different elements. But I remarked that as the number of instances pointing to A as the antecedent grows, the uncertainty of the method lessens and the existence of a law connecting A with *a* comes closer to certainty. Now we have to determine *how much* experience is needed for this certainty to be regarded as practically attained, and the connection between A and *a* to be accepted as an empirical law.

In more familiar terms: After how many and what sort of instances are we entitled to conclude that an observed coincidence between two phenomena is not the effect of chance?

For understanding the logic of induction, it is vitally important to have a distinct conception of •what is meant by *chance*, and of •how the phenomena that common language ascribes to that abstraction—·chance·—are really produced.

§2. Chance is usually spoken of in direct antithesis to law. The thought is that whatever can't be ascribed to any law is due to chance. But it's certain that *everything* that happens is the result of some law; is an effect of causes, and could have been predicted from a knowledge of the existence of those causes and of their laws. When I turn up the Queen of diamonds, that's a consequence of its place in the pack. Its place in the pack was a consequence of •how the cards were shuffled, or of •the order in which they were played in the last game; and those again were effects of prior causes. At every stage, if we knew enough about the causes in existence, it would have been theoretically possible—·not necessarily possible in practice·—to foretell the effect.

An event occurring by chance may be better described as a coincidence from which we have no basis for inferring a uniformity—the occurrence of a phenomenon in certain circumstances without this giving us reason to think that it will happen again in those circumstances. But this implies that not all the circumstances have been taken into account. Whatever the phenomenon is, since it has occurred once we can be sure that if *all* the same circumstances were repeated it would occur again; and not only if *all*—there's some particular subset of those circumstances on which the phenomenon is invariably consequent. It isn't connected in any permanent manner with *most* of the circumstances; its conjunction with those is said to be the effect of chance, to be merely casual. Facts casually conjoined are separately the effects of causes and therefore of laws, but of different causes, and causes not connected by any law.

So it's wrong to say that any phenomenon is produced by chance; but it is all right to say that two or more phenomena are conjoined by chance, i.e. that they coexist or succeed one another only by chance. That means that there's no causal relation *between* them, i.e. it is not the case that they are related

•as cause and effect, or as
•effects of a single cause, or as
•effects of causes that are related by a law of coexistence, or
•effects of a single collocation of primeval causes.

. . . .There is no simple test for this. A coincidence can occur again and again, and yet be only casual. . . . The recurrence of the same coincidence more than once, or even its frequent recurrence, doesn't prove that it is an instance of any law—doesn't prove that it is not casual, or (in common language) 'the effect of chance'.

But when a coincidence can't be deduced from known laws or proved by experiment to be itself a case of causation, the frequency of its occurrence is the only basis we have for inferring that it is ·not casual but· the result of a law. I'm not talking about its absolute frequency, ·i.e. the answer to the question 'How often has it occurred?'·. The question is not whether the coincidence occurs often or seldom. . . ., but whether it occurs more often than chance will account for—more often than it would be reasonable to expect if the coincidence were casual. So we have to decide what degree of frequency in a coincidence can be accounted for by chance, and there can be no general answer to this. All I can do is to state the principle by which the answer must be determined; the answer itself will be different in every different case.

Suppose that one of the phenomena, A, exists always, and the other phenomenon, B, exists only occasionally. It follows that every instance of B will be an instance of B's coincidence with A, and yet the coincidence will be merely casual, not the result of any connection between them. The fixed stars have been in existence ever since the beginning of human experience, and all phenomena that have come under human observation have. . . .coexisted with them; yet this coincidence, though just as invariable as what exists between any one (x) of those phenomena and x's own cause, doesn't prove that the stars are in any way causally connected with x. This is as strong a case of coincidence as can possibly exist—much stronger in mere frequency than most of the ones that *do* prove laws—but it doesn't here prove a law. Why not? Because the stars exist always and therefore coexist with every other phenomenon, whether connected with it by causation or not. The uniformity is no greater than would occur if there were no such connection.

On the other hand, suppose we're inquiring whether there's any connection between rain and some particular ·kind K of· wind. We know that rain occasionally occurs with every wind; so the connection ·between rain and K wind·, if it exists, can't be an actual law; but still rain may be connected with K wind through causation. They can't always be effects of a single cause (for if they were they would regularly coexist), but there may be some causes common to them both, so that to the extent that either of them is produced by those common causes they will. . . .be found to coexist. How are we to ascertain this? The obvious answer is: by observing whether rain occurs with K wind more often than with any other. But that's not enough, for it might be because K wind blows more often than any other; so that its blowing more often in rainy weather is merely what you'd expect if K wind had no connection with the causes of rain. . . . In England, westerly winds blow for about twice as much of the time as do easterly winds. So if it rains only twice as often with a westerly wind as with an easterly one, that's no reason to infer that any law of nature is at work in the coincidence. If it rains *more than* twice as often, we can be sure that some law is concerned. Either

•there's some cause in nature which in this climate tends to produce both rain and a westerly wind, or

•a westerly wind itself has some tendency to produce rain.

But if it rains less than twice as often, we can draw a directly opposite conclusion, inferring that the occurrence of rain is connected •with causes adverse to westerly winds or with •the absence of some cause that produces such winds; and though it may still rain much oftener with a westerly wind than with an easterly, that wouldn't proving any connection between rain and westerly wind; quite the contrary, it would prove a connection between rain and easterly wind. . . .

So here are two examples:

•one where the greatest possible frequency of coincidence, with no instance to the contrary, doesn't prove that there is any law; and

•one where a much lower frequency of coincidence (perhaps even lower than the frequency of non-coincidence) does prove that there is a law.

The same principle is at work in both. In both we consider the positive frequency of the phenomena themselves, and on that basis calculate how frequently they would coincide if there were no connection •between them or •between one of them and some cause tending to block the other. If we find a greater frequency of coincidence than this, we conclude that

there's some connection: one of the phenomena can under some circumstances cause the other, or there's something capable of causing them both.

And if we find a lesser frequency, we conclude that

there's some blocking: one of the phenomena, or some cause that produces one of them, can counteract the production of the other.

We have thus to deduct from the observed frequency of coincidence as much as can be the effect of chance, i.e. of the mere frequency of the phenomena themselves; and the remainder—if there is one—is the residual fact that proves the existence of a law.

The frequency of the phenomena can be ascertained only within definite limits of space and time. That's because it depends on the quantity and distribution of the primeval natural agents, and we can't know anything about that except by human observation, since we can't find any law about it enabling us to infer the unknown from the known. But for present purposes this is no disadvantage, because it merely confines the question within the same limits as the data. The coincidences occurred in certain places and times, and within those we can estimate how frequently such coincidences would be produced by chance. If we find from observation that A exists in one case out of every two, and B in one case out of every three, then if there's neither connection nor opposition between them or between any of their causes, the instances in which A and B will both exist, i.e. will coexist, will be one case in every six. For A exists in three cases out of six; and B—existing in one case out of every three independently of whether A is present or absent—will exist in one case out of those three. Of the six cases, therefore, we can expect there to be

•two in which A exists without B,

•one in which B exists without A,

•two in which neither B nor A exists, and

•one in which they both exist.

If we find that A and B coexist oftener than in one case out of six,. . . .there is some cause in existence that tends to produce a conjunction between A and B.

Generalising this result, we can say that if A occurs in a larger proportion of the B cases than of the not-B cases, then B will also occur in a larger proportion of the A cases than of the not-A cases, and there's some causal connection between A and B. If we could track back to the causes of A and B, we would find somewhere along the line some cause

or causes common to both; and if we could ascertain what these are we could form a generalisation that would be true without restriction of place or time. But until we can do that, the fact of a connection between A and B remains an empirical law.

§3. Having considered how it can be determined whether any given conjunction of phenomena is •casual or •the result of some law, we need now to complete the theory of chance by considering the effects that are partly the result of chance and partly of law, i.e. cases where •the effects of casual conjunctions of causes are habitually blended in one result with •the effects of a constant cause.

This is composition of causes, with a special feature: instead of two or more causes intermixing their effects in a regular manner, we now have one constant cause producing an effect that is successively modified by a series of variable causes. As summer advances, the sun's approach towards a vertical position tends to produce a constant •increase of temperature; but •this effect of a constant cause is mixed with the effects of many variable causes—winds, clouds, evaporation, lightning and the like—so that the temperature on any given day depends partly on these fleeting causes and only partly on the constant cause. If the effect of the constant cause is always accompanied and disguised by effects of variable causes, it's impossible to ascertain the law of the constant cause in the ordinary way by observing it apart from all other causes. That creates a need for an additional rule of experimental inquiry.

When the action of a cause A is liable to be interfered with. . . .by different causes at different times, and when these are so frequent or so indeterminate that we can't exclude them all from any experiment, though we can vary them, we can try to discover what the effect is of all the variable causes taken together. This is how we do it:

> •We make as many trials as possible, keeping A invariable and varying everything else as much as possible. The results of these different trials will naturally be different, because their indeterminate modifying causes are different. If we find that these results oscillate about a certain point—one experiment giving a result a little greater, another a little less; one giving a result tending a little more in one direction, another a little more in the opposite direction—while the average or mid-point doesn't vary. . . ., then that mean or average result is the part in each experiment that is due to the cause A, and is the effect that would have occurred if A had acted alone; the variable remainder is the effect of chance, i.e. of causes whose coexistence with A was merely casual.

This induction counts as sufficient if any increase in the number of trials doesn't materially [see Glossary] alter the average.

This kind of elimination, in which what we eliminate is not one assignable cause but a multitude of floating unassignable ones, can be called 'the elimination of chance'. We produce an example of it when we repeat an experiment ·several times· so as to get rid of the effects of the unavoidable errors of each individual experiment by taking the mean of the different results. When there's no permanent cause that would produce a tendency to error in one direction, we are justified by experience in assuming that the errors on one side will—in a certain number of experiments—just about balance the errors on the opposite side. So we go on repeating the experiment until any change in the over-all average falls within limits of experimental error. How those

limits are set will depend on what we are aiming to discover by our inquiry. [1]

§4. I have been assuming that the effect of the constant cause A is such a large and conspicuous part of the over-all result that there's no room for doubt that it exists, and the eliminating process is merely an attempt to ascertain *how much* of the over-all result is caused by A, i.e. to discover what A's exact law is. But in some cases the effect of a constant cause is such a small portion of the total upshot that it escapes notice; and the fact that there is an effect arising from a constant cause is first learned by the process whose usual role is only to ascertain the quantity of that effect. This happens in cases where a given effect E is •known to be chiefly determined by changeable causes and •not known not to be wholly so determined. In that situation we run a large number of trials, watching to see if we get either of these results:

  (i) The effects of the different changeable causes cancel one another out, ·and the series homes in on E·.
  (ii) The long series of trials homes in on a definite result, but it isn't precisely E; it differs from E by an amount that is small in comparison with the total effect, but it is definitely there in our results.

In case **(i)** we conclude that the changeable causes are the only cause of E; in **(ii)** we conclude that some constant cause is at work, making the results of our trials oscillate around a definite point that isn't quite E, and we may hope to discover what that cause is by some of the methods I have presented. This last process can be called the *discovery of a residual phenomenon by eliminating the effects of chance.*

That is how loaded dice can be discovered. Of course no die is so clumsily loaded that it always comes up with the same number; if that happened, the fraud would be instantly detected. The loading, a constant cause, mingles with the changeable causes that determine how the die is thrown in each individual instance. If the die wasn't loaded and the throw depended entirely on those changeable causes, in a long enough series of throws the changeable causes would balance one another so that the numbers on the die would come up about the same number of times. If we throw the die often enough so that we stop having any material effect on the relative frequencies of the numbers, and find that the stable distribution of numbers that we eventually reach has one number coming up significantly more often than any other, we can be sure that some constant cause is at work favouring that number—i.e. that the die is not fair—and we know exactly *how* unfair it is. . . .

§5. After these general remarks about the nature of chance, I'm now ready to consider how we can become sure that a conjunction between two phenomena that has been observed a certain number of times is not casual, but a result of causation. When we *are* sure of that we can accept this going-together of the two phenomena as one of the uniformities of nature, though (until accounted for deductively) only as an empirical law.

Suppose that the phenomenon B has *never* been observed except in conjunction with A. Even then, the probability that they are connected isn't measured by the total number of instances in which they have been found together, but by that number minus the number stating the absolute

---

[1]   I have been speaking of the **mean** as if it were the **average**. But for purposes of inductive inquiry the mean is *not* the average or arithmetical mean, though the difference can be disregarded in informal illustrations of the theory. If the deviations on one side of the average are much more numerous than those on the other (these last being fewer but bigger), the effect due to the invariable cause won't coincide with the average but will be either below or above it, the deviation being toward the side on which the greatest number of the instances are found. . . .

frequency of A. If A exists always, and therefore coexists with everything, no number of instances of *A together with B* would prove a connection—as in the example of the fixed stars. If A occurs so commonly that we can presume it to be present in half of all the cases that occur, and therefore in half the cases in which B occurs, our only evidence that there's a connection between A and B would have to come from A's occurring in more than half the occurrences of B.

In addition to the question

(i) In a great multitude of trials, how many coincidences can on average be expected to arise from chance alone?

there is also the question

(ii) In a number of instances smaller than that required for striking a fair average, how much deviation from that average can be expected from chance alone?

That is, we have to consider not only (i) the general result of the chances in the long run, but also (ii) what the extreme limits of variation from the general result are that can occasionally be expected as the result of some smaller number of instances. The consideration of (ii) and any further consideration of (i) belong to what mathematicians term the doctrine of chances, or in a grander phrase, the Theory of Probabilities.

Chapter 18. The calculation of chances

§1. In his *Essai philosophique sur les probabilités*, Laplace wrote:

'Probability has reference partly to our ignorance, partly to our knowledge. We know that among three or more events exactly one must happen, but we have no grounds on which to pick just one and believe that *it* will happen. In this state of indecision, we can't say with certainty anything about which one will occur. But we can say of each of them that it probably won't occur, because we know of several equally possible events that exclude its occurrence, and only one that favours it.

'The theory of chances consists in •reducing all events of the same kind to a certain number of equally possible cases, i.e. cases that we are equally undecided about; and •determining the number of these cases that are favourable to the event whose probability we are looking for. The ratio of that number to the number of all the possible cases is the measure of the probability. So the probability ·of an event· is a fraction, having for its numerator the number of cases favourable to the event and for its denominator the number of all the possible cases.'

For a calculation of chances, then, according to Laplace, two things are necessary: •we must know that of several events some one and only one will certainly happen; and •it mustn't be the case that we know, or have any reason to expect, that this or that one in particular is going to happen. It has been contended that these aren't the only requirements, and that Laplace has overlooked a necessary part of the foundation of the doctrine of chances. To declare two events to be equally probable (say these critics) we need **three** things:

•to know that one of the two must happen,
•to *not* know which one will happen, and
•to know from experience that the two events occur equally often.

Why when we flip a coin do we think it to be equally probable that it will come up heads or tails? Because we know that in any large number of throws, heads and tails are thrown about equally often; and that the more throws we make the nearer we come to perfect equality. We can if we wish know this •by actual experiment, or •by the daily experience that life gives us of events of the same general sort, or •deductively from the effect of mechanical laws on a symmetrical body acted on by forces varying indefinitely in quantity and direction. We may know it, in short, either by specific experience or on the evidence of our general knowledge of nature. But we must know it somehow if we are to call the two events equally probable; and if we don't know it, we're running as much risk in staking equal sums on the result as in laying odds.

That's the view of the subject that I took in the first edition of the present work; but I have since become convinced that the theory of chances, as conceived by Laplace and by mathematicians generally, doesn't have the basic fallacy of which I had accused it.

Remember that an event's probability is not a quality of the event itself; it's merely a name for the strength of the grounds that we or others have for expecting it. The probability of an event to you is a different thing from its the probability to me, and also different from the probability to you after you have acquired additional evidence. The probability to me that John Doe, of whom I know nothing but his name, will die within the year is totally altered when I'm told that he has severe tuberculosis. Yet this knowledge of mine makes no difference to the event itself or to any of the causes on which it depends. Every event is in itself •certain, not •probable; if we knew all ·the relevant facts· we would either know that it will happen or know that it won't. Its probability to us is just the degree of expectation of its

occurrence that our present evidence entitles us to have.

Bearing this in mind, I think it must be admitted that even when we have no knowledge to guide our expectations except the knowledge that what happens must be some one of a certain number of possibilities, we can still reasonably judge that one supposition is more probable to us than another; and if the outcome matters to us in any way, we ought to act on that judgment.

§2. Suppose we're required to take a ball from a box of which we know only that it contains black balls and white ones, and none of any other colour. We know that the ball we select will be either black or white, but we have no basis for expecting one colour rather than the other. In that case, if we have to make a choice and bet something on one or the other supposition, we'll have no prudential reason to select either colour, and we'll act precisely as we would have acted if we had known that the box contained an equal number of black and white balls. But though our conduct would be the same, it wouldn't be based on a guess that the balls were in fact equally divided. To see why, suppose we •know for sure that the box contains 99 balls of one colour and only one of the other, but •don't know which colour has only one and which has 99; in that case the drawing of a white and of a black ball will be equally probable to us. We'll have no reason for staking anything on one event rather than on the other; the option between the two will be a matter of indifference; in other words, it will be an even chance.

Now vary the case: suppose that instead of two there are three colours—white, black, and red—and that we're entirely ignorant about how many of each. We have no reason to expect one more than another, and if we have to bet we'll regard each colour as on a par with each of the other two. But if there's a question of betting on (say) *white* as against

red or black, would it be a matter of indifference which way we betted? Surely not! Because black and red are each as probable to us as white, the two together must be twice as probable. We would in this case expect *not-white* rather than *white*, and so much 'rather' that we would bet two to one on it. It's true that for all we know there may be more white balls than black and red together; and in that case our bet would, if we knew more, be seen to be a disadvantageous one. But then for all we know to the contrary there may be more red balls than black and white, or more black balls than white and red, and in that case the effect knowing more would be to prove to us that our bet was more advantageous than we had supposed it to be. But in the actual state of our knowledge there's a rational probability of two to one against white–a probability fit to be made a basis of conduct. No reasonable person would lay an even wager in favour of white against black and red; though against black alone or red alone he might do so without imprudence.

So the common theory of the calculation of chances seems to be tenable. Even when we know nothing except the number of the possible and mutually excluding contingencies, and are entirely ignorant of their comparative frequency, we may have grounds—ones that can be evaluated numerically—for acting on one supposition rather than on another; and this is the meaning of probability.

§3. The reasoning here is based on the obvious principle that when there are several mutually exclusive kinds of possible outcome, it's impossible for each of those kinds to be a majority of the whole. On the contrary, there must be a majority against each kind except one at most; and if

any kind has more than its share in proportion to the total number, the others collectively must have less. Granting this axiom, and assuming that we have no ground for selecting any one kind as more likely than the rest to surpass the average proportion, it follows that we can't rationally presume this in our betting. Thus, even in this extreme case of the calculation of probabilities—which doesn't rest on special experience at all—the logical ground of the process is our knowledge of the laws governing the frequency of occurrence of the different cases. But *this* is knowledge of universal and axiomatic truths, and doesn't bring in any specific experience or any considerations arising from the special nature of the problem under discussion.

But I can't conceive of a case where we ought to be satisfied with an estimate of chances based on the absolute minimum of knowledge concerning the subject—except in games of chance, the purpose of which requires ignorance instead of knowledge. It's clear that in the case of the coloured balls a very slight ground for thinking that the white balls outnumbered each of the other colours would undermine the whole calculation made in our previous state of ·ignorance and· indifference. It would equip us with more advanced knowledge, in which the probabilities-to-us were different from what they had been before; and in estimating these new probabilities we would have to proceed on a totally different set of data, provided by specific knowledge of facts rather than by mere counting of possible suppositions. We ought always to try to get such additional data, and it's •always possible to get some that are, if not good bases for action, at least better than none at all; well, •always unless

---

[1]   [The marker for this footnote occurs high on the next page.] It seems to me, indeed, that the calculation of chances in the absence of data based on special experience or on special inference must in the vast majority of cases *break down*, from sheer impossibility of assigning any principle by which to be guided in setting out the list of possibilities. In the case of the coloured balls we can easily list the possibilities because *we* determine what they are. But now take a case that is more like the ones that occur in nature: instead of three colours, let the box contain all possible colours,

we're dealing with something that is equally beyond the range of our means of knowledge and our practical uses.[1]

It is obvious, too, that even when the probabilities are derived from observation and experiment,

- •a very slight improvement in the data, by better observations or by attending more fully to the special circumstances of the case

is more useful than

- •the most elaborate application of the calculus to probabilities based on the unimproved data.

The neglect of this obvious point has led to misapplications of the calculus of probabilities that have made it the scandal of mathematics. Just look at how it is applied to **(i)** the credibility of witnesses and to **(ii)** the correctness of the verdicts of juries. **(i)** Common sense would dictate that it is impossible to say what the average level of truthfulness etc. is for mankind in general or for any class of people ·or indeed for any individual·; and even if this were possible, the use of it for such a purpose—·i.e. for deciding how much trust to place in the testimony of witness John Doe·—reveals a misunderstanding of the use of averages. . . . In the case of a witness, persons of common sense will go by the degree of consistency of his statements, his conduct under cross-examination, and the relation of the case itself to his interests, his partialities, and his mental capacity, instead of applying such a rough standard (even if it could be verified) as the ratio of true to erroneous statements that he had made in the course of his life.

(ii) Some mathematicians have set out from the proposition that the judgment of any one judge or juryman is at least *somewhat* more likely to be right than wrong, and have inferred from this that the chance of a number of persons all reaching the same wrong verdict is small in proportion that the number of judges or jurymen is large; so that if there are enough judges the correctness of their judgment can be raised almost to certainty. This raises the question of the effect on the moral position of the judges by multiplying their numbers—the virtual destruction of their individual responsibility, and the weakening of their mental focus on the subject—but let all that pass. I merely point out the fallacy of reasoning from •a wide average to •cases necessarily differing greatly from any average. If we look at all the legal cases judge J has been involved in, perhaps he has more often been right than wrong; but now look at his record in all the complicated and otherwise tricky cases he has been involved in, it's likely enough that in them he has more often been wrong than right. (Why focus on the difficult cases? Because it's only in them that it matters much who the judges are.) And there's another point: if judge J's errors in tricky cases have arisen from the intricacy of the case or from some common prejudice or mental infirmity, the odds are that such factors will have acted on most of the other judges in the same way; so that increasing the number of judges will *increase* the probability of a wrong decision.

These are merely *samples* of the errors often committed by men who, having learned to use difficult algebraic formu-

---

and suppose that we are ignorant of the comparative frequency with which different colours occur in nature or in the productions of art. How are we to list the possibilities? Is every distinct shade to count as a colour? If so, is the test ·of distinctness· to be ·conducted by· a common eye or an educated one—a painter's, for instance? Answers to these questions could make the difference between whether ·the chances against some particular colour should be estimated at 10:1 or 20:1 or perhaps 500:1. Whereas if we knew from experience that the particular colour occurs on an average of (say) 33 times in every hundred or thousand, we wouldn't need to know anything about the frequency of the other possibilities or even about how many of them there are.

lae in estimating chances in complex cases, would rather
•use those formulae to compute what the probabilities are
to a person who is half-informed about a case than •look
for ways of being better informed. If we're to get anything
scientifically useful out of the doctrine of chances, we must
first lay a foundation for an evaluation of the chances by
getting as much ·relevant· factual knowledge as we can. The
knowledge required is that of the comparative frequency with
which the different events actually occur. For the purposes of
the present work, therefore, it is permissible to suppose that
conclusions about the probability of a fact of kind K rest on
our knowledge of the proportion between •the cases where
K facts occur and •those in which they don't occur; this
knowledge being either derived from specific experiments or
deduced from our knowledge of causes that tend to produce
K facts compared with causes that tend to prevent them.

Such calculation of chances is based on an induction,
and the calculation isn't legitimate unless the induction is
valid. It's not stopped from being an induction by the fact
that it doesn't prove that a K event occurs in all cases of sort
S but only that out of a given number of S cases a K event
occurs in about such-and-such a number. The fraction that
mathematicians use to designate the probability of an event
is the ratio of these two numbers; the ascertained proportion
between •the number of cases in which a K event occurs
and •the number of all the cases (i.e. those in which a K
event does occur plus those in which it doesn't). In playing
at coin-tossing, the S cases are throws of the coin, and the
probability of heads is one-half because if we throw often
enough heads is thrown about half the time. In the cast of a
die, the probability of 6 is one-sixth; not simply

> •because there are six possible outcomes of which 6 is
> one, and we know no reason why one should turn up
> rather than another,

—though I have accepted the validity of this ground if it were
the best we could do—but

> •because we do actually know, either by reasoning or
> by experience, that in a hundred or a million throws
> 6 is thrown in about one-sixth of that number.

§4. When I say 'either by reasoning or by experience' I mean
specific experience. When we are estimating probabilities it
makes a difference which of these two is the basis for our
assurance. The probability of events •as calculated from their
mere frequency in past experience is a less secure basis for
practical guidance than their probability •as deduced from
an equally accurate knowledge of the frequency of occurrence
of their causes.

The generalisation that an E event occurs in **ten out
of every hundred** S cases is as real an induction as if the
generalisation were that it occurs in **all** S cases. But when
we reach this conclusion by merely counting S instances
in actual experience and comparing the number of them in
which an E has occurred with the number in which it hasn't,
our evidence is only that of the Method of Agreement, and
the conclusion amounts to a mere empirical law. We take a
step beyond this when we •ascend to the causes on which
the occurrence or non-occurrence of E events depends, and
•form an estimate of the comparative frequency—·among
all S cases·—of the causes favourable to E and of those
unfavourable to E. These are data of a higher order, by
which the merely empirical law. . . .will be either corrected or
confirmed, and either way we'll get a more correct measure
of probability than is given by the numerical comparison
underlying the empirical law. A writer in the *Prospective
Review* recently said, rightly, that in the kind of examples by
which the doctrine of chances is usually illustrated—namely,
balls in a box—the estimate of probabilities is supported

by reasons of causation, which are stronger than reasons from specific experience. 'What is the reason that in a box where there are nine black balls and one white, we expect to draw a black ball nine times as much (i.e. nine times as often, frequency being the gauge of intensity in expectation) as a white? Obviously because the local conditions are nine times as favourable; because the hand may alight in nine places and get a black ball, while it can only alight in one place and find a white ball; like the reason why we don't expect to succeed in finding a friend in a crowd, because the conditions for our coming together are so many and so difficult. This wouldn't hold to the same extent if the white ball were larger than the black ones, and in that case the probability would be different.'

It is in fact obvious that once causation has been admitted as a universal law, that law becomes the only rational basis for our expectation of events. For someone who recognises that every event depends on causes, a thing's having happened once is a reason for expecting it to happen again only because it happening once shows that there is—or is liable to be—a cause adequate to produce it. The frequency of the particular event, apart from any thought of its cause, can't give rise to any induction except an *inductio per enumerationem simplicem* [see Glossary]; and the precarious conclusions reached in this way are superseded, and disappear from the ·battle·field as soon as the principle of causation shows up there.[1]

Still, although an estimate of probability based on causes is theoretically better, in practice it can't be done much. In almost all cases where chances can be estimated precisely enough to be of any practical value, the numerical data are drawn not from •knowledge of the causes but from •experience of the events themselves—

> •the probabilities of life at different ages or in different climates;
> •the probabilities of recovery from a particular disease;
> •the chances of the birth of male or of a female offspring;
> •the chances of the destruction of houses by fire;
> •the chances of the loss of a ship in a particular voyage;

—these are all deduced from mortality statistics, hospital records, registers of births, registers of shipwrecks, and so on, i.e. from the observed frequency not of the causes but of the effects. We go about it in this way because in all these contexts the *causes* are either not open to direct observation at all, or not with the required precision, and

---

[1] [This footnote which Mill tied to '. . . a cause adequate to produce it' a few lines back, is a quotation from the *Prospective Review* article mentioned in the preceding paragraph.] 'Why do we feel so much more probability added by •the first instance than by •any single subsequent instance? It has to be because the first instance gives us its possibility (a cause adequate to it), while every other only gives us the frequency of its conditions. If no reference to a cause were implied, *possibility* would have no meaning; yet it's clear that before the event happened we might have thought it to be impossible, i.e. have believed that there was no physical energy really existing in the world equal to producing it. . . The first time of happening, then, is more important to the whole probability than any other one instance (because it proves the possibility); after that, the number of times becomes important as a sign of the intensity or extent of the cause, and its independence of any particular time. Suppose we want to estimate the probability someone's being able to perform a tremendous leap a certain number of times; at first we don't know whether the leap is possible, but the all-important first leap gets rid of that doubt. Every leap after that shows the power to be •more perfectly under ·the athlete's· control, •greater, and •more invariable, and so it increases the probability. No-one would think of reasoning in this case directly from one instance to the next, without referring to the physical energy that each leap indicated. So it's clear that we do not ever conclude directly from the happening of an event to the probability of its happening again; rather, we refer to the cause, regarding the past cases as a sign of the cause, and the cause as our guide to the future.' [Mill interrupts this by suggesting that '. . . we do not ever. . .' should be '. . . we do not in an advanced state of our knowledge. . .'.]

we have no way of judging of their frequency except from the empirical law provided by the frequency of the *effects*. But the inference still entirely depends on causation alone: we reason from an effect to a similar effect by passing through the cause. If the actuary in an insurance office infers from his tables that among a hundred 50-year-old persons now living five on average will reach the age of seventy, his inference is legitimate; not for the simple reason that this is the proportion who have reached seventy in the past, but because that statistical fact shows that 5:95 is the proportion existing at that place and time between the causes that prolong life to the age of seventy and the causes tending to bring it to an earlier close.[1]

§5. From the preceding principles it's easy to work out how to demonstrate the theorem that is the basis for the use of the concept of probability in application to •the occurrence of a given event or •the reality of an individual fact. The signs or evidences by which a fact is usually proved are some of its consequences; and the inquiry hinges upon determining what cause is most likely to have produced a given effect. The theorem applicable to such investigations is the Sixth Principle in Laplace's *Essai philosophique sur les Probabilités*, which he describes as the 'fundamental principle of the branch of the Analysis of Chances that consists in ascending from events to their causes.'[2]

Given an effect to be accounted for, with several causes that might have produced it, though nothing is known about their role (if any) in this particular case, the probability that the effect was produced by any one of these causes is •the antecedent probability of that cause multiplied by •the probability that that cause, if it existed, would produce the given effect.

Let E be the effect and C_1 and C_2 the two causes by either of which E might have been produced. To find the probability that it was produced by C_1 (say), ascertain which of the two is more likely to have existed, and which of them, if it did exist, was more likely to produce the effect E: the probability sought is a compound of these two probabilities. [Mill has slipped here. He speaks of 'these two probabilities', but he hasn't mentioned two probabilities, only two probability-comparisons.]

CASE I: The causes are alike in the second respect; C_1 and C_2 when they exist are equally likely (or equally certain) to produce E; but C_1 is twice as likely as C_2 to exist, i.e. is twice as frequent a phenomenon. Then it is twice as likely to have existed in this case, and to have been the cause that produced E.

Explanation: C_1 exists in nature twice as often as C_2, so in any 300 cases in which one or other existed, C_1 has existed 200 times and C_2 100. But either C_1 or C_2 must have existed wherever E is produced; therefore, in 300 times that

---

[1]   The writer last quoted says that estimating chances by comparing the number of cases in which the event occurs with the number in which it doesn't 'would generally be wholly erroneous' and 'is not the true theory of probability'. Well, it's the theory that forms the foundation of insurance, and of all the calculations of chances in the business of life. The writer's reason for rejecting the theory is that it 'would regard as certain an event that had never failed up to now; which is very far from the truth, even for a very large number of constant successes.' This isn't a defect in a particular theory, but in *any* theory of chances. No principle of evaluation can deal with a case such as this writer supposes. If an event has never once failed in a long enough series of trials to eliminate chance, it has all the certainty that an empirical law can provide; it is certain for as long as the relevant collocation of causes continues. If it ever fails, it will be because of some change in that collocation. Now, *no* theory of chances will enable us to infer the future probability of an event from the past, if the relevant causes have undergone a change.

[2]   Laplace doesn't state the theorem in exactly they way I have stated it, but it's easy to demonstrate that the two formulations are equivalent.

E is produced, C_1 was the producing cause 200 times, C_2 only 100; i.e. in the ratio of 2 to 1. Thus, if the causes are alike in their ability to produce the effect, the probability as to which actually produced it is in the ratio of their prior probabilities.

CASE II: The causes are equally frequent, i.e. equally likely to have existed, but not equally likely if they did exist to produce E. Specifically, in three times in which C_1 occurs it produces that effect twice, while C_2 in three occurrences produces it only once. Since the two causes occur with equal frequency, in every six times that either one or the other exists, C_1 exists three times and C_2 three times. C_1 produces E in two of its three occurrences, while C_2 produces E once in its three occurrences. Thus, in the whole six times, E is produced only three times; but of those three it is produced twice by C_1 and only once by C_2. Consequently, when the antecedent probabilities of the causes are equal, the chances that the effect was produced by them are in the ratio of the probabilities that if they did exist they would produce the effect.

CASE III: The causes are unalike in both respects. This is solved by the solutions of Cases I and II. For, when a quantity depends on two other quantities in such a way that while either of them remains constant it is proportional to the other, it must be proportional to the product of the two quantities, *product* being the only function of the two that obeys that law of variation. Therefore, the probability that E was produced by either cause is the antecedent probability of the cause's existing multiplied by the probability that if it existed it would produce E. QED.

Explanation: Let C_1 occur twice as often as C_2; and let C_1 produce E twice in four occurrences, and C_2 produce E three times in four occurrences. C_1's antecedent probability is to C_2's as 2 to 1; the probabilities of their producing E are as 2 to 3; the product of these ratios is the ratio of 4 to 3; and this will be the ratio of the probabilities that C_1 or C_2 was the producing cause in the given instance. Since C_1 is twice as frequent as C_2, out of twelve cases in which one or other exists, C_1 exists in 8 and C_2 in 4. But out of its eight occurrences C_1 produces E in only 4, while C_2 out of its four cases produces E in 3. So E is produced at all in seven of the twelve cases; in four of these it is produced by C_1, in three by C_2; hence the probabilities of its being produced by C_1 and by C_2 are as 4 to 3, and are expressed by the fractions $4/7$ and $3/7$. QED.

§6. How does the doctrine of chances relate to the special problem I discussed in chapter 17? I mean the problem of how to distinguish coincidences that are casual from ones that are the result of law, i.e. from ones in which the facts that accompany or follow one another are somehow connected through causation.

The doctrine of chances provides means by which, if we know the average number of coincidences to expect between two phenomena connected only casually, we can calculate how often any given deviation from that average will occur by chance. If the probability of any casual coincidence is $1/m$, the probability that the same coincidence will be repeated n times in succession is $1/(m \times n)$. In one throw of a die the probability of 4 is $1/6$; so the probability of throwing 4 twice in succession is $1/6^2 = 1/36$. To see why, consider: 4 is thrown once in six throws, or **six** in thirty-six throws; and of those **six**, when die is cast again 4 will be thrown only once; making once in thirty-six throws altogether. The chance of throwing 4 three times in succession is $1/6^3$, which is $1/216$. . . .

So we have a rule by which to estimate the probability that any given series of coincidences [see Glossary] arises from chance, provided we know the probability of a single coincidence. If we can get an equally precise expression for the probability that the same series of coincidences arises from causation, we'll only have to compare the numbers. But we usually can't do this. Let us see how near we can come, in practice, to the necessary precision.

The question falls within ·the scope of· Laplace's sixth principle, which I have just demonstrated. The series of coincidences may have originated either in **(i)** a casual conjunction of causes or in **(ii)** a law of nature. The probability that the series originated in manner **(i)** is given by •the antecedent probability of its being the case multiplied by •the probability that if it were the case it would produce that series of coincidences; and similarly, *mutatis mutandis*, for the probability that the series came from **(ii)** a law of nature. Well, the two are on a par as regards probability-of-producing-the-effect: if either of them were real, that series of coincidences would certainly occur. So the probability that the coincidences are produced by this or that one of the two causes is the antecedent probability of that cause's existing. The antecedent probability of **(i)** is a quantity we can measure. How exactly we can estimate the antecedent probability of **(ii)** will vary according to the nature of the case.

In some cases if the coincidence is result of causation we know what the cause must be—e.g. we know that if a consecutive series of 4s isn't accidental it must arise from the loading of the die. In such a case we may have a basis, in the characters of the parties concerned or other such evidence, for a conjecture as to the antecedent probability of such an event; but we can't possibly estimate that probability with anything like numerical precision. But the counter-probability—i.e. the probability that a consecutive series of

4s is accidental—dwindles very fast as the series continues; so that we soon reach the stage at which the chance that the die has been loaded, however small in itself, must be greater than the chance of a casual coincidence; and on this basis a practical decision can generally be reached without much hesitation if it's possible to repeat the experiment.

But when the situation is like the one we were looking at in chapter 17—i.e. when the coincidence can't be accounted for by any known cause, so that if the connection between the two phenomena is causal it must be the result of some law of nature that we don't yet know—then ·we have a new problem·. We may be able to estimate the probability of a casual coincidence, but the probability of the counter-supposition, namely the existence of an undiscovered law of nature, is clearly something we can't estimate even approximately. To have a basis for such an estimate we would need to know

> •what proportion of all the individual sequences or coexistences occurring in nature are the result of law, and •what proportion are mere casual coincidences.

Obviously, we can't make any plausible conjecture about this proportion, much less assign it a number; so we can't attempt any precise estimation of the comparative probabilities. But we are sure of this much: the detection of an unknown law of nature—of some previously unrecognised constancy of conjunction among phenomena—is not an uncommon event. Therefore, if

> •the number n_o of instances in which a coincidence is observed is so much larger than •the number n_c that would occur on the average from chance that it would be an extremely uncommon event for n_o coincidences to occur from accident alone,

then we have reason to conclude that the coincidence is an effect of causation and can be accepted (subject to correction from further experience) as an empirical law. We can't pin it

down more precisely than this, but in most cases this level of precision is all we need to resolve any *practical* doubt.

[This was originally a footnote.]   For a fuller treatment of many interesting questions in the theory of probabilities I recommend John Venn's recent *The Logic of Chance*, which is one of the most thoughtful and philosophical works on

any subject connected with logic and evidence that I know of. Some criticisms ·of my work· contained in it have helped me to revise the corresponding chapters of the present work. Any reader of Venn's work who is also a reader of this will see which of his opinions I don't accept.

Chapter 19. Extending derivative laws to adjacent cases

§1. I have frequently remarked that derivative laws are less general than the ultimate laws they are derived from, and also less certain. This is most conspicuous in the uniformities of coexistence and sequence between effects that depend ultimately on different basic causes. Such uniformities always reflect the same collocation of those primeval causes—·i.e. the causes coexist if the effects do, and occur in sequence if the effects do·. If the collocation of the causes varies, though the laws of the causes remain the same, the set of derivative uniformities can and usually will be totally different.

Even where the derivative uniformity is between different effects of a single cause, it won't exist as universally as the law of the cause does. If a and b accompany or succeed one another as effects of the cause C_1, it doesn't follow that C_1 is the only cause that can produce them, or that if there's another cause C_2 that can produce a it must produce b likewise. So it may be that the conjunction a and b doesn't hold universally, but holds only in the instances in which a arises from C_1. When it is produced by some other cause, a and b may be separated. Day is always in our experience followed by night; but day isn't the cause of night; both are successive effects of a common cause, the spectator's periodical move into and out of the earth's shadow, resulting

from •the earth's rotation and •the illuminating power of the sun. So if day is ever produced by a different set of causes from this, day may not be followed by night. On the sun's own surface, for instance, this may be the case.

Finally, even when the derivative uniformity is itself a law of causation (resulting from the combination of several causes), it isn't entirely independent of collocations. If a cause intrudes that wholly or partially counteracts the effect of any one of the combined causes, the effect will no longer conform to the derivative law. Thus, while each ultimate law is vulnerable to frustration from *one* set of counteracting causes, the derivative law is vulnerable to it from *several*. And the possibility of the occurrence of counteracting causes that don't arise from any of the conditions involved in the law itself depends on the original collocations.

It is true that laws of causation, whether ultimate or derivative, are in most cases fulfilled even when counteracted (I said this on page 220)—the cause produces its effect though that effect is destroyed by something else. So the fact that the effect can be frustrated doesn't harm the universality of the law governing the cause. But it is fatal to the universality of the sequences or coexistences of effects that are the subject-matter of most of the derivative laws flowing from laws of causation. . . . Here's an example.

From the combination of •a single sun with •an opaque body's rotation around its axis there results •an alternation of day and night on the whole surface of that opaque body. If one of the combined causes were counteracted—the rotation stopped, the sun extinguished, or a second sun added—this wouldn't affect the truth of that particular law of causation; it would be still true that *one sun shining on an opaque revolving body will alternately produce day and night*; but. . . .the derivative uniformity, the succession of day and night on the given planet, would no longer hold.

So the derivative uniformities that aren't laws of causation always depend to some extent on collocations; and that exposes them to the characteristic infirmity of empirical laws—namely, being acceptable only where the collocations are known by experience to be required for the truth of the law, i.e. only within the conditions of time and place confirmed by actual observation. (I said 'always'; it should have been 'always except in the rare case where they depend on one cause rather than a combination of causes'.)

§2. This principle, when stated in general terms, seems clear and indisputable; yet many of the ordinary judgments of mankind—ones that no-one challenges as improper—-seem to be inconsistent with it. On what grounds, it may be asked, do we expect that the sun will rise tomorrow? The time-span through which we have made observations includes thousands of past years, but it doesn't include the future. Yet we infer with confidence that the sun will rise tomorrow, and nobody doubts that we're entitled to do so. Let us consider what is the basis for this confidence.

In the example in question, we know the causes that the derivative uniformity depends on. They are •the sun giving out light and •the earth rotating and intercepting light. Given a completed induction showing these to be real causes, and not merely. . . .effects of a common cause, the only circumstances that could defeat the derivative law are ones that destroy or counteract one of the combined causes. For as long as the causes exist and aren't counteracted, the effect will continue. If they exist and aren't counteracted tomorrow, the sun will rise tomorrow.

Since the causes will exist until something destroys them, everything depends on the probabilities of their being destroyed or counteracted. We know by observation. . . .that these phenomena have continued for (let's say) 5,000 years. Within that time no cause has appreciably weakened them or counteracted their effect. So the chance that the sun won't rise tomorrow amounts to the chance that some cause that hasn't shown up in the smallest degree during 5,000 years will exist tomorrow with enough intensity to destroy the sun, the earth, the sun's light, or the earth's rotation, or to produce an immense disturbance in the effect resulting from those causes.

If such a cause *will* exist tomorrow or at any future time, some cause of that cause must exist now and must have existed during the whole 5,000 years. So if the sun doesn't rise tomorrow, that will be because there is some cause whose effects

> •have through 5,000 years been too small to be perceptible, but
> •will overnight become overwhelming.

Since this cause hasn't been recognised during all those years by observers on our earth, if it's a single cause it must either •be one whose effects develop gradually and very slowly or •one that existed in regions beyond our observation and is now on the point of arriving in our part of the universe. Now, all causes that we have experience of act according to laws

incompatible with the supposition that their effects could be imperceptible for 5,000 years and then swell to immensity in a single day. No mathematical law of proportion between an effect and the quantity or relations of its cause could produce such contradictory results. The sudden development of an effect of which there was no previous trace always arises from the coming together of several distinct causes that haven't previously been conjoined; but if such a sudden conjunction is going to take place tonight, the causes (or *their* causes) must have existed during the entire 5,000 years; and their not having once come together during all that time shows how rare that particular combination is. So we have a rigid induction to support us in thinking that *the known conditions required for the sun's rising will exist tomorrow* is probable in a degree that can't be distinguished from certainty.

§3. But this extension of derivative (not causative) laws beyond the limits of observation can only be to *adjacent* cases. If instead of 'tomorrow' I had said 'twenty thousand years from today', the inductions would have been anything but conclusive. ·That is, it's not out of the question that in that stretch of time something might happen to stop the sun from rising·. Consider:

> A cause that has, in opposition to very powerful causes, produced no perceptible effect during a considerable period will produce a very considerable effect by the end of a further much longer period.

Nothing in *that* conflicts with our experience of causes. ·There are at least three ways it could happen·:

(1) An agent whose effect over the past 5,000 years •hasn't amounted to a perceptible quantity •becomes considerable by accumulating over the next 20,000 years.

(2) There is moving towards us some ·heavenly· body that •hasn't influenced us during ·the past· 5,000 years but •will get close enough to produce extraordinary effects on us in ·the next· 20,000 years.

(3) Sunrise could be prevented by a certain combination of causes; and although that combination hasn't arisen in the past 5,000 years it will arise in the next 20,000 years.

So the inductions that authorise us to expect future events grow weaker and weaker the further we look into the future, until eventually they have no significant force.

I have considered the probabilities of the sun's rising tomorrow, as derived from the real laws; i.e. from the laws of the causes on which the day-night uniformity depends. Let us now see what the situation would be if for us this uniformity was only an empirical law [see page 258], i.e. if we didn't know that the sun's light and the earth's rotation are the causes on which the periodical occurrence of daylight depends. We could still extend this empirical law to cases adjacent in time, but not across such a large distance of time as we can now ·with our knowledge of what the causes of the uniformity are·. Having evidence that the effects had been unaltered and precisely conjoined for five thousand years, we could infer that the unknown causes the conjunction depends on had existed—neither diminished nor counteracted—during that same period. So the same conclusions would follow as in the empirical-law case, except that in the latter we would only know that during five thousand years nothing had occurred to defeat perceptibly this particular effect; whereas when we know the causes (·i.e. in the real-law case·) we have the additional assurance that during that interval no such change has been noticeable in the causes themselves that could, if multiplied and continued kong enough, defeat the effect.

Our knowledge of the causes enables us to judge whether any known cause could counteract them; whereas if we didn't know them we couldn't be sure that there weren't causes actually in existence that could destroy them ·and thus break the day-night uniformity·. A bed-ridden savage who had never seen the Niagara Falls but who lived within hearing of them might imagine that the sound he heard would last forever; but if he knew it to be the effect of a rush of waters over a barrier of rock that is steadily wearing away, he would know that within a certain number of ages it will stop. Thus, the less we know about the causes on which the empirical law depends, the less sure we can be that it will continue to hold good; and the further we look into the future the more likely it is that some one of the causes that jointly give rise to the derivative uniformity will be destroyed or counteracted. The longer the time, the more chances there are of such an event—i.e. its not having occurred so far becomes less of a guarantee that it won't occur within the given time. If, then, it is only to cases that are temporally adjacent (or nearly so) to the ones we have actually observed that *any* derivative law (not a law of causation) can be extended with an assurance equivalent to certainty, this is even more true of a merely empirical law. Fortunately, for our practical purposes we hardly ever have occasion to extend them further than that.

In respect of *place*, it might seem that a merely empirical law couldn't be extended even to adjacent cases—i.e. that we couldn't be sure of its being true in any place where it hasn't been specially observed. The past duration of a cause guarantees its future existence unless something occurs to destroy it; but the existence of a cause in one place (or any number of places) doesn't guarantee its existence in any other place. because there's no uniformity in the collocations of primeval causes. Thus, when an empirical law is extended beyond the spatial limits within which it has been found true

by observation, the cases to which it is being extended must be ones that are presumed to be within the influence of the same individual agents. If we discover a new planet within the known bounds of the solar system. . . .we can conclude with great probability that it revolves on its axis. All the known planets do so; and this uniformity points to some common cause, some event earlier than the first recorded astronomical observations ; and if Laplace is right in thinking that what is involved here is not merely the same *kind of cause* but the same *individual cause* (such as an impulse given to all the bodies at once), that cause—having acted at the extreme edges of the solar system—is likely (unless defeated by some counteracting cause) to have acted at every intermediate point and probably also somewhat beyond the limits. Which makes it likely to have acted on the supposed newly-discovered planet.

So when effects that are always found conjoined can be traced with any probability to a single cause (not merely a single *kind of* cause), we can with the same probability extend the empirical law of their conjunction to all places within the extreme spatial boundaries within which the fact has been observed (though allowing for the possibility of counteracting causes in some part of the field). And we can do this even more confidently when the law is not merely empirical, i.e. when the phenomena that we find conjoined are effects of *known* causes from whose laws we can deduce the conjunction of their effects. In that case, ·we have two advantages·. **(i)** We can extend the derivative uniformity over a larger space, because we can go beyond boundaries of our observation of the fact itself and include the extreme boundaries of the known influence of its causes. We know that the succession of day and night holds true of all the bodies in the solar system except the sun itself; but we know this only because we know what the causes of the

day-night succession are. If we didn't, we couldn't extend the proposition beyond the orbits of the earth and moon. . . . **(ii)** We needn't make as much allowance for the chance of counteracting causes. I have shown that our •loss of

confidence because of the probability of counteracting causes should be proportional to our •ignorance of the causes on which the phenomena depend. . . .

Chapter 20. Analogy

§1. The word 'analogy', as the name of a mode of reasoning, is generally taken to name some kind of argument of an inductive nature but not amounting to a complete induction. But no word is used more loosely, or in a greater variety of senses, than 'analogy'. It sometimes stands for arguments that could be presented as examples of the most rigorous induction. Whately, for instance. . . ., defines 'analogy' in a way that fits the meaning that mathematicians originally gave it, namely: *resemblance of relations.* In this sense, when a country that has sent out colonies is termed the 'mother country', the expression is analogical, signifying that the colonies of a country relate to it in the way children relate to their parents. And if any inference is based on this resemblance of relations—e.g. that obedience or affection is due from colonies to the mother country—this is called reasoning by analogy. And if it is argued •that a nation is best governed by an assembly elected by the people, from the admitted premise •that other associations for a common purpose, such as joint-stock companies, are best managed by a committee chosen by the relevant parties, this is again an argument from analogy in the sense I am examining. The premise is not

> •that a nation is like a joint-stock company, or
> •that Parliament is like a board of directors, but that
> •Parliament relates to the nation in the way a board of directors relates to a joint-stock company.

. . . .Like other arguments from resemblance, an argument by analogy may •amount to nothing or •be a perfect and conclusive induction. The respect in which the two cases are alike may be the material one—the source of all the consequences that matter in the particular discussion. In the example last given, the resemblance is one of relation; the basis of the relation is *the management by a few persons of affairs in which they and others have an interest.* Someone may contend that this feature that is common to the two cases, along with the various consequences that follow from it, have the main share in determining all the effects that make up what we regard as good or bad administration. If he can establish this, his argument has the force of a rigorous induction; if he can't, he is said to have 'failed in proving the analogy' between the two cases—a turn of phrase implying that when the analogy is proved the argument based on it can't be resisted.

§2. But 'analogical evidence' is usually taken to cover any sort of resemblance (provided it doesn't amount to a complete induction), without highlighting resemblance of relations. Analogical reasoning, in this sense, comes down to this:

> •Two things resemble each other in one or more respects.
> •A certain proposition is true of one of them, Therefore
> •it is true of the other.

But that schema fits all reasoning from experience; nothing in it picks out analogy ·in particular· as distinct from induction ·in general·. In the strictest induction, equally with the faintest analogy, we argue that because A resembles B in one or more properties P_1, it also resembles it in a certain other property P_2. The difference is that in a complete induction it has been previously shown. . . .that there's an invariable conjunction between P_1 and P_2, whereas in so-called analogical reasoning no such conjunction has been claimed. There has been no opportunity to use the Method of Difference or even the Method of Agreement; we merely conclude (and this is all that the argument of analogy amounts to) that a fact m that is known to be true of A is more likely to be true of B •if B agrees with A in some of its properties (even though no connection is known to exist between m and those properties), than •if no resemblance at all could be found between B and anything else known to possess the attribute m. [The switch from 'fact' to 'attribute' is Mill's.]

This argument of course requires that the properties common to A and B are merely **not known to be** connected with m; they must not be properties **known not to be** connected with it. If we can. . . .show somehow that they have nothing to do with m, the argument of analogy is put out of court. The supposition the argument relies on is that m does depend on some property of A but we don't know which. . . . After setting aside all the properties of A that we know to have nothing to do with m, there remain several that we can't decide between; and B has one or more of these. We regard this as providing more or less strong grounds for concluding by analogy that B has the attribute m.

There can be no doubt that every such resemblance that can be pointed out between B and A provides some degree of probability, beyond what there would otherwise be, in favour of the conclusion drawn from it. If B resembled A in *all* its ultimate properties, its possessing the attribute m would be a certainty, not a probability; and every resemblance that can be shown to exist between A and B places the conclusion that much nearer to that point, i.e. to certainty. If A resembles B in having some ultimate property, there will be a resemblance between them in all the derivative properties flowing from that ultimate property, and m may be one of these. If A and B are alike •in some derivative property, there's reason to expect that they are also alike •in the ultimate property from which that one derives, and ·therefore· •in the other derivative properties that depend on that same ultimate property. Every resemblance that can be shown to exist provides ground for expecting indefinitely many other resemblances; so the particular resemblance we are looking for will be found more often among things known to be alike than among things between which we know of no resemblance.

I might infer that there are probably inhabitants in the moon, because there are inhabitants on the earth, in the sea, and in the air; and this is the evidence of analogy. The property of *having inhabitants* is here assumed to be not •ultimate but •a consequence of other properties;. . . .but we don't know which properties they are. Now, the moon resembles the earth in

- •being a solid, opaque, nearly spherical substance,
- •appearing to contain or to have contained active volcanoes;
- •receiving about as much heat and light from the sun as our earth does;
- •revolving on an axis;
- •being composed of materials that gravitate, and obeying all the laws resulting from that property.

If this were all that was known regarding the moon, these

various resemblances would make the thesis *The moon has inhabitants* more probable than it would otherwise be, though it would be useless to try to estimate *how much* more.

Along with the fact that

> every resemblance proved between B and A in any respect that isn't known to be irrelevant to the attribute *m* adds to the case for presuming that B has *m*,

it is clearly also true that

> every dissimilarity proved between B and A ·in any respect that isn't known to be irrelevant to the attribute *m*· creates a counter-probability of the same sort on the other side.

[Mill seems to mean by this '. . . adds to the case for presuming that B doesn't have *m*' rather than merely '. . . detracts from the case for presuming that B has *m*'.] It sometimes happens that different ultimate properties produce the same derivative property, but on the whole it is certain that things that differ in their ultimate properties will differ at least as much in the aggregate of their derivative properties, and that the unknown differences will bear some proportion to those that are known. So we will weigh •the known respects of alikeness between A and B against •their known respects of difference; and the answer to

> 'Do the analogies between A and B count for or against B's having the property *m*?'

will depend on which way the balance tilts. The moon is like the earth in the respects I have mentioned; but differs in

- •being smaller,
- •having a surface that is more uneven, and apparently volcanic throughout,
- •having, at least on the side facing the earth, no atmosphere sufficient to refract light,
- •having no clouds, and (it is therefore concluded) no water.

These differences, considered merely in themselves, might balance the resemblances, so that analogy wouldn't provide any presumption either way. But some of the features that the moon lacks are, on the earth, indispensable conditions of animal life; so we can conclude that if life does exist in the moon (or at all events on the nearer side), its causes must be totally different from those on which animal life depends here—a consequence of the moon's •differences from the earth not of its •similarities. Viewed in this light, all the resemblances between the moon and the earth count *against* the moon's being inhabited. Because life can't exist there in the way it does here, the greater the resemblance of the lunar world to the terrestrial one in other respects, the *less* reason we have to believe that the moon can contain life.

But the earth has a much closer resemblance to certain planets in our solar system—planets that have an atmosphere, clouds, consequently water (or some fluid analogous to it), and even give strong indications of snow in their polar regions; while temperature, though differing greatly on the average from ours, is in some parts of those planets, possibly not more extreme than in some habitable regions of our own. To balance these agreements, the known differences are chiefly in

- •the average light and heat,
- •speed of rotation,
- •density of material,
- •intensity of gravity,

and similar features of a secondary kind. With regard to these planets, therefore, the argument by analogy decidedly comes down in favour of their resembling the earth in its derivative properties such as that of having inhabitants; though when we consider how countlessly many their unknown properties are compared with the few that we know,

we can't attach any significant weight to any considerations of resemblance in which the known elements amount to so little compared with the unknown ones.

As well as competition between analogy and diversity, there can be a competition between conflicting analogies. The new case may be similar in some respects to cases in which *m* exists, and in other respects to cases in which it is known *not* to exist. Amber has some properties in common with vegetable products, others with mineral products. A painting of unknown origin may in some ways resemble known works of Titian while in others as strikingly resemble those of Raphael. A vase may bear some analogy to works of Grecian art and some to those of Etruscan or Egyptian. . . .

§3. So the value of an analogical argument inferring one resemblance from other resemblances without prior evidence of a connection between them depends on the extent of

(i) known resemblance

compared first with the extent of

(ii) known difference

and next with the extent of

(iii) the unexplored region of properties that might go either way.

It follows that where **(i)** the resemblance is very great, **(ii)** the known difference very small, and **(iii)** our knowledge of the subject-matter fairly extensive, the argument from analogy may be nearly as strong as the conclusion of a valid induction. If after much observation of B we find that it agrees with A in nine out of ten of its known properties, we can conclude with a probability of 9:1 that it will have any given derivative property of A. If we discover an unknown animal or plant closely resembling some known one in most of the properties we observe in it but differing in a few, we can reasonably expect to find in the unobserved remainder of its

properties a general agreement with those of the former, but also a difference whose size corresponds proportionately to the amount of observed diversity.

We learn from this that the conclusions derived from analogy aren't of much value unless the case toward which we are reasoning is *adjacent*—not near in place or time, but near in circumstances. In the case of effects whose causes are known imperfectly if at all, so that the observed order of their occurrence amounts only to an empirical law, it often happens that the conditions that have coexisted whenever the effect was observed have been very numerous. If a new case turns up in which these conditions don't all exist though by far greater part of them do, with only a few lacking, the inference that the effect will occur—despite this absence of *complete* resemblance to the cases where it has been observed—may be highly probable, although this is only an argument from analogy. Of course no competent inquirer into nature will rest satisfied with this, however high its probability is, if a complete induction can be had, but will consider the analogy as a mere guide-post indicating the direction in which more rigorous investigations should be carried out.

It's as guideposts that considerations of analogy have the highest scientific value. As I have remarked, analogical evidence doesn't itself support any very high degree of probability except when the resemblance is very close and extensive; but *any* analogy, however faint, can be of the utmost value in suggesting experiments or observations that may lead to more positive conclusions. When the agents and their effects are out of the reach of further observation and experiment—as in the speculations about the moon and planets—such slight probabilities aren't important, presenting merely an interesting theme for the pleasant exercise of imagination; but *any* suspicion that sets an able person to

work devising an experiment, or providing a reason for trying one experiment rather than another, may be of the greatest benefit to science.

For this reason although I can't accept as secure truths any of the scientific hypotheses that can't be eventually brought to the test of actual induction—e.g. the two theories of light, the emission-·of-particles· theory of the last century and the undulatory theory that currently predominates—I can't agree with those who regard such hypotheses as negligible. As is well said by David Hartley in his *Observations on Man*,. . . . 'any hypothesis that has enough plausibility to explain a considerable number of facts helps us to digest these facts in proper order, to bring new ones to light, and make decisive experiments for the sake of future inquirers'. If an hypothesis •explains known facts and •has led to the prediction of others that were previously unknown

and have since been verified by experience, the laws of the phenomenon x that is the subject of inquiry must be very like the laws of the class of phenomena to which the hypothesis assimilates x; and since an analogy that extends that far may well extend further, nothing is more likely to suggest experiments tending to throw light on the real properties of x than following out such an hypothesis. And this doesn't require that the hypothesis be mistaken for a scientific truth. On the contrary, *that* illusion blocks the progress of real knowledge by leading inquirers to restrict themselves to the particular hypothesis that is most in favour at the time, instead of •looking out for every class of phenomena whose laws are in any way like the laws of x, and •trying all such experiments as might tend to the discovery of further analogies pointing in the same direction.

Chapter 21. Evidence for the law of universal causation

§1. I have now completed my review of the logical processes by which the laws or uniformities in the •sequence of phenomena, and uniformities in their •coexistence that depend on their laws of sequence, are ascertained or tested. As I recognised at the outset, and have shown more clearly as the investigation progressed, all these logical operations are based on the law of causation.

The validity of all the inductive methods depends on the assumption that every event—i.e. the beginning of every phenomenon—must have some •cause, some •antecedent whose existence it invariably and unconditionally follows. In the Method of Agreement this is obvious; that method openly proceeds on the supposition that we have found the true

cause as soon as we have ruled out every other. Similarly with the Method of Difference. That method authorises us to infer a general law from two particular instances:

> •one in which A exists together with many other circumstances, and B follows; and
> •one in which A is absent while all the other circumstances remain the same, and B is prevented.

What *does* this prove? It proves that B in the particular instance **can't have had any cause other than A**; but to infer from this that **A was the cause**, or that A will on other occasions be followed by B, is legitimate only the assumption that B **must have had some cause**, I.e. that among its antecedents in any single instance in which it occurs there must be one

that has the capacity to produce it at other times. . . . There's no need for me to spend time proving that this holds for the other inductive methods as well. The universality of the law of causation is *assumed*—not proved—in all of them.

Is this assumption justified? You may want to object:

> No doubt *most* phenomena are connected as effects with some antecedent or cause, i.e. are never produced unless some assignable fact has preceded them; but the very fact that complicated inductive processes are sometimes needed shows that in some this regular order of succession isn't apparent to our unaided apprehension [= 'to our naked senses']. So if the processes that bring these cases into the same category as the rest require us to *assume* the universality of the very law that they don't at first sight appear to *exemplify*, isn't this a *petitio principii* [see Glossary]? Can we prove a proposition by an argument that takes it for granted? And if it isn't proved in that way, what is the evidence for it?

For this difficulty—which I have deliberately stated as strongly as possible—the school of metaphysicians who have long predominated in this country find a ready response. They say that the universality of causation is a truth that we can't help believing—the belief in it is an instinct, one of the laws of our believing faculty. As the proof of this they say (and it's all they have to say) that everyone *does* believe it; and they include it in their rather large catalogue of propositions that •can be logically argued against, and perhaps •can't be logically proved, but •are of higher authority than logic, and •are so deeply built into the human mind that even someone who denies them in theory shows by his habitual practice that his arguments make no impression upon himself.

. . . .I protest against offering the disposition—however strong and however general—of the human mind to believe that P as evidence that P is true in external nature. Belief is not proof, and doesn't dispense with the need for proof. I'm aware that to ask for evidence for a proposition that we're supposed to believe instinctively is to expose oneself to the charge of 'rejecting the authority of the human faculties'; and of course no-one can consistently do *that*. Why not? Because the human faculties are all that anyone has to judge by; and given that the meaning of the word 'evidence' is supposed to be 'something that when laid before the mind induces it to believe', to demand evidence when the belief is ensured by the mind's own laws is appealing to the intellect against the intellect. That's what they say. But I think this is a misunderstanding of the nature of evidence. What we mean by 'evidence' is not 'anything and everything that produces belief'! Many things generate belief besides evidence. A mere strong association of ideas often causes a belief so intense as to be unshakable by experience or argument. Evidence isn't what the mind *does* or *must* yield to, but what it *ought to* yield to because that will keep the mind's belief conformable to fact. There is no appeal ·to any higher court· from the human faculties generally, but there is an appeal from one human faculty to another; from the judging faculty to those that attend to facts—i.e. the faculties of sense and consciousness. The legitimacy of this appeal is admitted whenever it is allowed that our judgments ought to fit the facts. To say that *belief suffices for its own justification* is making opinion the test of opinion; it is denying the existence of any outward standard that an opinion has to meet to count as true. We call one way of forming opinions 'right' and another 'wrong' because one does and the other doesn't tend to make the opinion agree with the facts—to make people believe what really is, and expect what really will be.

A mere disposition to believe that P, even if is instinctive, is no guarantee that P is true. If indeed the belief ever did amount to an irresistible necessity, there would be no use calling it into question because there would be no possibility of altering it. But even then it wouldn't follow that the belief was *true*; it would only follow that mankind were under a permanent necessity of believing something that might be false. . . . But in fact there is no such permanent necessity. There is no proposition of which it can be said that every human mind must eternally and irrevocably believe it. Many of the propositions of which this is most confidently stated have in fact been disbelieved by many people. There are countless things of which it has been supposed that nobody could possibly help believing them, but no two generations would have the same list of them! One age or nation unquestioningly believes what to another seems incredible and inconceivable; one individual has no vestige of a belief that someone else thinks is absolutely built into human nature. None of these supposed instinctive beliefs is really inevitable. Everyone has the powers to develop habits of thought that make him independent of them; especially the habit of philosophical analysis, which is the best way to enable the mind to •command the laws of the merely passive part of its own nature, rather than •being commanded by them. This habit of thought shows us that things aren't necessarily connected in fact because their ideas are connected in our minds, and is thus is able to loosen countless associations that reign despotically over the undisciplined or early-prejudiced mind—including associations that the school of thought I'm discussing thinks are born with us and instinctive. I'm convinced that anyone who •is accustomed to abstraction and analysis, •is willing to exert his faculties for this purpose, and •frees his imagination to make room for unfamiliar notions, will have no difficulty in conceiving that

in some part. . . .of the universe events succeed one another at random without any fixed law. Nothing in our experience or in the nature of our minds constitutes *any* reason for believing that this is nowhere the case.

If the present order of the universe were brought to an end (which we're perfectly able to imagine happening), starting off a chaos with •no fixed succession of events and •no clues to the future in the past, then if a human being miraculously survived to witness this change, he would surely stop believing in uniformity because there wouldn't *be* any uniformity. If this is right, then the belief in uniformity either isn't an instinct, or is one that can—like all other instincts—be conquered by acquired knowledge.

But there's no need to speculate on what •might be, when we have certain knowledge of what •has been. It's simply not true that mankind have always believed that all the successions of events were uniform and according to fixed laws. The Greek philosophers, even including Aristotle, recognised Chance and Spontaneity as among the agents in nature, so that for them there was no guarantee that the past had been similar to itself [presumably meaning 'that each part of the past had resembled all its other parts'], or that the future would resemble the past. Even now at least half of the philosophical world, including the metaphysicians who most strenuously maintain that the belief in uniformity is instinctive, regard *volitions* as an exception to the uniformity and not governed by a fixed law.

·Start of a long footnote·

Baden Powell's *Essay on the Inductive Philosophy* contains an excellent passage which I'm glad to be able to quote, in confirmation of both the history and the doctrine that I have presented. Speaking of the 'conviction of the universal and permanent uniformity of nature', Powell writes: 'This

idea isn't widely accepted and doesn't grow in us naturally. Everyone on the basis of his experience comes to embrace a certain view of this kind—but it is limited to the thesis that what is going on around him at present, in his own narrow sphere of observation, will go on in the same way in future. The peasant believes that the sun that rose today will rise again tomorrow; that the seed put into the ground will be followed by the harvest this year as it was last year, and so on; but he has no notion of inferences like that on topics beyond his immediate observation. . . . And it's not only the *most* ignorant who limit the truth in this way. There's a general propensity to •believe that apart from specially ascertained laws of nature everything beyond common experience is left at the mercy of chance or fate or arbitrary intervention; and even to object to any attempted explanation by physical causes of an apparently unaccountable phenomenon.

'So we have this ·limited· idea of the uniformity of nature; but how are we to generalise it? That task isn't obvious, natural, or intuitive—far from it! It is utterly beyond the reach of most people. The fully universal notion of the uniformity of nature is a mark of the philosopher: it's clearly the result of philosophical cultivation and training, and absolutely *not* the spontaneous offspring of any primary principle [see Glossary] naturally inherent in the mind, as some seem to believe. It is not a mere vague opinion taken up without examination as a common assumption to which we are always accustomed; on the contrary, all common prejudices and associations are against it. It is pre-eminently an *acquired idea*. It is not attained without deep study and reflection. The best informed philosopher is the man who most firmly believes it, even in opposition to received notions; its acceptance depends on the breadth and depth of his inductive studies.'

·END OF FOOTNOTE·

§2. As I remarked on page 152, our belief in the universality throughout nature of the law of cause and effect is itself an instance of induction; and by no means one of the earliest that any of us—let alone mankind in general—can have made. We arrive at this universal law by generalisation from many less general laws. We would never have had the notion of *causation* (in the philosophical meaning of the word) as a condition of all phenomena unless many cases of causation— i.e. many partial uniformities of sequence—had previously become familiar. The more obvious of the particular uniformities suggest and give evidence for the general uniformity, and once the general uniformity is established it enables us to prove the remainder of the particular uniformities of which it is made up. As, however, all rigorous processes of induction presuppose the general uniformity, our knowledge of the particular uniformities from which *it* was first inferred was, of course, derived not from •rigorous induction but from •the loose and uncertain procedure of *inductio per enumerationem simplicem* [see Glossary]; and because the law of universal causation is based on results obtained in that way it can't itself rest on any better foundation. [Throughout that paragraph, 'particular' should have been 'less general'.]

So it seems that induction *per enumerationem simplicem*, far from being an illicit logical process, is actually the only kind of induction possible; because the more elaborate process ·of sophisticated kinds of induction· depends for its validity on a law that is itself obtained in that inartificial way. [By 'inartificial' Mill means that induction *per enumerationem simplicem* doesn't require skill, isn't governed by complex rules, is (as its name indicates) simple.] Then isn't there an inconsistency in contrasting the looseness of one method with the rigidity of another, when the rigid method is based on the looser one?

This inconsistency is only apparent. Of course if induction by simple enumeration were an *invalid* process, no

process based on it could be valid—any more than we could rely on telescopes if we couldn't trust our eyes. But it isn't invalid; it's merely fallible; and there are different *degrees* of fallibility. If we can substitute for the *more* fallible forms of a process an operation based on the same process in a *less* fallible form, that will be a very material improvement. And that's what scientific induction does.

A procedure for drawing conclusions from experience must be regarded as untrustworthy when subsequent experience refuses to confirm it. By this criterion, induction by simple enumeration—i.e. generalisation of an observed fact from the mere absence of any known instance to the contrary—is in general a precarious and unsafe basis for confidence, because we're constantly finding such generalisations to be false. Still, it provides some assurance. . . .for the ordinary guidance of conduct. It would be absurd to say that the generalisations arrived at by mankind at the outset of their experience—e.g. food nourishes, fire burns, water drowns—are not fit to be relied on.[1] There's a *scale* of trustworthiness in the results of the original unscientific induction; and as I pointed out in chapter 4 the rules for the improvement of the process depend on the differences marked by this scale. The improvement consists in correcting one of these inartificial generalisations by means of another. This (I repeat myself here) is all that art can do. To test a generalisation by showing that it either follows from or conflicts

with some stronger induction, some generalisation resting on a broader foundation of experience, is the beginning and end of the logic of induction.

§3. For any generalisation G reached by the method of simple enumeration, the broader (i.e. more general) G is, the less precarious it is. The process is misleading and inadequate exactly in proportion as the subject-matter of the observation is special and limited in extent. As the sphere widens, this unscientific method becomes less and less liable to mislead; and the most universal class of truths—including the law of causation and the principles of number and of geometry—are satisfactorily proved by that method and can't be proved in any other way.

As applied to the uniformities that depend on causation, that remark follows obviously from the principles laid down in the preceding chapters. When a fact [see Glossary] has been observed several times to be true and never to be false, if we at once affirm it as a universal truth or **law of nature**—without testing it by any of the four methods of induction or deducing it from other known laws—we'll usually err grossly; but we're perfectly justified in affirming it as an **empirical law**, true within certain limits of time, place, and circumstance, provided the number of instances is greater than can plausibly be attributed to chance. Why not extend it beyond those limits? Because its holding true within them may be

---

[1] These early generalisations didn't presuppose causation as scientific inductions do. What they did presuppose was uniformity in physical facts. But the observers were as ready to presume uniformity in the •coexistence of facts as in the •sequences of facts. On the other hand, they never thought of assuming that this uniformity was a principle pervading all nature: their generalisations didn't imply that there was uniformity in everything, but only that as much uniformity as existed within their observation existed also beyond it. The induction *fire burns* doesn't require for its validity that all nature should observe uniform laws, but only that there should be uniformity in. . . .the effects of fire on the senses and on combustible substances. And uniformity to this extent was not assumed, anterior to the experience, but proved by the experience. The same observed instances that proved the narrower truth proved the *corresponding* wider one. It's because people lost sight of this fact, and thought that the law of causation in its full extent is necessarily presupposed in the very earliest generalisations, that they have been led to believe that the law of causation is known *a priori* and is not itself a conclusion from experience.

an upshot of collocations that can't be concluded to
exist in one place because they exist in another;
or it may be
dependent on the accidental absence of counteracting
agencies, which might be brought into play by any
variation of time or the smallest change of circum-
stances.
With that in mind, now consider the case of a generalisa-
tion whose subject-matter is so widely diffused that *every*
time, place, and combination of circumstances provides an
example either of its truth or of its falsity, and suppose that
it is never found to be otherwise than true. If the truth of
this generalisation depends on collocations, they must be
ones that exist at all times and places; and if it could be
frustrated by any counteracting agencies, they must be ones
that never actually occur! So it's an empirical law that is
coextensive [see Glossary] with all human experience; at which
point the distinction between empirical laws and laws of
nature vanishes, and the proposition takes its place among
the most firmly established as well as broadest truths that
science can discover.

Of all the generalisations that experience supports con-
cerning the sequences and coexistences of phenomena, the
most extensive in its subject-matter is the law of causation.
It stands at the top of all observed uniformities—top in
•universality, and therefore (if what I have been saying is
right) top also in •certainty. . . . We're justified in considering
this fundamental law, though it was obtained by induction
from particular laws of causation, as actually *more* certain
than any of those from which it was drawn. It adds to
them as much proof as it receives from them. Even the
best established laws of causation are probably sometimes
counteracted and thus suffer apparent exceptions; and this
would have shaken mankind's confidence in the universality

of those laws if inductive processes based on the universal
law hadn't enabled us to attribute those exceptions to the
agency of counteracting causes, thereby reconciling them
with the law that they apparently conflict with. . . . When it
comes to the ·universal· law of causation, on the other hand,
we don't know of any exceptions. And the exceptions that
limit or apparently invalidate the special laws. . . .actually
confirm the universal law: in all cases that are sufficiently
open to our observation we can trace the difference of result—
·the apparent exception·—either to •the absence of a cause
that had been present in ordinary cases, or to •the presence
of one that had been absent; ·and this tracing involves the
use of the universal law of causation·.

Because •the law of cause and effect is certain, it can
pass its certainty on to all other inductive propositions that
can be deduced from it; and the narrower inductions can
be seen as getting their ultimate sanction from •that law,
because every one of them x gains in certainty when we
connect it with that larger induction and show that x can't
be denied, consistently with the law that everything that
begins to exist has a cause. So we're justified in the seeming
inconsistency of
•holding induction by simple enumeration to be good
for proving this general truth, which is the foundation
of scientific induction, and yet
•refusing to rely on it for any of the narrower
inductions.
I fully admit that if we didn't know the law of causation we
could still generalise the more obvious cases of uniformity
in phenomena—always a bit precariously and sometimes
extremely so—and this would give us a certain measure
of probability. But there would be no need for us to esti-
mate *how* probable such a result was, because ·we know
in advance that· it never could amount to the degree of

assurance that a proposition acquires when we show—by applying the four methods—that the supposition of its falsity is inconsistent with the law of causation. So we are •theoretically entitled and •practically required to disregard the probabilities derived from the early rough method of generalising, and not to consider a minor generalisation as proved unless the law of causation confirms it, or as probable unless we can reasonably expect it to be so confirmed.

§4. To assert both of these:

　　•Our inductive processes assume the law of causation.
　　•The law of causation is itself a case of induction.

is paradoxical only on the old theory of reasoning, according to which the universal truth (i.e. major premise) in a ratiocination is the **real** proof of the particular truths that are **ostensibly** inferred from it. According to the doctrine I presented in II.3.4 the major premise is not the proof of the conclusion; it is itself proved, along with the conclusion, from the same evidence. 'All men are mortal' is not the proof that Lord Palmerston is mortal; but our past experience of mortality authorises us to infer both the general truth and the particular fact, and both with exactly the same degree of assurance. Lord Palmerston's mortality is inferred not •from the mortality of all men but •from the experience that proves the mortality of all men; and it's a correct inference from experience if that general truth is correct. This relation between our general beliefs and their particular applications holds equally true in the more comprehensive case that we're now discussing. Any new fact of causation inferred by induction is rightly inferred if it's open to no objection that isn't also an objection to the general truth that every event has a cause. The utmost certainty that can be given to a conclusion arrived at by inference stops at this point. When we have ascertained that the particular conclusion

must stand or fall with the general uniformity of the laws of nature—is open to no doubt except doubt about whether every event has a cause—we have done all that can be done for it. The strongest assurance we can obtain of any theory respecting the cause of a given phenomenon is that the phenomenon has either that cause or none.

At a very early stage in our study of nature it might have been admissible to suppose 'It has no cause'. But at the stage that mankind have now reached we can see that the generalisation that produces the law of universal causation has grown into a stronger and better induction, one deserving of greater reliance, than any of the subordinate generalisations. I think indeed that we can go a step further than this and regard the conclusion of that great induction as not merely •more certain than anything else but for all practical purposes •completely certain.

As I see it, there are two considerations that *now* give to the proof of the law of uniformity of succession this character of completeness and conclusiveness. **(1)** We now know it directly to be true of the great majority of phenomena; there are none of which we know it not to be true; the most that can be said about that is that there are some phenomena that we can't—positively and from direct evidence—affirm it to be true of; but as phenomena become better known to us they are constantly passing from the 'not known to be uncaused' class into the 'known not to be uncaused' class; and for any phenomenon x whose cause we haven't yet found, the absence of direct proof is accounted for by

　　•the rarity or the obscurity of x,
　　•our deficient means of observing x, or
　　•the logical difficulties arising from the complication of
　　　the circumstances in which x occurs;

so that even if x depends as rigidly on conditions as does any other phenomenon it wasn't likely that we would know more

about those conditions than we do. **(2)** There are phenomena the production and changes of which escape all our attempts to explain them in terms of any known law; but in every such case, the phenomenon or the objects involved in it are found in some instances to obey the known laws of nature. The wind, for example, is the type [see Glossary] of uncertainty and caprice, yet we sometimes find it obeying—with as much constancy as any phenomenon in nature—the law of the tendency of fluids to distribute themselves so as to equalise the pressure on every side of each of their particles; as in the case of the trade-winds and the monsoons. Lightning might once have been supposed to obey no laws; but now that we know it to be identical with electricity, we know that lightning in some of its manifestations is implicitly obedient to the action of fixed causes [see page 257]. I don't think that there is *any* object or event in all our experience of nature—within the solar system, at least—that hasn't either been •discovered by direct observation to follow laws of its own or •proved to be closely similar to objects and events which, in more familiar manifestations or on a more limited scale, follow strict laws. Our inability to trace the same laws on a larger scale and in the more specialised instances is explained by the number and complication of the modifying causes or by their inaccessibility to observation.

So the progress of experience has blown away the doubt there used to be about the universality of the law of causation, back when there were phenomena that seemed to be *sui generis* [see Glossary], not subject to the same laws as any other class of phenomena, and not yet found to have special laws of their own. But this great generalisation, back then, could reasonably have been—as in fact it was—acted on as highly probable before there were sufficient grounds for accepting it as a certainty. In this territory, as in everything, we don't need and can't have the absolute. We must hold

even our strongest convictions with an opening left in our minds for the reception of facts that contradict them. Until we have taken this precaution, we aren't entitled to act on our convictions with complete confidence just because no such contradiction appears. If something x has been found true in countless instances, and never found to be false after due examination, we are safe in acting on x as universally true, doing this *provisionally* until an undoubted exception appears; provided that this is a case where a real exception could scarcely have escaped our notice. When every phenomenon that we ever knew well enough to be able to answer the question had a cause on which it was invariably consequent, it was more rational to suppose that our inability to assign the causes of other phenomena arose from our ignorance than to think that some phenomena were uncaused, they being precisely the ones that we hadn't yet had sufficient opportunity to study.

Notice, though, that the reasons for this reliance don't hold in circumstances that we don't know and can't possibly have experience of. It would be folly to affirm confidently that this general law prevails in distant parts of the stellar regions, where the phenomena may be entirely unlike any we are acquainted with. . . . The uniformity in the succession of events, otherwise called 'the law of causation', must be accepted as a law not of the universe but only of the part of it that lies within the range of our means of sure observation, with some extension to adjacent cases. To extend it further is to make a supposition for which we have no evidence and can't reasonably try to find any.

·Long footnote ending chapter 21·

A rising thinker of the new generation in France, **Hippolyte Taine**, has given in the *Revue des Deux Mondes* the most masterly analysis, at least from one point of view, that has

yet been made of this work ·of mine·. I mention him here because he assigns to the law of causation and to some other universal laws a certainty beyond the bounds of human experience, which I haven't been able to accord to them.

He does this on the strength of our faculty of *abstraction*, which he seems to see as an independent source of evidence, not indeed as
> •disclosing truths that aren't contained in our experience, but as
> •assuring us, as experience can't, that truths we get from experience are universally true.

Taine seems to think that abstraction enables us not merely •to analyse the part of nature that we see, and exhibit separately the elements that pervade it, but also •to pick out those that are elements of the system of nature considered as a whole, not merely incidents belonging to our limited terrestrial experience. I'm not sure that I fully enter into Taine's meaning; but I confess that I don't see how any mere abstract conception extracted by our minds from our experience can be evidence of an objective fact in universal nature, evidence not provided by the experience itself. . . .

In an able article in the *Dublin Review* **William George Ward** contends that the uniformity of nature can't be proved from experience, but only from 'transcendental considerations', and that physical science would have no basis if such transcendental proof were impossible.

When physical science is said to depend on the assumption that the course of nature is invariable, all that is meant is that the conclusions of physical science aren't known as *absolute* truths: their truth is *conditional* on the uniformity of the course of nature; and all that the most conclusive observations and experiments can prove is that the result arrived at will be true if the present laws of nature are valid, and for as long as they are valid. But this is all the assurance

we require for the guidance of our conduct. Ward doesn't think that his transcendental proofs make our assurance practically greater, for he believes (as a Catholic) that the course of nature has been and frequently still *is* suspended by supernatural intervention.

All I needed to prove was this *conditional* conclusiveness of the evidence of experience, which is sufficient for the purposes of life; but I have ·gone further, and· given reasons for thinking that the uniformity ·of nature·, as itself a part of experience, is sufficiently proved to justify *undoubting* reliance on it. Ward challenges this, for three reasons.

(a) Even if it's true that there has never yet been a well-authenticated case of a breach in the uniformity of nature, 'the number of natural agencies constantly at work is incalculably large; and the *observed* cases of uniformity in their action must be immeasurably fewer than one-thousandth of the whole. Scientific men (let's assume for the moment) have discovered that in a certain proportion of instances— immeasurably fewer than one-thousandth of the whole—a certain fact has prevailed, namely the fact of uniformity; and they haven't found a single instance where that fact doesn't prevail. Are they justified in inferring from these premises that the fact is universal? Surely the question answers itself!' [Ward rubs this in with a 'very grotesque' example; we don't need it to follow Mill's reply.]

Ward's argument doesn't touch mine as it stands in the text. My argument is based on the fact
> •that the uniformity of the course of nature as a whole is constituted by the uniform sequences of special effects from special natural agencies;
> •that the number of these natural agencies in the part of the universe we know is not 'incalculable', not even extremely great;

•that we now have reason to think that most of them have been made sufficiently open to observation—if not separately, then at least in some of the combinations they enter into—for us to ascertain some of their fixed laws; and

•that this amount of experience justifies us in being as sure that the course of nature is uniform throughout as we used to be about the uniformity of sequence among the phenomena best known to us.

This view of the subject, if correct, destroys the force of Ward's first argument.

(b) Next, he argues that many or most persons, both scientific and unscientific, believe that there are well authenticated cases of breaks in the uniformity of nature, namely miracles. This also fails to touch what I have said in the text. The only uniformity in the events of nature that I accept is the law of causation; and (as I shall explain in chapter 25) a miracle is not an exception to that law. In every case of an alleged miracle, a new antecedent is said to exist—a counteracting cause, namely the volition of a supernatural being. Thus, for anyone for whom beings with superhuman power over nature are a *vera causa* [see Glossary], a miracle is a case of the law of universal causation, not a deviation from it.

(c) Ward's last argument (which he says is the strongest) is the familiar one of Reid, Stewart, and their followers—that whatever knowledge experience gives us of the past and present, it gives us none of the future. I confess that I can't see any force in this argument. How does a future fact differ from a present or a past fact, except in their momentary relation to the human beings at present in existence? The answer made by Priestley in his *Examination*

of Reid seems to me sufficient—namely that though we have had no experience of what *is* future, we have had abundant experience of what *was* future. The 'leap in the dark' (as Bain calls it) from the past to the future is *exactly* as much in the dark as the leap from a past that we have personally observed to a past that we haven't. I agree with Bain's opinion that the resemblance of what we haven't experienced to what we have is by a law of our nature presumed through the mere energy of the idea, before experience has proved it. But this •psychological truth is not, as Ward seems to think in his criticism of Bain, inconsistent with the •logical truth that experience does prove it. The proof comes after the presumption, and consists in its invariable verification by experience when the experience arrives. . . .

In his *Examination of Mr J. S. Mill's Philosophy*, **James McCosh** maintains that •the uniformity of the course of nature is a different thing from •the law of causation; and while he allows that the former is only proved by a long continuance of experience, and that it is not inconceivable or incredible that there may be worlds where it doesn't prevail, he thinks that the law of causation is known intuitively. But the only uniformity in the events of nature is what arises from the law of causation; so as long as there remained any doubt that the course of nature was uniform throughout, at least when not modified by the intervention of a new (supernatural) cause, a doubt was necessarily implied not of the •reality of causation but of its •universality. If the uniformity of the course of nature has any exceptions—if any events succeed one another without fixed laws—to that extent the law of causation fails, and there are events that don't depend on causes.

·END OF FOOTNOTE·

Chapter 22. Uniformities of coexistence that don't depend on causation

§1. Phenomena occur either successively or simultaneously; so the uniformities in their occurrence are either uniformities of succession or uniformities of coexistence. Uniformities of succession are all covered by the law of causation and its consequences. Every phenomenon has a cause, which it invariably follows; and this gives rise to other invariable sequences among the successive stages of the same effect, as well as between the effects resulting from causes that invariably succeed one another.

In the same way a great variety of uniformities of coexistence also take their rise. Coordinate effects of a single cause naturally coexist with one another. High water at any point on the earth's surface is uniformly simultaneous with high water at the diametrically opposite point, resulting from the directions in which the combined attractions of the sun and moon act on the oceans. An eclipse of the sun to us is invariably coexistent with an eclipse of the earth to a spectator on the moon, and their coexistence can also be deduced from the laws of their production.

So the question naturally arises: Can *all* the uniformities of coexistence among phenomena be explained in this way? Well, between phenomena that are themselves effects any coexistences must depend on the causes of those phenomena. If they are effects—immediately or remotely—of a single cause, the only way they can coexist is by virtue of some laws or properties of that cause; if they are effects of different causes, they must coexist because their causes coexist; and any uniformity of coexistence among these effects proves that those particular causes, within the limits of our observation, have uniformly been coexistent.

§2. But one class of coexistences can't depend on causation, namely coexistences between things' *ultimate* properties— the properties that cause all phenomena without themselves being caused by any phenomenon. (If they are caused, it is not by any phenomenon but by the origin of all things.) Yet among these ultimate properties there are coexistences, and indeed uniformities of coexistence. General propositions can be formed saying that whenever certain properties are found certain others are found along with them. We perceive water (for example) and recognise it to be water by certain of its properties P_1. Having recognised it, we can ascribe to it countless other properties P_2; and we couldn't do that unless it were a general truth—a law or uniformity in nature—that the P_1 properties always have the P_2 properties conjoined with them.

In I.7.4 I explained in some detail what is meant by the Kinds of objects—classes that differ from one another not by a limited and definite set of distinctions but by an indefinite and unknown set of them. I now add that **every proposition asserting something about a Kind affirms a uniformity of coexistence**. All we know about any Kind is its properties; so the Kind, to us, *is* the set of properties by which it is distinguished from every other Kind.[1] In affirming

---

[1]   A Kind may be identified by some one remarkable property: but usually it takes several, each separate property being shared with other Kinds. The diamond's colour and brightness are shared with the paste from which false diamonds are made; its octohedral form is shared with alum and magnetic iron ore; but the colour and brightness and form together identify its Kind—i.e. are a sign to us that it is combustible, that when burned it produces carbonic acid, that it can't be cut with any known substance, along with many other ascertained properties and the fact that there are indefinitely many still unascertained.

anything of a Kind, therefore, we're affirming something to be uniformly coexistent with the properties by which the kind is recognised; and that's *all* that the assertion means.

All the properties of Kinds, then, can be counted amongst the natural uniformities of coexistence. They aren't *all* independent of causation—only some of them. Some are ultimate properties, others derivative; for some no cause can be assigned, but others clearly depend on causes. Pure oxygen gas is a Kind, and one of its most straightforward properties is its gaseous form; but this property has for its cause the presence of latent heat; and if that heat were taken away (as has been done from many gases in Faraday's experiments), the oxygen would lose its gaseous form along with many other properties that depend on—i.e. are caused by—that property.

Now for chemical compounds, which can be seen as resulting from the juxtaposition of substances that are different in Kind from themselves: there's good reason to presume that a compound's specific properties are effects of some of the properties of the elements ·that make it up·, though little progress has been made in tracing any invariable relation between any compound's properties and the properties of its elements. There's even more reason to make such a presumption when the object itself is not a primeval agent—·i.e. not an uncaused cause·—but an effect that depends on a cause or causes for its very existence. (Organisms are examples of this.) Thus, the only Kinds with properties that can confidently be regarded as ultimate are the ones that chemists call 'simple substances' or 'elementary natural agents'; and the ultimate properties of these are probably much more numerous than we recognise, because every successful resolution of the properties of a chemical compound into simpler laws ·governing its elements· leads to the recognition of properties in the elements distinct from any previously known:

- The resolution of the laws of the motions of heavenly bodies established •the previously unknown ultimate property of mutual attraction between all bodies;
- the ongoing resolution of the laws of crystallisation, of chemical composition, of electricity, of magnetism etc. points to •various polarities that are ultimately inherent in the particles bodies are composed of;
- the resolution into more general laws of the uniformities in the proportions in which substances combine with one another led to the discovery of •the comparative atomic weights of different kinds of bodies;

and so forth. So the situation is this: every resolution of a complex uniformity into simpler and more elementary laws has an *apparent* tendency to reduce the number of the ultimate properties, and really *does* remove many properties from the list; but the result of this simplifying process is to trace an ever greater variety of different effects back to a single cause, and the further we go in this direction the more properties we are forced to recognise in a single object; and the coexistences of those properties must accordingly be ranked among the ultimate generalities of nature.

§3. So there are only two kinds of propositions that assert uniformity of coexistence between properties. If the properties depend on causes, the proposition that says they are coexistent is a derivative law of coexistence between effects, and it has the status of an empirical law until it's resolved into the laws of causation on which it depends. . . . If the properties don't depend on causes—i.e. are ultimate properties—then if it's true that they invariably coexist they must all be ultimate properties of a single Kind; and it's only of these that the coexistences can be classified as a special sort of laws of nature.

When we say that all crows are black, we assert a uniformity of coexistence. We assert that the property of blackness invariably coexists with the properties that define the class crow in common language or in our chosen scientific classification. Now, supposing blackness to be an ultimate property of black objects—i.e. supposing that it isn't a result of causation, isn't connected with antecedent phenomena by any law—then if all crows are black this must be an ultimate property of the kind *crow* or of some kind that includes it. If on the contrary blackness is an effect depending on causes, the proposition 'All crows are black' is clearly an empirical law; and what I have already said about empirical laws applies here too. [Mill ran 'All crows are black' in harness with 'All negroes have woolly hair', making exactly the same points regarding each.]

We have seen that in the case of all compounds—i.e. of everything except nature's elementary substances and primary powers—the presumption is that the properties do really depend on causes; and it's *never* possible to be certain that they don't. So we wouldn't be safe in claiming for any generalisation about the coexistence of properties a degree of certainty that wouldn't be justified if the properties turn out to be the result of causes. A generalisation about coexistence, i.e. about the properties of Kinds, *may* be an ultimate truth, but it may be merely a derivative one; and if the latter, it is one of those derivative laws that •aren't laws of causation and •haven't been resolved into the laws of causation on which they depend; so it can't be more evident than an empirical law can.

§4. We have found that a system of rigorous scientific induction can be applied to the uniformities in the •succession of phenomena; but nothing like that can be applied to the ultimate uniformities of •coexistence. The basis for such a

system is lacking: there's no general axiom relating to the uniformities of coexistence in the way the law of causation relates to the uniformities of succession. The Methods of Induction that can be used to discover causes and effects are based on the principle that everything x that has a beginning must have a cause; that among the circumstances that actually existed at the time x began there is one combination on which x is unconditionally consequent, and on the repetition of which x would certainly start again. But in an inquiry whether some Kind (such as *crow*) universally possesses a certain property (such as *blackness*) there is no room for any assumption analogous to this. We have no previous certainty that the property must have something that constantly coexists with it, i.e. must have an invariable coexistent in the same way that an event must have an invariable antecedent. When we feel pain, we must be in some state under which, if exactly repeated, we would always feel pain. But when we're conscious of blackness, it doesn't follow that there is something else present of which blackness is a constant accompaniment. So there is no room for elimination, no method of Agreement or Difference or Concomitant Variations. . . . We can't conclude that the blackness we see in crows must be an invariable property of crows merely because there's nothing else present of which it can be an invariable property! So we inquire into the truth of a proposition like 'All crows are black' under the same disadvantage as if, in our inquiries into the cause of x, we had to allow for the possibility that x occurred without any cause.

Overlooking this grand distinction was, it seems to me, the central error in Bacon's view of inductive philosophy. He thought that the principle of elimination—that great logical instrument that he had the immense merit of first bringing into general use—could be applied (in the same

sense and in as unqualified a manner) to the investigation of the coexistences of phenomena as to their successions. He seems to have thought that just as every event has a cause or invariable antecedent, so also every property of an object has an invariable coexistent, which he called its 'form'. And his chosen examples to illustrate his method were inquiries into such forms: taking up objects that agree in some one general property—hardness or softness, dryness or moistness, heat or cold—he asked *what else* they have in common. Such inquiries couldn't lead anywhere. The objects seldom have any such circumstances in common. They usually agree in the one property inquired into, and in nothing else. A great proportion of the properties that seem to us to be the likeliest to be really ultimate, seem to be inherently properties of many different Kinds of things that don't have anything else in common. As for properties that we can give some account of because they are effects of causes, they have generally nothing to do with the ultimate resemblances or diversities in the objects themselves, but depend on some outward circumstances under the influence of which *any* object can manifest those properties. Certainly the case with Bacon's favourite subjects of scientific inquiry—heat and coldness—as well as with hardness and softness, solidity and fluidity, and many other conspicuous qualities.

In the absence of any universal law of coexistence like the universal law of causation that regulates sequence, we're thrown back upon the unscientific induction of the ancients—*induction by simple enumeration where there are no counter-examples* [Mill gives that phrase in Latin]. The reason we have for believing that all crows are black is simply that we have seen and heard of many black crows and never one of any other colour. We now face two questions: How far can this evidence reach? How are we to measure its strength in any given case?

§5. It sometimes happens that a mere change in the wording of a question, without changing its meaning, is a long step toward finding the answer. Our present topic is a case of this, I think. Here are two formulations for exactly the same thing:

  • the degree of certainty of a generalisation for which our only evidence is that no counter-examples to it has yet been observed;
  • the degree of improbability that a counter-example, if there were one, could have remained unobserved until now.

The reason for believing that all crows are black is measured by the improbability that crows of any other colour should have existed to the present time without our being aware of it. Let us state the question in this second way and consider: What is implied in the supposition that there may be crows that aren't black? Under what conditions are we justified in regarding this as incredible?

If there really are non-black crows, one of two things must be the case. **(i)** The blackness in all crows hitherto observed is (as it were) an accident, not connected with any distinction of Kind. **(ii)** Blackness is a property of Kind, and non-black crows are a new Kind that we have overlooked although they fit the general description by which we have always recognised crows. We might prove **(i)** to be true if we casually discovered a white crow among black ones, or if we found that black crows sometimes turn white. And **(ii)** would be shown to be the fact if in Australia or Central Africa a species or a race of white or gray crows were found to exist.

§6. Supposition **(i)** implies that the colour is an effect of causation. If blackness in the crows in which it has been observed isn't a property of Kind—so that an object can have it or lack it without any difference in its other properties—

then it isn't an ultimate fact in the individuals themselves but certainly depends on a cause. There are many properties that vary from individual to individual of the same Kind. . . . Some flowers can be either white or red without differing in any other respect. But these properties aren't ultimate; they depend on causes. So far as a thing's properties belong to its own nature and don't arise from some external cause, they are always the same in the same Kind. Take, for instance, all •simple substances and •elementary powers, which are the only things of which we're certain that at least some of their properties are ultimate. Colour is generally regarded as the most variable of all properties; but we don't find that sulphur is sometimes yellow and sometimes white, or that it varies in colour at all except to the extent that colour is an effect of some external cause—e.g. the sort of light thrown upon it, or the mechanical arrangement of the particles after fusion, etc. We don't find that iron is sometimes fluid and sometimes solid at the same temperature; gold sometimes malleable and sometimes brittle; that hydrogen sometimes combines with oxygen and sometimes not; or the like. If from simple substances we pass to any of their definite compounds such as water, lime, or sulphuric acid, there's the same constancy in their properties. When properties vary from individual to individual, the individuals are either

> •miscellaneous aggregations such as atmospheric air or rock, composed of heterogeneous substances and not belonging to any real Kind, or
> •organisms.

In organisms there is great variability: animals of the same species and race, human beings of the same age, sex, and nationality will be extremely unalike, e.g. in face and figure. But there's reason to believe that none of their properties are ultimate—that all of them are derivative, produced by causation. Why? Because •an organism is regulated by an extremely complicated system of laws, so that it's open to being influenced by more (and more various) causes than any other phenomenon; and •the organism itself had a beginning, and therefore a cause. The presumption ·of non-ultimateness· is confirmed by the fact that the properties that vary from one individual ·organism· to another also generally vary at different times in the same individual; any such variation, like any other event, has a cause and thus implies that the properties are not independent of causation.

So if blackness is merely accidental in crows, and can vary while the Kind remains the same, its presence or absence is doubtless not an ultimate fact but the effect of some unknown cause. If that is so, the universality of the experience that all crows are black is sufficient proof of a common cause, and establishes 'All crows are black' as an empirical law. Because there are countless instances in the affirmative, and so far none in the negative, the causes the property depends on must exist everywhere within the limits of the observations that have been made; and the proposition can be accepted as universally true within those limits, and with the permissible degree of extension to adjacent cases.

§7. In the second place—·i.e. picking up on item **(ii)** in **§5**·—if the property, in the instances in which it has been observed, is not an effect of causation, it is a property of Kind; and in that case the generalisation 'All crows are black' can't be set aside except by the discovery of a new Kind of crow. But it's not very improbable that a hitherto-undiscovered Kind should turn out to exist in nature—it happens often. We have no basis for trying to limit the Kinds of things that exist in nature. The only unlikelihood is the discovery of a new Kind in a region that we previously had reason to think we had thoroughly explored; and even this improbability depends on the how *conspicuously* different the newly-discovered Kind

is from all the others. We often detect in the most frequented situations new Kinds of minerals, plants, and even animals that we had overlooked or confused with known species. On this second ground, therefore, as well as on the first, the observed uniformity of coexistence can only hold good as an empirical law, within the limits of actual observation as accurate as the nature of the case requires. That's why it is that (as I remarked in chapter 3.3 (page 155) we so often give up generalisations of this class at the first challenge. If any credible witness said he had seen a white crow, under circumstances that made it credible that it should have escaped notice until then, we would believe him.

So we find that uniformities in the coexistence of phenomena—those we have reason to regard as ultimate, as well as those that arise from the laws of causes that we haven't yet detected—are entitled only to be accepted as empirical laws; and aren't to be presumed true except within the limits of time, place, and circumstance in which the observations were made, or in strictly adjacent cases.

§8. We saw in chapter 21,3 page 291) that when empirical laws reach a certain point of generality they become as certain as laws of nature—or rather, at that point there's no longer any distinction between •empirical laws and •laws of nature. As empirical laws approach this point—i.e. as they become more general—they also become more certain, so that their universality can be more strongly relied on. Even with the uniformities treated of in this chapter we can never be certain that they aren't results of causation; and if they are, the more general they are the greater is the space in which
   •the necessary collocations occur and
   •no causes exist that could counteract the unknown
   causes on which the empirical law depends.

To say that P is an invariable property of **some very limited class of objects** is to say that P invariably accompanies some numerous and complex group of distinguishing properties; and this, if causation is at all concerned in the matter, indicates a combination of many causes and therefore a great openness to counteracting causes; while the comparatively narrow range of the observations makes it impossible for us to predict how widely unknown counteracting causes may be distributed throughout nature. But when a generalisation has been found to hold good of **a very large proportion of all things whatever**, it is already proved that most of the causes in nature have no power over it; that few changes in the combination of causes can affect it; because the majority of possible combinations must have already existed in some of the instances in which it has been found true. So if an empirical law is a result of causation, the more general it is the more it can be depended on. And even if it's not a result of causation but is an ultimate coexistence, the more general it is the greater the amount of experience it is derived from, so the greater is the probability that if exceptions had existed some of them would already have shown up.

For these reasons, much more evidence is needed to establish an exception to one of the more general empirical laws than to establish an exception to a more special one. We could easily believe that there might be a new Kind of crow, i.e. a new Kind of bird resembling a crow in the properties we have until now regarded as distinctive of *crow*. It would be much harder to convince us of the existence of a Kind of crow having properties at variance with any *generally* recognised universal property of *birds*; and even harder if the properties conflicted with any recognised universal property of *animals*. And that fits the way of judging that is approved by mankind's common sense and general practice; how incredulous people are about ·alleged· novelties in nature

depends on how general the experience is that these novelties would contradict.

§9. It is conceivable that the alleged properties might conflict with some recognised universal property of all *matter*. Their improbability would be at the highest but it still wouldn't amount to incredibility. There are only two known properties common to all matter,. . . .namely resistance to movement and gravitation. As Bain expresses it, inertia and gravity are coexistent through all matter, and proportionate ·to one another· in their amount. Neither of these properties, as he truly says, implies the other; and just for that reason we always have to allow that a Kind may be discovered having one of the two properties without the other. The hypothetical 'ether', if it exists, may be such a Kind. Our senses can't recognise either resistance or gravity in it; but if the reality of a **resisting** medium should eventually be proved (e.g. by alteration in the times of revolution of comets, combined with evidence provided by the phenomena of light and heat), it would be rash for us to conclude from this alone, without other proofs, that it must **gravitate**.

Even the greater generalisations, which concern comprehensive Kinds that include a great number and variety of lowest species, are only empirical laws that rest merely on induction by simple enumeration and not on any process of elimination—a process inapplicable to this sort of case. Such generalisations, therefore, ought to be based on an examination of *all* the lowest species covered by them—not just *some* of them. Just because a proposition is true of a number of animals we can't conclude that it is therefore true of all animals. If anything P is true of two species x and z that differ more from one another than either differs from a third species y, especially if y occupies in most of its known properties a position between x and z, there's some

probability that P will also be true of y; for it is often (not always) found that there's a sort of parallelism in the properties of different Kinds, and that their degree of unlikeness in one respect bears some proportion to their unlikeness in others. We see this parallelism in the properties of the different metals; in those of sulphur, phosphorus, and carbon; of chlorine, iodine, and bromine; in the natural orders of plants and animals, etc. But there are countless anomalies and exceptions to this sort of conformity—if indeed the conformity itself is anything but an anomaly and an exception in nature.

So we learn this about universal propositions that •concern the properties of superior Kinds and •are not based on proved or presumed causal connection: they ought not to be hazarded until one has separately examined every known sub-kind included in the larger Kind; and even then they must be held in readiness to be given up when some new anomaly turns up, which is likely enough to happen, even with the most general of these empirical laws. Thus, the many universal propositions that people have tried to lay down concerning •simple substances or •any of the classes that have been formed among simple substances have either faded into emptiness with the progress of experience or been proved to be erroneous; and each Kind of simple substance remains with its own collection of properties apart from the rest, apart from a certain parallelism with a few other Kinds that are the most similar to itself. In organisms, indeed, many propositions have been ascertained to be universally true of genera that are that are high in the classification table, and to many of these the discovery of exceptions is extremely improbable. But these, as I said already, we have every reason to believe that these properties depend on causation ·and therefore lie outside the scope of this paragraph·.

Uniformities of coexistence, then, not only when they follow from laws of succession but also when they are

ultimate truths, must for logical purposes be classified among empirical laws, and fall under exactly the same rules as the unresolved uniformities that are known to depend on causation.

Chapter 23. Approximate generalisations. Probable evidence

§1. As well as generalisations from experience that profess to be universally true there are inductive truths that don't claim to be universal—don't say that the predicate is *always* true of the subject—but which are nevertheless extremely valuable. An important part of the field of inductive knowledge consists not of universal truths but of approximations to such truths; and when a conclusion is said to rest on probable evidence, the premises it is drawn from are usually generalisations of this sort.

Just as every **certain** inference about a particular case implies that there is ground for a general proposition of the form 'Every A is B', so also every **probable** inference supposes that there's ground for a proposition of the form 'Most A are B'; and in an average case the degree of probability of the inference will depend on the proportion between •the number of instances existing in nature that accord with the generalisation and •the number that conflict with it.

§2. Propositions of the form 'Most A are B' are much less important in science than in everyday life. To the scientific inquirer they are valuable mainly as stepping-stones to universal truths. The discovery of universal truths is the proper end of science; its work isn't done if it stops at the proposition that *a majority of A are B*, without providing some way of marking off that majority from the minority. As well as being

•relatively imprecise and •impossible to apply confidently to individual cases, these imperfect generalisations are •almost useless as means of discovering ulterior truths through deduction. Admittedly we can infer 'Most A are C' from 'Most A are B' and 'Every B is C'; but in most cases where a second proposition of the approximate kind is introduced—or even when there's only one and it is the major premise—nothing can be positively concluded. When the major is 'Most B are D' then even if the minor is 'Every A is B' we can't infer that most A are D; we can't even infer with any certainty that *some* A are D. Though the majority of the class B have the attribute signified by D, the whole of the sub-class A may belong to the minority.[1]

For practical guidance, however, approximate generalisations are often all we have to rely on. Even when science has discovered the universal laws of a phenomenon, they don't serve our everyday purposes. •They are usually too cluttered with conditions to be suitable for everyday use; and •the cases that turn up in ordinary life are too complicated, and our decisions have to be taken too rapidly, to allow us to wait until the existence of a phenomenon can be proved by what have been scientifically ascertained to be universal marks of it. To be indecisive and reluctant to act because we don't have perfectly conclusive evidence to act on is a defect

---

[1]   De Morgan in his *Formal Logic* rightly says that from 'Most A are B' and 'Most A are C' we can infer with certainty that some B are C. But this is the utmost limit of the conclusions that can be drawn from two approximate generalisations whose precise degree of approximation to universality is unknown or undefined.

sometimes found in scientific minds, and when that happens it makes the mind in question unfit for practical emergencies. If we want to succeed in action, we must judge by indications that sometimes (though not usually) mislead us, and we must try to make up for the incomplete conclusiveness of one indication by obtaining others to corroborate it. So the principles of induction applicable to approximate generalisations are as important a subject of inquiry as the rules for the investigation of universal truths. You might reasonably expect the former inquiry to occupy nearly as much of this book as the latter, ·but in fact it won't, because· the principles governing approximate generalisations are mere corollaries of the principles I have already discussed—namely the principles governing universal propositions.

§3. There are two sorts of cases where we have to steer by generalisations of the form 'Most A are B'. **(i)** They are all we have; we haven't been able to carry our investigation of the laws of the phenomena any further. For example:
- •Most dark-eyed persons have dark hair;
- •Most springs contain mineral substances;
- •Most stratified formations contain fossils.

This class of generalisations isn't very important, and here is why. It often happens that we see no *reason* why what's true of most individuals in A isn't also true of the remainder, and we can't find a general description that marks off the ones of which it is true from the remainder, yet if we will settle for propositions that are less general and will break down the class A into sub-classes, we can generally obtain a collection of propositions that are exactly true. We don't know why most wood is lighter than water, nor can we point out any general property marking off wood that is lighter than water from wood that is heavier. But we know exactly which species are the one and which the other. . . .

(ii) It often happens, however, that 'Most A are B' is not the peak of our scientific attainments, though the knowledge we have that goes further can't conveniently be brought to bear upon the particular instance. Even when we know what circumstances distinguish the part of A that has B from the part that doesn't, it can happen in an individual case that we don't have the means (or don't have time) to examine whether those characteristic circumstances exist or not. This is generally our situation when the inquiry is of the kind called 'moral', i.e. the kind that aims to predict human actions. If we are to affirm universally anything about the actions of classes of human beings, the classification must be based on the circumstances of their mental culture and habits, which in an individual case are seldom exactly known; and classes based on these distinctions would never exactly coincide with the classes into which mankind are divided for social purposes. All propositions about the actions of human beings as ordinarily classified, or as classified according to any kind of external indications, are merely approximate. We can only say 'Most persons of a particular age, profession, country, or rank in society, have such-and-such qualities'; or 'Most persons, when placed in certain circumstances, act in such-and-such a way'. We often know well enough what causes the qualities depend on, or what sort of persons they are who act in that particular way; but we seldom have the means of knowing whether any individual person has been under the influence of those causes, or is a person of that particular sort. We could replace the approximate generalisations by universally true propositions; but these would hardly ever be applicable in practice. We would be sure of our majors, but we wouldn't be able to get minors to fit; so we are forced to draw our conclusions from coarser and more fallible indications.

§4. An approximate generalisation can be accepted only as an empirical law. Propositions of the form 'Every A is B' *aren't necessarily* laws of causation, or ultimate uniformities of coexistence; propositions like 'Most A are B' *necessarily aren't* so. Propositions that have been true in every observed instance needn't follow necessarily from laws of causation; and if they don't, they may for all we know be false beyond the limits of our observation; and this holds even more obviously for propositions that are true only in a mere majority of the observed instances.

How certain we can be of the proposition 'Most A are B' depends in part on whether **(i)** that approximate generalisation is the whole of our knowledge of the subject or **(ii)** it isn't. In the case **(i)** we know only that most A are B, not why they are so nor in what respect those that are B differ from those that aren't. Then how did we learn that most A are B? In exactly the way in which we would have learned that all A are B if that had been the fact of the matter. We collected enough instances to rule out chance, and then compared the number of affirmative instances with the number of negative ones. The result, like other unresolved derivative laws, can be relied on only within the limits of place and time *and circumstance* under which its truth has been observed. ·Why 'and circumstance'?· Because we are ignorant of the causes that make the proposition true, so we can't tell how any new circumstance might affect it. The proposition 'Most judges can't be swayed by bribes' would probably be found true of Englishmen, Frenchmen, Germans, North Americans, and so forth; but if on this evidence we extended the assertion to Orientals we would be overstepping the limits, not only of place but of circumstance, within which the fact had been observed, and would let in possibilities of the absence of the determining causes or the presence of counteracting ones that might be fatal to the approximate generalisation.

(ii) When the approximate proposition is not the peak of our scientific knowledge but only the most available form of it for practical guidance—when we know not only that most A have the attribute B but also the causes of B or some properties that mark off the portion of A that has B from the portion that doesn't—we are better placed than we were in **(i)**. Now we have two ways of ascertaining whether it's true that most A are B:

•the direct way, as in **(i)**, and
•an indirect way, namely examining whether the proposition can be deduced from the known cause of B or from any known criterion of B.

Consider the question 'Is it true that most Scotchmen can read?' We and our informants may not have observed a sufficient number and variety of Scotchmen to ascertain this fact; but when we consider that the ability to read is caused by being taught to read, another way of answering the question presents itself, namely inquiring whether most Scotchmen have been sent to schools where reading is effectively taught. Sometimes one of these two approaches is the more available, sometimes the other. . . . It often happens that neither can yield as satisfactory an induction as could be desired, and that the grounds on which the conclusion is accepted are compounded of both. . . .

[Mill adds a paragraph saying that it is sometimes right for us to go beyond 'Most A are B' when we know enough to do so. Should we believe this witness to the crime? We wouldn't want to answer that simply on the grounds that 'Most persons on most occasions speak the truth'. He concludes the section:] It seems unnecessary to spend longer on the question of the evidence of approximate generalisations; so I'll proceed to an equally important topic, that of the cautions to be observed in arguing from these incompletely universal propositions to particular cases.

§5. There's no difficulty about this when it's a matter of directly applying an approximate generalisation to an individual instance. If 'Most A are B' has been established, by a sufficient induction, as an empirical law, we can conclude that *This particular A is B* with a probability based on the preponderance of the number of affirmative instances over the number of exceptions. If we have numerical precision in the data, we can have equal precision about the chances of error in the conclusion. If we have established as an empirical law that *nine out of every ten A are B* there will be one chance in ten of error in assuming that any given A is a B; but this holds only within the same limits of time, place, and circumstance as bounded the observations, so it can't be counted on for any sub-class or variety of A (or for A in any set of external circumstances) that weren't included in the average. We can guide ourselves by the proposition *Nine out of every ten A are B* only in cases of which we know only they are within the class A. If we know that a particular instance *i* not only that it belongs to A but also what species or variety of A it belongs to, we'll usually go wrong in applying to *i* the average we have found for the whole genus A, because the average corresponding to that species alone would probably differ from it materially. Similarly, if *i*, instead of being a particular sort of instance, is an instance known to be affected by a particular set of circumstances, it would again probably be misleading to apply to *i* the same probability of being B as holds on average for all of A's members. A general average should be applied only to cases that aren't known to be, and can't be presumed to be, other than average cases. Such averages, therefore, are usually of little practical use except in affairs that concern large numbers. Tables of life-expectancy are useful to insurance offices, but they don't go far towards informing you about your life-expectancy or me about mine, because almost everyone has a life-expectancy that is either better or worse than the average. Such averages merely supply the first term in a series of approximations, the subsequent terms reflecting growing knowledge of the circumstances of the particular case.

§6. From the application of a single approximate generalisation to individual cases, I proceed to the application of two or more of them together to the same case.

When a judgment J applied to an individual instance is based on the conjunction of two approximate generalisations P_1 and P_2, the latter may support J in two different ways. **(a)** In one, P_1 and P_2 are each separately applicable to the case in hand, and we combine them so as to give to J the double probability arising from P_1 and P_2 separately. This could be called joining two probabilities by way of **addition**; it gives to J a greater probability than either P_1 or P_2 has. **(b)** The other occurs when P_1 is directly applicable to the case, P_2 being applicable to it only by virtue of the application of P_1. This is joining two probabilities by way of **ratiocination or deduction**; it gives to J a lower probability than either P_1 or P_2 has. The type of **(a)** is

> Most A are B;
> Most C are B;
> This thing is both an A and a C; therefore
> This thing is probably a B.

The type of **(b)** is

> Most A are B;
> Most C are A;
> This thing is a C; therefore
> This thing is probably an A, therefore
> This thing is probably a B.

Examples of **(a)**: the guilt of the accused man is inferred from •the testimony of two unconnected witnesses, or from

•the evidence of two incriminating facts—e.g. he concealed himself, and his clothes were stained with blood. Examples of **(b)**: the man's guilt is inferred from •one witness's testimony about what he heard another person say, or from •the fact that he washed or destroyed his clothes, which is supposed to make it probable that they were stained with blood. Instead of only two links, as in these instances, there can be chains of any length. . . .

(a) When approximate generalisations are joined by way of addition, we can use the theory of probabilities laid down in chapter 17 to work out how each of them adds to the probability of a conclusion that has the support of them all.

If on average two of every three As are Bs, and three of every four Cs are Bs, what is the probability that something that is both an A and a C is also a B? [Mill presents his answer to this in two rather obscure plain-language versions, and then more clearly thus:] The chance that an A is not a B is $1/3$, the chance that a C is not a B is $1/4$; hence if the thing is both an A and a C, the chance of its not being a B is $1/3 \times 1/4 = 1/12$, and the chance of its being a B is $11/12$.[1]

This computation assumes of course that the probabilities arising from A and C are independent of each other. There mustn't be any connection between A and C such that a thing's belonging to one affects the probability of its belonging to the other. Otherwise the not-Bs that are Cs may be, most or even all of them, identical with the not-Bs that are As; in which last case the probability arising from A and C together will be no greater than that arising from A alone.

(b) When approximate generalisations are joined together by way of deduction, the probability of the conclusion *lessens* at each step. From two premises such as *Most A are B* and *Most B are C* we can't with certainty conclude that even a single A is C; for the whole of the portion of A that falls under B may be contained in the exceptional part of B, ·the part that doesn't fall under C·. Still, those two propositions provide an appreciable probability that any given A is C, provided the average on which *Most B are C* is based wasn't biased by any reference to *Most A are B*. That is, the proposition *Most B are C* must have been arrived at in a manner leaving no suspicion that the probability arising

---

[1] [Mill has here a long footnote in which he reports an objection that 'a mathematical friend' made to this paragraph. He states the reasoning behind the objection and admits that in the seventh edition of this work 'I accepted this reasoning as conclusive. More attentive consideration, however, has convinced me that it contains a fallacy.' He is right, and we needn't go through all this. Here's its last paragraph:] The true theory of the chances is best found by going back to the scientific grounds on which the proportions rest. The degree of frequency of a coincidence depends on, and is a measure of, the-frequency-combined-with-the effectiveness of the causes that are favourable to it. If out of every twelve As taken indiscriminately eight are Bs and four are not, this implies that

there are causes operating on each A that tend to make it a B, and these causes are sufficiently constant and powerful to succeed in eight out of twelve cases, but fail in the remaining four.

So if out of twelve Cs nine are Bs and three are not, it must be the case that

there are causes operating on each C that tend to make it a B, and these causes succeed in nine cases and fail in three.

Now suppose twelve items that are both As and Cs. The whole twelve are now operated on by both sets of causes. One set is sufficient to prevail in eight of the twelve cases, the other in nine. The analysis of the cases shows that six of the twelve will be Bs through the operation of both sets of causes; two more in virtue of the causes operating on A; and three more through those operating on C, and that there will be only one case in which all the causes will be inoperative. The total number, therefore, which are Bs will be eleven in twelve, and the evaluation in the text is correct.

from it is not fairly distributed over the section of B that belongs to A. For though the instances that are A *could* be all in the minority, they also *could* be all in the majority; and these two possibilities cancel out. On the whole, the probability arising from the two propositions taken together will be correctly measured by the probability arising from the one multiplied by the probability arising from the other. If nine out of ten Swedes have light hair, and eight out of nine inhabitants of Stockholm are Swedes, the probability arising from these two propositions that any given inhabitant of Stockholm is light-haired will amount to $8/10$, though it is *possible* that the whole Swedish population of Stockholm belongs to that tenth of the people of Sweden who don't have light hair.

[Where this paragraph has 'Let the proposition *Most A are B* be true because nine-tenths of the As are B', what Mill actually wrote was 'Let the proposition, Most A are B, be true of nine in ten'. That doesn't make sense: there's no way 'Most A are B' could be *true of* any individual A or, therefore, of nine As out of every ten. Other occurrences of this slip are silently corrected.] If the premises are known to be true not because of •a bare majority of their respective subjects but because of •nearly the whole, we can go on joining one such proposition to another for several steps before reaching a conclusion that isn't presumably true even of a majority. The error of the conclusion will amount to the sum of the errors of all the premises. Let the proposition *Most A are B* be true because nine-tenths of the As are B, and let *Most B are C* be true because eight-ninths of Bs are C; then not only will one A in ten not be C, because not B, but even of the nine-tenths that are B only eight-ninths will be C; i.e. the cases of A that are C will be only $8/9 \times 9/10 = 72/90 = 4/5$. Let us now add *Most C are D* and suppose this to be true because seven-eighths of Cs are D; the proportion of A that is D will be only $7/8 \times 8/9 \times 9/10 = 7/10$. Thus the probability

progressively dwindles. But we usually can't *measure* the lessening of probability that occurs at each step, because the experiences on which our approximate generalisations are based usually can't be numerically estimated. So we have to settle for remembering •that it does diminish at every step, and •that the conclusion after a few steps is worth nothing unless the premises are extremely close to being universally true. A hearsay of a hearsay, or an argument from supposed evidence that depends not on immediate marks but on marks of marks is worthless at a very few removes from the first stage.

§7. There are, however, two cases in which reasonings depending on approximate generalisations can be carried to any length we please •with as much assurance as if they were composed of universal laws of nature and •with no departure from strictly scientific standards. . . . These are cases where the approximate generalisations are, for purposes of ratiocination, as suitable as if they were complete generalisations, because they can be transformed into exactly equivalent complete generalisations.

(i) If our reason for stopping at 'Most As are B' is not the impossibility but only the inconvenience of going further—i.e. if we know what marks off the As that are B from those that aren't—we can replace the approximate proposition by a universal proposition with a proviso. The proposition

> 'Most persons who have uncontrolled power employ it badly'

is a generalisation of this sort, and can be replaced by

> All persons who have uncontrolled power employ it badly provided they don't have unusual strength of judgment and rectitude of purpose.

The proposition carries the hypothesis or proviso with it, so it can be dealt with not as an approximate proposition

but as a universal one. However many steps the reasoning takes, the proviso is carried along to the conclusion and indicates exactly how far the conclusion is from being applicable universally. If other approximate generalisations are introduced along the way, each of them also being expressed as a universal proposition with a proviso attached, the sum of all the provisos will appear at the end as the sum of all the errors that affect the conclusion. To the indented proposition a few lines back let us add

> All absolute monarchs have uncontrolled power unless they need the active support of their subjects

(as was the case with Queen Elizabeth, Frederick of Prussia, and others). Combining these two propositions we can deduce a universal conclusion that will be subject to both the provisos in the premises:

> All absolute monarchs employ their power badly unless they need the active support of their subjects, or unless they are persons of unusual strength of judgment and rectitude of purpose.

It doesn't matter how rapidly the provisos in our premises accumulate, as long as we can in this way record each of them and keep an account of the aggregate as it swells up.

(ii) There is a case where approximate propositions count for scientific purposes as universal ones, even if we don't know the conditions that mark off the 'most' from the others. This occurs when we are studying the properties not of •individuals but of •multitudes. The main one is the science of politics, or of human society. This science is principally concerned with the actions not of solitary individuals but of masses, with the fortunes not of single persons but of communities. For a statesman it is generally enough to know that *most* persons act or are acted on in a particular way; since his theorising and his practical arrangements refer almost exclusively to cases in which the whole community, or some large portion of it, is acted on all at once, so that what is done or felt by *most* persons determines what the body at large does or undergoes. He can get on well enough with approximate generalisations on human nature, since what is true approximately of all individuals is true absolutely of all masses. [That striking sentence is verbatim from Mill.] And even when the conduct of individual men have a part to play in the statesman's deductions—e.g. when he is reasoning about kings or other single rulers—still he must in general both reason and act as if what is true of most persons were true of all, because he is providing for indefinite duration involving an indefinite succession of such individuals.

Those two considerations are a sufficient refutation of the popular error that theorising about society and government, because it rests on merely probable evidence, must be less certain and scientifically accurate than the conclusions of what are called the exact sciences, and less reliable in practice. There are reasons enough why the sciences dealing with human behaviour must remain inferior to at least the more perfect of the physical sciences; why the laws of their more complicated phenomena can't be so completely deciphered, or their phenomena predicted with as much assurance. But though we can't attain to so many truths, there is no reason why those we *can* attain should deserve less reliance, or have less of a scientific character. I'll drop this topic now, and return to it in Book VI.

Chapter 24. The remaining laws of nature

§1. I showed in I.5 that all the assertions that can be conveyed by language express one or more of five things:

> existence
> order in place
> order in time
> causation
> resemblance.

Causation, on my view of it, isn't fundamentally different from order in time, so the five species of possible assertions are reduced to four. The present Book up to here has been concerned with order in time in each of its two modes, coexistence and succession. And now I have finished with that topic insofar as it falls within the limits assigned to this work, discussing the nature of the evidence on which order-in-time propositions rest, and the processes of investigation by which they are ascertained and proved. There remain three classes of facts—existence, order in place, and resemblance—in regard to which the same questions are now to be answered.

Little needs to be said about **existence** in general, which is a topic not for logic but for metaphysics. To determine what things can be recognised as really existing independently of our own states of mind, what 'exists' means as applied to such things, belongs to the consideration of 'Things in themselves', a topic that I have kept at as great a distance as possible throughout this work. Existence, so far as logic is concerned about it, has reference only to *phenomena*—to actual or possible states of external or internal consciousness in ourselves or in others. The only things whose existence can be a subject of logical induction are the feelings of beings that have them, or the possibilities of having such feelings; because those are the only things whose existence in individual cases can be a subject of experience [= 'can be known through experience'].

It's true that we say a thing 'exists' even when it is absent and therefore can't be perceived. But then its 'existence' is to us only another word for our conviction that we would perceive it. . . .if we were in the appropriate circumstances of time and place and had perfect sense-organs. My belief that the Emperor of China exists is simply my belief that if I were transported to the imperial palace or some other locality in Peking I would see him. My belief that Julius Caesar existed is my belief that I would have seen him if I had been present ·at an appropriate time· in the senate-house at Rome. When I believe that stars exist further away than I can see even with help from the most powerful telescopes yet invented, my belief, philosophically expressed, is that with still better telescopes I could see them, or that they could be perceived by beings closer to them in space or equipped with better eyesight than mine.

So a phenomenon's 'existence' is simply another word for its *being perceived* or for the inferred *possibility of its being perceived.* When the phenomenon is within the range of present observation, that's how we assure ourselves of its existence; when it is beyond that range and is therefore said to be 'absent', we infer its existence from marks or evidences. These evidences are other phenomena that are ascertained by induction to be connected—either in succession or in coexistence—with the given phenomenon. So the simple existence of an individual phenomenon, when it's not directly perceived, is inferred from some inductive law of succession or coexistence; and consequently it can't be brought under any inductive principles that are special to itself. We prove

310

the existence of a thing by proving that it is connected by succession or coexistence with some known thing.

General propositions of this class, i.e. ones affirming the bare fact of existence, have a special feature that makes the logical treatment of them a very easy matter—namely, being generalisations that are sufficiently proved by a single instance. That ghosts or unicorns or sea-serpents exist would be fully established if it could be ascertained definitely that such things had been seen even once. Whatever has once happened can happen again; the only question relates to the conditions under which it happens.

With simple •existence, therefore, inductive logic has no knots to untie. So we can move on to the remaining two great classes into which facts have been divided. •resemblance and •order in place.

§2.   Resemblance and its opposite are seldom regarded as subjects of science (except when they take the form of equality and inequality). They're supposed to be perceived by simple apprehension; by merely applying our senses or directing our attention to the two objects at once, or in immediate succession. And this simultaneous (or virtually simultaneous) application of our faculties to the two things that are to be compared is indeed the ultimate appeal wherever it can be done; but in most cases the objects can't be brought so closely together that a complete feeling of their resemblance directly arises in the mind. All we can do is to compare them with some third object that can be transported from one to the other. And even when the objects *can* be set side by side, we don't have a perfect knowledge of their resemblance or difference unless we compare them minutely, part by part. Until that is done things that are really very dissimilar often appear absolutely alike. Two lines of very different lengths will appear about equal when lying

in different directions; but if we put them parallel with their distant ends even, and then look at the nearer ends, we can directly perceive their inequality.

So it's not always as easy as you might think to ascertain •whether two phenomena are alike and •how they differ if they do differ. When the two can't be brought together in a way that lets the observer compare their several parts in detail, he must come at the comparison indirectly, through reasoning and general propositions. When we can't bring two straight lines together to determine whether they are equal ·in length·, we do it with the •physical aid of a foot-rule applied first to one and then to the other, and the •logical aid of the general proposition 'Things that are equal to the same thing are equal to one another'. The comparison of two things through the intervention of a third thing when their direct comparison is impossible—that's the appropriate scientific process for ascertaining resemblances and dissimilarities, and it's the sum total of what logic has to teach on this subject.

Locke stretched this line of thought too far, holding that •reasoning itself is nothing but the comparison of two ideas through the medium of a third, and •knowledge is the perception of the agreement or disagreement of two ideas—doctrines that the Condillac school blindly adopted, without the qualifications and distinctions that they were carefully guarded with by their illustrious author. Of course when the question one is pursuing is actually about the agreement or disagreement (i.e. the resemblance or dissimilarity) of two things, as happens especially in the arithmetic and geometry, then if a solution can't be found by direct perception it must be indirectly sought by comparing these two things through the medium of a third. But this is far from being true of all inquiries. The knowledge that *bodies fall to the ground* is a perception not of •agreement

or disagreement but of •a series of physical occurrences, a succession of sensations. Locke's definitions of *knowledge* and of *reasoning* needed to be limited to knowledge of and reasoning about resemblances. Even then, what he says isn't strictly correct, because the comparison is made not between 'the ideas of' the two phenomena but between the phenomena themselves. I pointed out this mistake in I.5.1 and II.5.5, and traced it to an imperfect conception of what happens in mathematics, where very often the comparison really *is* made between the ideas, without any appeal to the outward senses; but that's only because in mathematics a comparison of the ideas is strictly equivalent to a comparison of the phenomena themselves. In the case of numbers, lines, and figures, our idea of an object is a complete picture of the object so far as the matter in hand is concerned; so we can learn from the picture whatever could be learned from the object itself by merely contemplating it at the instant when the picture is taken. No mere contemplation of •gunpowder would ever teach us that a spark would make it explode, so the contemplation of •the idea of gunpowder wouldn't do that either; but the mere contemplation of a straight line shows that it can't enclose a space, so the contemplation of the idea of it will show the same. What takes place in mathematics is thus no argument that the comparison is always between the ideas. It is always, either indirectly or directly, a comparison of the phenomena.

In some cases we can't bring the phenomena to the test of direct inspection at all, or not in a precise enough way, but must judge of their resemblance by inference from other (dis)similarities that are more open to observation. In those cases we of course require, as in all ratiocination, generalisations or formulae applicable to the subject. We must reason from laws of nature—from observable *uniformities* involving likeness or unlikeness.

§3. The most comprehensive of these laws or uniformities are the ones supplied by mathematics—the axioms relating to equality, inequality, and proportionality, and the various theorems based on them. And these are the only Laws of Resemblance that need to be treated separately—indeed the only ones that *can.* There are indeed countless other theorems affirming resemblances among phenomena, e.g. that the angle of the reflection of light is equal to its angle of incidence (equality being merely exact resemblance in magnitude), and that the planets describe equal areas in equal times. . . . Propositions like these affirm resemblances of the same sort as those asserted in mathematical theorems; what is different ·between mathematics and physical sciences· is that the propositions of mathematics are true of all phenomena, or at least without distinction of origin; while the truths of physical science are affirmed only of special phenomena that originate in a certain way; and the equalities, proportionalities, or other resemblances that exist between such phenomena must be either derived from, or identical with, the law of their origin—the law of causation they depend on. The equality of the areas described in equal times by the planets is derived from the laws of the causes, and until its derivation was shown it was ·merely· an empirical law. The equality of the angles of reflection and incidence is identical with the law of the cause; because the cause is a light-ray's hitting a reflecting surface, and the equality in question is the very law according to which that cause produces its effects. So this class of uniformities of resemblance between phenomena are inseparable—in fact and in thought—from the laws of the production of those phenomena; and the principles of induction applicable to them are precisely the ones I have discussed in the preceding chapters of this Book.

Not so with mathematical truths. The laws of equality and inequality between spaces, or between numbers, have no connection with laws of causation. The proposition that

>   the angle of reflection equals the angle of incidence

is a statement of the mode of action of a particular cause; the proposition but that

>   when two straight lines intersect, the opposite angles are equal

is true of all such lines and angles, whatever their causes are. That

>   the squares of the periodic times of the planets are proportional to the cubes of their distances from the sun

is a uniformity derived from the laws of the causes (or forces) that produce the planetary motions; but that

>   the square of any number is four times the square of half the number

is true independently of any cause. So the only laws of resemblance that we have to consider independently of causation are those of mathematics.

§4. The same thing is evident with respect to the last of the five categories ·listed on page 310·, namely **order in place**. The order in place of the effects of a cause x is (like everything else that's true of x's effects) a consequence of x's laws. The order in place—which I have been calling the 'collocation'—of the absolutely basic causes is. . . .in each instance an ultimate fact in which no laws or uniformities are traceable. The only remaining general propositions about order in place, and the only ones having nothing to do with causation, are some of the truths of geometry. I'm talking about laws that enable us to infer from •the order in place of certain points, lines, or spaces •the order in place of others that are connected with the former in some known way,

this being done without bringing in the physical cause from which they happen to derive their origin, and indeed without bringing in *any* facts about those points, lines, or spaces other than facts about position or magnitude.

It turns out, therefore, that mathematics is the only department of science whose methods I still have to inquire into. This needn't take long, because I have already gone a fair distance into it in Book II. I said there that the directly inductive truths of mathematics are few in number—only •the axioms and •certain existence-propositions that are tacitly involved in most of the so-called definitions. And I gave reasons—seemingly conclusive ones—for affirming that these basic premises from which the remaining truths of mathematics are deduced are results of observation and experience, i.e. are based on the evidence of the senses. They don't seem to be, but they are. That *things equal to the same thing are equal to one another* and that *two straight lines that have once intersected one another continue to diverge* are inductive truths that rest—as does the law of universal causation—only on induction *per enumerationem simplicem*, i.e. on the fact that they have been perpetually perceived to be true and never once found to be false. But ·there's a difference between the law of causation and these mathematical axioms·. For a long time there were events that appeared not to be caused, though really they were; but with the axioms of mathematics there aren't even *apparent* exceptions. All that's needed to perceive the truth of one of them in any individual case is the simple act of looking at the objects in a proper position. Their infallible truth was recognised from the very dawn of theoretical thought; and because their extreme familiarity made it impossible for the mind to conceive the objects in any other way, the axioms came to be (and still are) generally considered as self-evidently true, i.e. as truths recognised by instinct.

§5. Something that seems to require explanation is the fact that the immense multitude of truths in the mathematical sciences (a multitude still as far as ever from being exhausted) can be extracted from so few elementary laws. It's hard to see, at first, how there can be room for such an infinite variety of true propositions on subjects that are apparently so limited.

To begin with the science of number. The elementary or ultimate truths of this science are the common axioms concerning equality, namely, 'Things that are equal to the same thing are equal to one another', and 'Equals added to equals make equal sums' (no other axioms are required[1]), together with the definitions of the various numbers. Like other so-called definitions, these are composed of two things—•the explanation of a name and •the assertion of a fact; and only the latter of these can be a first principle or premise of a science. The fact asserted in the definition of a number is a physical fact. Each of the numbers two, three, four, etc. denotes physical phenomena, and connotes [see Glossary] a physical property of those phenomena. *Two* denotes all pairs of things, and *twelve* denotes all dozens of things, connoting what makes them pairs or dozens; and what makes them so is something physical—because it can't be denied that two apples are physically distinguishable from three apples, two horses from one horse, and so on, i.e. that they are a different visible and tangible phenomenon. I'm not undertaking to say what the difference is; it is enough

that there is a difference that the senses can recognise. And although a 102 horses are not distinguished from 103 horses as easily as two horses are from three, the horses *can* be so placed that the difference is perceptible; if that weren't so we would never have distinguished them and given them different names. Everyone knows that weight is a physical property of things; yet small differences between great weights are as imperceptible to the senses in most situations as small differences between great numbers. They become evident only when the two objects are placed in a special position—namely, in the opposite scales of a delicate balance.

Well, then, what *is* connoted by a name of number—·i.e. by a numeral·? Of course it's some property belonging to the agglomeration of things that we call by the name; and that property is

> the characteristic manner in which the agglomeration is made up parts and can be separated into parts.

I'll try to make this more intelligible by a few explanations.

When we call a collection of objects 'two', 'three', or 'four', they aren't two, three, or four in the abstract; they are two, three, or four things of some particular kind—pebbles, horses, inches, pounds' weight. What the numeral [see Glossary] connotes is the way single objects of the given kind must be put together to produce that particular aggregate. If it's an aggregate of pebbles, and we call it 'two', the name implies that to compose the aggregate one pebble must be

---

[1]   The axiom, 'Equals subtracted from equals leave equal differences' can be demonstrated from the two axioms in the text. If $A = a$ and $B = b$, then $A - B = a - b$. For if not, let $A - B = a - b + c$. Then since $B = b$, adding equals to equals $A = a + c$. But $A = a$. Therefore $a = a + c$, which is impossible. —This proposition having been demonstrated, we can use it to demonstrate the following: 'If equals are added to unequals, the sums are unequal.' If $A = a$ and $B \neq b$, $A + B \neq a + b$. For suppose $A + B = a + b$. Then, since $A = a$ and $A + B = a + b$, subtracting equals from equals, $B = b$; which is contrary to the hypothesis. —We can also prove 'Two things of which one is equal and the other unequal to a third thing are unequal to one another'. If $A = a$ and $A \neq B$, then $a \neq B$. For suppose $a = B$. Then since $A = a$ and $a = B$, and since things equal to the same thing are equal to one another, $A = B$; which is contrary to the hypothesis.

joined to one pebble. If we call it 'three', one and one and one pebble must be brought together to produce it, or else one pebble must be joined to an already existing aggregate of the kind called 'two'. The aggregate that we call 'four' has a still greater number of characteristic modes of formation. One and one and one and one pebble may be brought together; or two aggregates of the kind called 'two' may be united; or one pebble may be added to an aggregate of the kind called 'three'. Every number in the ascending series can be formed by joining smaller numbers in a growing variety of ways. Even limiting the parts to two, the number can be formed (or divided) in as many different ways as there are numbers smaller than itself; and there are even more ways of doing it if we admit threes, fours, etc. Other ways of reaching the same aggregate present themselves, not by uniting smaller aggregates but dismembering larger ones: three pebbles can be formed by removing one pebble from an aggregate of four, two pebbles by an equal division of a similar aggregate, and so on.

Every arithmetical proposition—every statement of the result of an arithmetical operation—is a statement of one of the ways of forming a given number. It affirms that •a certain aggregate A could have been formed by putting together certain other aggregates, or by removing certain portions of some aggregate, and that •and that therefore we could reproduce those aggregates from A by reversing the process.

Thus, when we say that $12^3 = 1728$, what we affirm is this:

If having a sufficient number of pebbles (say), we put them together into the particular sort of aggregates called 'twelves', and put together these twelves again into similar collections, and finally make up twelve of these largest parcels, the aggregate we have formed will be of the sort we call '1728'—namely, that which (to take the most familiar of its modes of formation)

can be made by joining the parcel called 'a thousand' pebbles, the parcel called 'seven hundred' pebbles, the parcel called 'twenty' pebbles, and the parcel called 'eight' pebbles.

The converse proposition that the $1728^{-3} = 12$ says that this large aggregate can again be decomposed into the twelve twelves of twelves of pebbles that it consists of.

There are countless ways of forming any number; but when we know one way of forming a number, all the other ways can be determined deductively. If we know that
- •a is formed from b and c,
- •b is formed from d and e,
- •c is formed from d and f,

and so forth, until we have included all the numbers of any scale we choose to select, we have a set of propositions from which we can reason to all the other ways of forming those numbers from one another. (In doing this we must take care that for each number the mode of formation is really a distinct one, not bringing us round again to the former numbers but introducing a new one.) Having established a chain of inductive truths connecting all the numbers of the scale, we can ascertain the formation of any one of those numbers from any other merely by travelling from one to the other along the chain. Suppose that we know only the following modes of formation:

$$6 = 4 + 2$$
$$4 = 7 - 3$$
$$7 = 5 + 2$$
$$5 = 9 - 4.$$

We could determine how 6 can be formed from 9. For $6 = 4 + 2 = 7 - 3 + 2 = 5 + 2 - 3 + 2 = 9 - 4 + 2 - 3 + 2$. So it can be formed by taking away 4 and 3, and adding 2 and 2. If we also know that $4 = 2 + 2$, we can get 6 from 9 by merely taking away 3.

So we need only to select *one* of the various ways of forming each number, and then we can ascertain all the rest. And since the understanding finds easiest to receive and retain things that are uniform and therefore simple, there's an obvious advantage in •selecting a number-forming mode that is alike for all, •fixing the connotation of numerals on one uniform principle. The system of numerals that we actually use has this advantage, and the additional one of conveying to the mind *two* of the ways of forming every number. Each number is regarded as formed by adding a unit to the number next below it, and this way of forming it is conveyed by its place in the series. And each is also regarded as formed by adding a number of units less than ten, and a number of aggregates each equal to one of the successive powers of ten; and this way of forming it is expressed by its spoken name and by its numerical character.

What makes arithmetic the type [see Glossary] of a deductive science is the role in it of the comprehensive law 'The sums of equals are equals' or (in language that is less familiar but theoretically better) 'Whatever is made up of parts is made up of the parts of those parts'. This truth is obvious to •the senses in all cases that it makes sense to submit to •their judgment, and is so general that it's coextensive with nature itself; and because it's true of all sorts of phenomena. . . .it must be considered an inductive truth—or law of nature—of the highest order. Every arithmetical operation is an application of this law or of other laws that can be deduced from it. This is our warrant for all calculations. We believe that $5 + 2 = 7$ on the strength of •this inductive law combined with •the definitions of those numerals. We arrive at that conclusion—as you may remember from your childhood—by adding units one at a time: $5 + 1 = 6$, therefore $5 + 1 + 1 = 6 + 1 = 7$ and again $2 = 1 + 1$, therefore $5 + 2 = 5 + 1 + 1 = 7$.

§6. The countless true propositions about particular numbers can't unaided give an adequate conception of the extent of the truths that make up the science of number. The propositions I have been speaking of are the least general of all numerical truths. It's true that even these are coextensive with all nature; the properties of the number *four* are true of anything that is divisible into four equal parts, and *everything* is so divisible either actually or ideally. But the propositions making up the science of algebra are true not ·merely· of •a particular number but of •all numbers; not ·merely· of all things considered •as being divided in a particular way but of all things considered •as being divided in any way—as being designated by a numeral at all.

Any number's mode of formation belongs to it alone; it couldn't also be the mode of formation of some other number; so it's a kind of paradox to say both that

> all propositions that can be made about numbers relate to how they are formed from **other** numbers

and yet that

> some propositions are true of **all** numbers.

But this very paradox leads to the real source of generalisation about the properties of numbers. Two numbers can't be formed in the same way from the same numbers; but they can be formed in the same way from different numbers—as nine is formed from three by multiplying it into itself, and sixteen is formed from four by the same process. Thus there arises a classification of ways of forming numbers—i.e. (in the language mathematicians prefer) a classification of *functions*. Any number, considered as formed from any other number, is called a function of it; and there are as many kinds of functions as there are ways of forming numbers. There aren't many simple functions. Most functions are formed by combining several of the operations that form simple functions, or by repetitions of one of those operations.

The simple functions of any number x are all reducible to the following forms:

$x + a$

$x - a$

ax

x/a

x^a

$\sqrt[a]{x}$

$log.x$ (to the base a)

and the same expressions varied by switching x and a wherever that switch would alter the value.... All other functions of x are formed by putting some one or more of the simple functions in the place of x or a, and subjecting them to the same elementary operations.

In order to reason generally about functions we need a system of naming that enables us to express any two numbers by names that show what function each is of the other, without saying what particular numbers they are.... The system of general language called 'algebraical notation' does this. The expressions a and $a^2 + 3a$ denote, respectively, •any number and •the number formed from that in a particular way. The expressions a, b, n, and $(a+b)^n$ denote •any three numbers and •a fourth that is formed from them in a certain way.

Here is the general problem of the algebraical calculus: F being a certain function of a given •number, find what function F will be of any •function of that number. For example, a binomial $a + b$ is a function of its two parts a and b, and the parts are in their turn functions of $a + b$. Now, $(a + b)^n$ is a certain function of the binomial; what function will this be of a and b, the two parts? The answer is the binomial theorem. [Mill states the theorem in its general form; it's hard to take in, and for present purposes it may be enough to say that the special case of it where $n = 2$ is the

familiar equation

$(a + b)^2 = a^2 + 2ab + b^2$

and where $n = 3$

$(a + b)^3 = a^3 + 3a^2b + 3ab^2 + b^3$

and so on. Mill continues:] This shows how the number that is formed by multiplying $a + b$ into itself n times could be formed without that process, directly from a, b, and n. All the theorems of the science of number are like that. They assert the identity of the result of different ways of forming numbers. They affirm that some process of number-forming from x produces the same number as some process of number-forming from a certain function of x.

Besides these general truths or formulae, what remains in the algebraical calculus is the resolution of equations. But the resolution of an equation is also a theorem. If the equation is

$x^2 + ax = b$

the resolution of it, namely

$x = -\frac{1}{2}a \pm \sqrt{\frac{1}{4}a^2 + b}$

is a general proposition, which may be regarded as an answer to the question: 'If b is a certain function of x and a—namely $x^2 + ax$—what function is x of b and a? The resolution of equations is, therefore, a mere variety of the general problem as I have stated it. The problem is: Given a function, what function is it of some other function? And in the resolution of an equation, the problem is to find what function of one of its own functions the number itself is.

That tells you what algebra aims to do. As for its ways of doing it, everyone knows that they are simply deductive. In demonstrating a theorem or solving an equation we travel from the *datum* to the *quaesitum* [= 'from the *given* to the *sought*' = 'from the problem to the solution'] by pure ratiocination. The only premises are •the original hypothesis ·or problem or equation to be solved· and •the fundamental axioms that

things equal to the same thing are equal to one another, and that the sums of equal things are equal. At each step in the demonstration or in the calculation, we apply one or other of these truths or truths deducible from them. . . .

This isn't the place to go further into the analysis of the truths and processes of algebra. There's also no need for me to do so, because a great deal of the task has been performed by other writers. . . . The profound treatises of a truly philosophical mathematician, Augustus De Morgan, should be studied by everyone who wants to understand •why mathematical truths are evident, and •what is meant by the more obscure processes of algebra. What August Comte writes in his *Cours de Philosophie Positive* about the philosophy of the higher branches of mathematics is among the many valuable gifts for which philosophy is indebted to that eminent thinker.

§7. The extreme generality of the laws of number, and their remoteness. . . .from visual and tactual imagination, makes it rather difficult. . . .to think of them as really being physical truths obtained by observation. But that difficulty doesn't arise with regard to the laws of extension. The facts expressed by those laws are of a kind specially accessible to the senses, and suggesting admirably clear images to the imagination. *That geometry is a strictly physical science* would doubtless have been recognised down through the centuries if it hadn't been for the illusions produced by two circumstances: **(i)** the fact (which I mentioned earlier) that the truths of geometry can be collected from our ideas or mental pictures of objects as effectively as from the objects themselves; and **(ii)** the demonstrative nature of geometrical truths, which at one time was supposed to constitute a deep difference between them and physical truths, the latter resting on merely probable evidence and therefore regarded as essentially uncertain and imprecise. The advance of knowledge, however, has shown plainly that physical science in its better understood branches is quite as demonstrative as geometry. The task of deducing its details from a few comparatively simple principles turns out to be anything but the impossibility it was once thought to be; and the supposed greater certainty of geometry is an illusion, arising from the ancient prejudice which mistakes the ideal data from which we reason •in geometry for a special class of realities, while the corresponding ideal data in any deductive •physical science are recognised as what they really are, hypotheses.

Every theorem in geometry is a law of external nature, and could have been discovered by generalising from observation and experiment, which in this case come down to comparison and measurement. But it was found to be convenient and therefore desirable to deduce these truths by ratiocination from a small number of general laws of nature—the first principles and basic premises of the science—whose certainty and universality are obvious to the most casual observer. Among these general laws must be included the two that I have presented as basic principles of the science of number also, and are applicable to every sort of quantity. I mean

> •The sums of equals are equal, and
> •Things that are equal to the same thing are equal to one another;

the latter of which can be expressed in a way that more openly suggests the inexhaustible multitude of its consequences, namely:

> •Whatever is equal to any one of a number of equal magnitudes, is equal to any other of them.

For geometry we must add a third law of equality, namely:

> •Lines, surfaces, and solid spaces that can be applied to one another so that they coincide are equal.

Some writers have said that this law of nature is a mere verbal definition, that 'equal magnitudes' means nothing but 'magnitudes that can be applied to one another so that they coincide'. I don't agree. The *equality* of two geometrical magnitudes can't differ fundamentally in its nature from the *equality* of two weights, two degrees of heat, or two stretches of time, and the proposed definition of equality isn't suitable for any of these. None of these things can be 'applied to one another so that they coincide', yet we understand perfectly what we mean by calling them 'equal'. Things are equal in magnitude, as in weight, when they are felt [Mill's word] to be exactly similar in respect of the attribute in which we compare them. As for the application of lines etc. to each other in geometry, that's merely bringing them into a position in which our senses can recognise deficiencies of exact resemblance that would otherwise escape our notice. It's on a par with balancing objects in a pair of scales to determine whether their weights are equal.

Along with these three general principles or axioms, the other premises of geometry are the so-called definitions—i.e. propositions each of which •asserts the real existence of some object and •states some one property of it. In some cases more than one property is commonly assumed, but there's never a need for more than one. It is assumed that there are such things in nature as straight lines, and that any two of them setting out from the same point diverge more and more without limit. This assumption (which includes and goes beyond Euclid's axiom that *two straight lines can't enclose a space*) is as indispensable as any of the other axioms in geometry, and it's as evident as they are because like them it rests on a simple, familiar, and universal observation. It is also assumed that straight lines diverge from one another in different degrees, meaning that there are such things as angles and that they can be equal or unequal. It's assumed that there is such a thing as a circle, and that all its radii are equal; such things as ellipses, and that the sums of the focal distances are equal for every point in an ellipse; such things as parallel lines, and that those lines are everywhere equally distant.[1]

§8. It is a matter of more than curiosity to ask:

> What special feature of the physical truths that are the subject of geometry makes them all deducible from such a small number of original premises? Why it is that we can start with •one characteristic property of each kind of phenomenon and •two or three general truths relating to equality, and travel from mark to mark until we obtain a vast body of derivative truths that don't look a bit like those elementary ones?

The explanation of this remarkable fact seems to lie in the following ·two· facts. First, all questions of position

---

[1]   Geometers have usually preferred to define parallel lines by the property of *being in the same plane and never meeting*. But this has required them to assume as an additional axiom some other property of parallel lines; and the unsatisfactory way in which Euclid and others have selected properties for that purpose by has always been regarded as the disgrace of elementary geometry. Equidistance is a fitter property to characterise parallels by, even as a verbal definition, because it is the attribute really involved in the name's meaning. If all that is meant by 'x any y are parallel' were 'x and y are in the same plane and never meet', we would happily speak of a curve as 'parallel to' its asymptote [i.e. to a line that gets nearer to it *ad infinitum* but doesn't meet it]. The meaning of 'parallel lines' is 'lines that run in exactly the same direction and therefore don't become nearer or further from one another'—a conception immediately suggested by the contemplation of nature. That the lines •will never meet is of course included in the more comprehensive proposition that they •are everywhere equally distant. And that *any straight lines that are in the same plane and not equidistant will certainly meet* can be demonstrated in the most rigorous manner from the basic property of straight lines assumed in the text, namely that if they set out from the same point they diverge more and more without limit.

and figure can be resolved into questions of magnitude. The position and figure of any object are determined by determining the position of a sufficient number of points in it; and the position of any point can be determined by the magnitude. . . .of the perpendiculars drawn from the point to three planes at right angles to one another, arbitrarily selected. This transformation of all questions of quality into questions only of quantity turns geometry into the single problem of the measurement of magnitudes, i.e. the ascertaining of the equalities between them. Now remember that ascertaining any equality between x and y

- •proves (according to one of the general axioms) as many other equalities as there are other things equal to either x or y, and that
- •proves (according to another of the axioms) the equality of as many pairs of magnitudes as can be formed by the numerous operations that resolve themselves into the addition of x and y to one another or to other equals.

When we bear *that* in mind, we cease to be puzzled by the fact that •the more a science has to do with equality the more copious its supply of marks of marks, and that •the sciences of number and extension, which have to do with equality and little else, are the most deductive of all the sciences.

Secondly, two or three of the principal laws of space or extension are especially well fitted for making one position or magnitude a mark of another, thereby contributing to making the science largely deductive. •The magnitudes of enclosed spaces, whether in two or three dimensions, are completely determined by the magnitudes of the lines and angles that bound them. •The length of any line, straight or curved, is measured (certain other things being given) by the angle it subtends, and *vice versa*. •The angle that any two straight lines make with each other at an inaccessible

point is measured by the angles they separately make with any third line we choose to select. By means of these general laws, the measurement of *all* lines, angles, and spaces could be accomplished by measuring a single straight line and a large enough number of angles—which is what they actually do in making a trigonometrical survey of a country. It's lucky for us that this is practicable, because the exact measurement of long straight lines is always difficult and often impossible, whereas angles are easy to measure. Those three generalisations provide such facilities for indirectly measuring magnitudes (by supplying us with known lines or angles that are marks of the magnitude of unknown ones, and thereby of the spaces they enclose), that it's easy to understand how from a few data we can go on to ascertain the magnitude of indefinitely many lines, angles, and spaces that we couldn't easily measure—or couldn't measure at all—by any more direct process.

§9. I have said all I need to say here about the laws of nature that are the special subject of the sciences of number and extension. The immense part those laws play in giving a deductive character to the other branches of physical science is well known; and it's not surprising, when we consider that all causes operate according to mathematical laws. The effect is always dependent on—i.e. is a function of—the cause's quantity and generally of its position also. So we can't reason about causation without introducing considerations of quantity and extension at every step; and when the phenomena are such that we can get accurate enough numerical data, the laws of quantity become the grand instrument for calculating forward to an effect or backward to a cause. In all other sciences, as well as in geometry, questions of quality nearly always depend on questions of quantity, as can be seen in the most familiar

phenomena, even colour. When a painter mixes colours on his palette, the comparative quantity of each entirely determines the colour of the mixture.

[For further discussion of these matters Mill refers the reader to Comte's *Cours de Philosophie Positive*, which he also credits with a full discussion of Mill's next topic, namely:] the limits to how far mathematical principles can be used to improve other sciences. They obviously can't be used on classes of phenomena whose causes

- •are so little open to our observation that we can't ascertain their numerical laws by a proper induction; or
- •are so numerous and intermixed in such a complex way that even if their laws were known the computation of the over-all effect is beyond the powers of mathematics as it is or is likely to be; or
- •are themselves are in a state of perpetual fluctuation— as in physiology, and still more (if possible) in the social sciences.

The mathematical solutions of physical questions become progressively more difficult and imperfect in proportion as the questions lose their abstract and hypothetical character and come closer to the degree of complication actually existing in nature. [The quotations that follow are from Comte.] The result is that except for astronomical phenomena and those most nearly analogous to them, mathematical accuracy is generally obtained 'at the expense of the reality of the inquiry'; while even in astronomical questions, 'despite the admirable simplicity of their mathematical elements, our feeble intelligence becomes incapable of effectively following out

the logical combinations of the laws on which the phenomena depend, as soon as we try to take into consideration more than two or three essential influences at once'. A remarkable example of this is the *three-body problem* that I mentioned on page 228—a comparatively simple question the complete solution of which has defeated the skill of the most profound mathematicians. This shows us that mathematical principles can't be usefully applied to phenomena that depend on the mutual action of the innumerable minute particles of bodies, e.g. •chemistry, and still more •physiology. And for similar reasons those principles remain inapplicable to the still more complex inquiries into the phenomena of •society and government.

The value of mathematical instruction as a preparation for those more difficult investigations consists in the applicability not of its •doctrines but of its •method. Mathematics will always be the most perfect type of the deductive method in general; and the applications of mathematics to the deductive branches of physics provide the only classroom in which philosophers can effectively learn the most difficult and important part of their art, namely the use of the laws of simpler phenomena for explaining and predicting the laws of more complex ones. These grounds are quite sufficient for regarding mathematical training as an indispensable basis of real scientific education, and regarding (according to the dictum which an old but unauthentic tradition ascribes to Plato) one who is ignorant of mathematics as lacking in one of the most essential qualifications for successfully pursuing the higher branches of philosophy.

Chapter 25. The grounds of disbelief

§1. In the past 24 chapters I have discussed—as far as space and my abilities permitted—the method of arriving at general truths (i.e. general propositions fit to be believed) and the nature of the evidence they are based on. But an examination of evidence doesn't always produce belief, or even suspension of judgment; it sometimes produces disbelief. So a complete philosophy of induction and experimental inquiry must treat the grounds not only of belief but also of disbelief. I'll devote my final chapter to that.

By 'disbelief' I don't mere absence of belief. The ground for abstaining from belief is simply the absence or insufficiency of proof; and in considering what *is* sufficient evidence to support a conclusion I have already implicitly considered what evidence *is not* sufficient for the same purpose. By 'disbelief' I mean the state of mind in which we are fully convinced that some opinion is *not true*; so that if evidence— even apparently strong evidence—were produced in favour of the opinion, we would believe that the witnesses spoke falsely, or that they or we ourselves (if we were the direct percipients) were mistaken.

No-one is likely to deny that there are such cases. Assertions for which there is abundant positive evidence are often disbelieved because of what is called their 'improbability' or 'impossibility'. The question we have to think about is: 'What do those two words mean in this context? And how far and in what circumstances do the properties they express give sufficient grounds for disbelief?'

§2. When positive evidence produced in support of an assertion is rejected because it is impossible or improbable, it never amounts to full proof. It is always based on some approximate generalisation. The claim may have been asserted by a hundred witnesses, but the thesis that *whatever a hundred witnesses affirm is true* has many exceptions. We may seem to ourselves to have actually seen the fact, but the thesis that *we really see what we think we see* is far from being a universal truth—our sense-organs may have been diseased, or we may have •inferred something and imagined that we •perceived it. Thus, given that the evidence for the affirmative is never more than an approximate generalisation, everything will depend on what the evidence is for the negative. If that also rests on an approximate generalisation, this is a case for comparison of probabilities. If the approximate generalisations leading to the affirmative add up to something less strong—i.e. further from being universal—than the approximate generalisations that support the negative side of the question, the proposition is said to be 'improbable' and is to be disbelieved *provisionally*. But when an alleged fact contradicts (not any number of approximate generalisations, but) a completed generalisation based on a rigorous induction, it is said to be 'impossible' and is to be disbelieved *totally* [here = 'unconditionally'].

This last principle, simple and evident as it appears, aroused a violent controversy on the occasion of an attempt to apply it to the question of the credibility of miracles. Hume's celebrated doctrine that *nothing is credible that is contradictory to experience or at variance with laws of nature* is merely the plain and harmless proposition that *whatever is contradictory to a complete induction is incredible*. That such a maxim as this should be accounted •a dangerous heresy or •a great and recondite truth speaks ill for the state of philosophical theorising on such subjects!

You may want to ask:

> Doesn't the very statement of the proposition imply a contradiction? An alleged fact, according to this theory, is not to be believed if it contradicts a complete induction. But a complete induction mustn't contradict any known fact. So isn't it a *petitio principii* [see Glossary] to say that the fact ought to be disbelieved because the induction opposed to it is complete? How can we have a right to declare the induction complete when facts supported by credible evidence present themselves in opposition to it?

We do have that right whenever the scientific canons of induction give it to us, i.e. whenever the induction can be complete. We have it, for example, in a case of causation where there has been a decisive experiment. If A is added to a set of antecedents that hasn't been followed by B, and B does now follow, then in that instance A is B's cause or an indispensable part of its cause; and if A is tried again with many *different* sets of antecedents and B still follows, then it is the whole cause. (In each case it is of course essential that adding A to a set of antecedents doesn't change the set in any other way.) If these observations or experiments are repeated often enough, and by enough people, to exclude any suspicion of error in the observer, a *law of nature* is established; and as long as this law is accepted as such, the assertion that on some particular occasion

> A occurred and B didn't follow, *though there was no counteracting cause*

must be disbelieved. Such an assertion shouldn't be credited on any evidence short of what would suffice to overturn the law. The general truths that

> •Whatever has a beginning has a cause, and
> •When none but the same causes exist, the same effects follow,

rest on the strongest inductive evidence possible; whereas the proposition that

> •Things affirmed by a crowd of respectable witnesses are true

is only an approximate generalisation; and—even if we fancy we actually saw or felt whatever-it-was that contradicts the law—what a human being can see is merely a set of appearances, from which the real nature of the phenomenon is merely an inference, and such inferences usually make heavy use of approximate generalisations. So if we decide to hold by the law, *no* amount of evidence ought to persuade us that something that contradicts it has happened. If the evidence E that is produced makes it more likely that •the observations and experiments the law is based on were inaccurately performed or incorrectly interpreted than that •E is false, we may believe the evidence; but then we must abandon the law. And since the law had been accepted on the basis of what seemed to be a complete induction, it can only be rejected on evidence equivalent to that—i.e. as being inconsistent not with •any number of approximate generalisations but with •some other and better established law of nature. The extreme case of a conflict between two supposed laws of nature has probably never actually occurred in contexts where each 'law' was investigated according to the true canons of scientific induction; but if it did occur, it would have to lead to the total rejection of one of the 'laws'. It would prove that there's a flaw in the logical process by which one or other of the 'laws' was established, showing that that supposed general truth is no truth at all. We can't admit a proposition as a law of nature while believing something that contradicts it. We must disbelieve the alleged fact, or believe that we were mistaken in accepting the supposed law.

For an alleged fact to contradict a law of causation, the allegation must be. . . .that this happened *in the absence of any adequate counteracting cause*. Now, in the case of an alleged miracle, the assertion is the exact opposite of this. It is that the effect was defeated not in •the absence of a counteracting cause but in •consequence of one, namely, an. . . .act of the will of some being who has power over nature; and in particular of a Being whose will is assumed to have given all the causes their causal powers and can therefore easily be supposed to be able to counteract them. As Thomas Brown rightly said in his *Inquiry into the Relation of Cause and Effect*, a miracle doesn't contradict the law of cause and effect; it is a •new effect that is supposed to be produced by the introduction of a •new cause. There can be no doubt that this cause, if present, is adequate to do the job; the only antecedent improbability that can be ascribed to the miracle is the improbability that any such cause exists.

So all that Hume has shown—and this he must be credited with showing—that no evidence can prove a miracle to anyone who

> •doesn't already believe in the existence of one or more beings with supernatural power; or
> •believes he has full proof that the character of the Being whom he recognises is inconsistent with His having interfered on the occasion in question.

[Mill builds into his statement of what Hume showed the proviso 'at least in the imperfect state of our knowledge of natural agencies, which leaves it always possible that some of the physical antecedents may have been hidden from us'. It's not obvious how this fits in, and Mill doesn't explain it.]

If we don't already believe in supernatural agencies, no miracle can prove their existence to us. That *the supposed miracle actually occurred*, considered merely as an extraordinary fact, can be satisfactorily certified by our senses or by testimony; but nothing can ever prove that *it was a miracle*, because there's always the rival hypothesis that it was a result of some unknown natural cause; and this possibility can't be shut out so completely that the only alternative remaining is to admit the existence and intervention of a being superior to nature. Those who already believe in such a being have two hypotheses to choose from, a •supernatural agency and an unknown •natural agency, and they have to judge which of the two is more probable in the particular case. In working towards a judgment about this they'll have to think about whether it would be in character for the Deity, as they conceive him, to have caused this particular event. But with the knowledge we now have of the general uniformity of the course of nature, religion has been compelled to follow in the wake of science by acknowledging that the over-all government of the universe is carried on by general laws and not by special interpositions. For anyone who holds *this* belief there's a general presumption against any supposition of divine agency not operating through general laws. In other words, for such a person there's an antecedent improbability in every miracle—an improbability that could be outweighed only by an extraordinarily strong antecedent probability based on the special features of the case.

§3. So the assertion that *a cause has failed to produce an effect that is connected with it by a completely ascertained law of causation* is to be disbelieved or not according to the probability or improbability that this particular instance contained an adequate counteracting cause. To estimate this isn't harder than estimating other probabilities. With regard to all known causes that could counteract the given causes we usually have some previous knowledge of how often they occur, from which we can infer the antecedent improbability of their having been present in any particular case. And

with known or unknown causes we don't have to pronounce on the probability of their existing in nature, but only the probability of their having existed at the time and place at which the miracle is alleged to have happened. We usually have the means (when the circumstances of the case are at all known to us) of judging how likely it is that such a cause existed at that time and place •without showing its presence by some other marks and (in the case of an unknown cause) •without having shown its existence ever before. . . .

So much for the case where the alleged fact conflicts, or appears to conflict, with a real law of causation. A more common case, perhaps, is that of its conflicting with •mere uniformities of coexistence that aren't proved to depend on causation, i.e. with •the properties of Kinds. It is with these uniformities that travelers' marvellous stories are apt to conflict—e.g. tales of men with tails or with wings, and (until confirmed by experience) of flying fish; or of ice, in the famous anecdote of the Dutch travelers and the King of Siam. Facts of this description—facts that haven't previously been heard of, but that no known law of causation implies to be impossible, are what Hume characterises as not •contrary to experience but merely •unconformable to it. . . .

In a case of this sort, the fact asserted is the existence of a new Kind. This in itself is not in the least incredible, and should be rejected only if the improbability

> that any sort of object existing at that particular place and time should have gone undiscovered until now

is greater than the improbability

> that the witnesses were mistaken or lied.

Accordingly, when such assertions are made by credible persons and concern unexplored places, they aren't disbelieved but only regarded as requiring confirmation from subsequent observers—unless the alleged properties of the supposed new Kind conflict with known properties of some larger Kind that

includes it. . . .as in the case of Pliny's men, or any other kind of animal with a structure different from what has always been found to coexist with animal life. As for how to deal such a case, I needn't add much to what I said in chapter 22 (pages 300–301). When the uniformities of coexistence that the alleged fact would violate are such as to raise a strong presumption of their being the result of causation, the fact that conflicts with them should be disbelieved—at least provisionally, subject to further investigation. When the presumption amounts to a virtual certainty, as with the general structure of organisms, all we have to ask is this: 'In phenomena as little understood as this. . .

> . . . mightn't there be at work a counteracting cause that we haven't known about before? or
> . . . mightn't the phenomena be capable of originating in some other way that would produce a different set of derivative uniformities?'

In some cases neither of those suppositions can be regarded as very improbable, because the generalisation to which the alleged fact would be an exception is very special and of limited range. Examples are the reports about •flying fish and about •the ornithorhynchus [= the platypus, an egg-laying, venomous, duck-billed, beaver-tailed, otter-footed mammal found only in Australia]. Faced with reports of such alleged anomalies, it is wise to suspend our judgment pending the subsequent inquiries that are sure to confirm the assertion if it is true. But when the generalisation is very comprehensive, taking in a vast number and variety of observations and covering a considerable province of nature's domain, then for reasons that I have fully explained such an •empirical law comes near to the certainty of an ascertained •law of causation; and alleged exceptions to it ought not to be accepted except on the evidence of some law of causation that is proved by a still more complete induction.

Uniformities in the course of nature that don't look like results of causation are, as I have shown, admissible as universal truths with a degree of belief proportioned to their generality. Those that are true of all things whatever, or at least are totally independent of the varieties of Kinds—namely the laws of number and extension, to which we may add the law of causation itself—are probably the only ones an exception to which is absolutely and permanently incredible. Accordingly, the word 'impossible' (or anyway 'totally impossible') seems usually to be confined to assertions regarded as contradictory to these laws or to others coming near to them in generality. Violations of other laws—of special laws of causation, for instance—are said by people who care about accuracy in speech to be 'impossible in the circumstances of the case' or 'impossible except where there's a cause that didn't exist in the particular case'.[1] If a cautious person is faced with an assertion that doesn't contradict any of these very general laws, he won't go further than to call it 'improbable'; and he won't mean 'improbable in the highest degree' unless the time and place in which the fact is said to have occurred make it almost certain that the anomaly, if real, couldn't have been overlooked by other observers. In any other case the judicious inquirer will avail himself of *suspense of judgment*, provided the testimony in favour of the anomaly presents, when well sifted, no suspicious circumstances.

The testimony hardly ever survives such a test in cases where the anomaly is not real. In the instances on record in which many witnesses of good reputation and scientific acquirements have testified to the truth of something that then turned out to be untrue there have almost always been details that would have made the testimony untrustworthy to a keen observer who had taken the trouble to sift the matter. There have generally been ways to explain the impression on the senses or minds of the alleged percipients, in terms of
> •fallacious appearances, or
> •some epidemic delusion propagated by the contagious influence of popular feeling, or
> •some strong interest—religious zeal, party feeling, vanity, or at least the passion for the marvellous.

When nothing like that can account for the apparent strength of the testimony; and where the assertion
> doesn't contradict either •the universal laws that know no counteraction or anomaly or •the generalisations just below them in comprehensiveness,

but only
> implies the existence of an unknown cause or an anomalous Kind, in circumstances where it is credible that hitherto unknown things may still come to light,

a cautious person will neither admit nor reject the testimony, but will wait for confirmation at other times and from other unconnected sources. That's what the King of Siam should have done when the Dutch travellers told him about ice. But an ignorant person is as obstinate in his contemptuous incredulity as he is unreasonably credulous. Anything unlike his own narrow experience he disbelieves if it doesn't answer

---

[1] One writer. . . .defines 'an impossibility' as 'that which there exists in the world no cause adequate to produce'. This definition doesn't take in such impossibilities as that two and two should make five, that two straight lines should enclose a space, or that anything should begin to exist without a cause. I can't think of any definition of 'impossibility' broad enough to include all its varieties, except the one I have given: An impossibility is something whose truth would conflict with a complete induction, i.e. with the most conclusive evidence we have of universal truth.
—As for the reputed impossibilities that rest purely on our ignorance of any cause that could produce the supposed effects: very few of them are certainly impossible or permanently incredible. The facts of travelling at 70 mph, painless surgical operations, and conversing by instantaneous signals between London and New York held a high place among such impossibilities not many years ago.

to his needs or tastes; any nursery tale is swallowed implicitly by him if it does.

§4. I now come to a very serious misunderstanding of the principles of this subject that has been committed by some writers against Hume's 'Essay on Miracles' and by Bishop Butler before them, in their anxiety to destroy what they saw as attack-weapon against the Christian religion. It has the effect of totally confusing the doctrine of the grounds of disbelief. The mistake consists in overlooking the distinction between . . . .the improbability that a mere guess is right and the improbability of an alleged fact being true. [The ellipsis in that sentence replaces '. . . (what may be called) improbability before the fact and improbability after it, or (since, as Venn remarks, the distinction of past and future is not the material circumstance) between. . . '.]

Many events that are altogether improbable to us before they have happened or before we're informed of their happening are perfectly credible when we are informed of them, because they aren't contrary to any induction, even an approximate one. In the throw of a perfectly fair die, the chances are 5:1 against throwing 4; that is, 4 will be thrown on an average only once in six throws. But this is no reason against believing that ace was thrown on a given occasion if any credible witness asserts it. It's true that 4 is thrown only once in six times, but if the die is thrown at all it must throw *some* number that is thrown only once in six times. The improbability (i.e. the unusualness) of any fact is no reason for disbelieving it if the situation makes it certain that either that or something equally improbable (i.e. equally unusual) did happen. Furthermore, even if the other five sides of the die are all 2s, still 4 would on the average come up once in every six throws, its coming up in a given throw would not in any way contradict experience. If we disbelieved all facts that had the chances against them beforehand, we would

believe hardly anything. We are told that John Doe died yesterday; the moment before we were told this the chances against his having died on that day may have been 10,000:1; but since he was certain to die at *some* time, and when he died it had to happen on some particular day, experience gives us no basis for discrediting any testimony that may be produced to the event's having occurred on 26.v.1872. The odds were against its happening on that day in particular, but only because they were against John Doe's dying on day n for *any* value of n.

Yet George Campbell and others have offered as a complete answer to Hume's doctrine that

> things that are **contrary to** the uniform course of experience are incredible

the undisputed fact that we don't disbelieve something that is **in strict conformity with** the uniform course of experience merely because the chances were against it; we don't disbelieve an alleged fact merely because the combination of causes it depends on occurs only very infrequently. It's obvious that whatever is shown by observation, or can be proved from laws of nature, to occur in a certain proportion (however small) of the whole number of possible cases is not contrary to experience; though we are right in disbelieving it if some other supposition regarding the matter in question takes us less far from the ordinary course of events. Yet on such grounds as this able writers have been led to the extraordinary conclusion that nothing supported by credible testimony ought ever to be disbelieved.

§5. I have considered two sorts of events that are commonly said to be improbable: one sort that are in no way extraordinary, but have an immense preponderance of chances against them and are therefore improbable until they are affirmed, but no longer; and another sort that are contrary

to some recognised law of nature and are therefore incredible on any amount of testimony except such as would shake our belief in the law ·of causation· itself. But there's also an intermediate class of events, consisting of what are commonly called 'coincidences'—in other words, combinations of chances that present some special and unexpected regularity that makes them look like the results of law. An example would be, in a lottery with a thousand tickets, the numbers being drawn in the exact order 1, 2, 3, etc. We haven't yet considered the principles of evidence that apply to this case—whether coincidences differ from ordinary events in the amount of testimony or other evidence necessary to make them credible.

It is certain that on every rational principle of expectation, a combination of this special sort may be expected quite as often as any other given series of a thousand numbers; that with perfectly fair dice, sixes will be thrown n times in succession (for any n) quite as often in a thousand or a million throws as any other succession of numbers fixed upon beforehand, and that no judicious player would give greater odds against the one series than against the other. [He means that the odds against throwing (for example) 6 6 6 6 are no greater than the odds against throwing 7 2 1 4 or 5 6 1 9 or... etc.] Yet there's a general disposition to regard the one as much more improbable than the other, and as needing much stronger evidence to make it credible. This impression is so strong that it has led some thinkers to conclude that nature finds it harder to produce regular combinations than to produce irregular ones—i.e. that there's some general tendency in things, some *law*, that prevents regular combinations from occurring as often as others. These thinkers include Jean D'Alembert, who in an essay on probabilities contends that regular combinations, though equally probable according to the •mathematical theory with any others, are •physically

less probable. He appeals to common sense, i.e. to common impressions, saying that if a die thrown repeatedly in our presence gave sixes every time, before there had been ten throws (let alone thousands of millions) we would be absolutely sure that the die was loaded.

The common and natural impression is in favour of D'Alembert; the regular series would be thought much more unlikely than an irregular one. But this common impression is merely based on the fact that scarcely anyone remembers having ever seen one of these conspicuous coincidences. Why is that? It's simply because no-one's experience extends to anything like the number of trials within which that or any other given combination of events can be expected to happen. The chance of sixes on a single throw of two dice being $1/36$, the chance of sixes ten times in succession is $1/36^{10}$, which is to say that such an concurrence is only likely to happen once in $3, 656, 158, 440, 062, 976$ trials, a number that no dice-player's experience comes up to a millionth part of. But if instead of sixes ten times some other given succession of ten throws had been fixed upon, it would have been exactly as unlikely that in any individual's experience *that* particular succession had ever occurred; although this doesn't *seem* equally improbable, because no-one would be likely to have remembered whether it had occurred or not, and because the comparison is tacitly made not between •sixes ten times and •any other particular series of ten throws, but between all regular successions and all irregular ones taken together.

D'Alembert is unquestionably right in saying that if the succession of sixes was actually thrown before our eyes we would ascribe it not to chance but to unfairness in the dice. But this arises from a totally different principle. What we should be asking is not

> How probable was it that sixes would be thrown ten times in a row?

but rather

>Given our knowledge that this did happen, how probable is it that the cause was C_1? C_2?....etc.

The regular series is as likely as the irregular one to be brought about by chance, but it is much more likely than the irregular one to be produced by design or by some general cause operating through the structure of the dice. It is the nature of casual combinations to produce a repetition of the same outcome

>**as often as any other series of outcomes**, and no oftener.

It is the nature of general causes to produce the same outcome

>in the same circumstances, **always**.

Common sense and science alike dictate that other things being equal we should attribute the effect to •a cause which if real would be very likely to produce it rather than to •a cause that would be very unlikely to produce it. According to Laplace's sixth theorem, which I demonstrated in chapter 18.5 (page 275, the difference of probability arising from the greater *efficacy* of the constant cause, namely unfairness in the dice, would after a very few throws far outweigh any antecedent probability there could be against its existence.

D'Alembert should have put the question differently. He should have supposed that we had ourselves previously tested the dice, and knew by ample experience that they were fair. Another person then tries them in our absence, and assures us that he threw sixes ten times in succession. Is the assertion credible or not? Here the effect to be accounted for is not •the occurrence itself, but •the fact of the witness's asserting it. This may arise either from its having really happened or from some other cause. What we have to estimate is the comparative probability of these two suppositions.

If this witness had reported having thrown some other series of ten numbers, assuring us that he took particular notice of the outcome of each throw, and if we regard him as generally truthful and careful, we would believe him. But the ten sixes are exactly as likely to have been really thrown as the ten other numbers, ·whatever they are·. So if the report **(i)** 'I threw ten sixes in a row' is less credible than **(ii)** 'I threw the following ten-member sequence of numbers...' etc., the reason must be not that **(i)** is less likely than **(ii)** to be said truly but that it is more likely than **(ii)** to be said falsely.

One reason obviously presents itself why 'coincidences' are asserted falsely more often than ordinary combinations are. The coincidence arouses wonder. It gratifies the love of the marvellous. So the motives to lie—one of the most frequent of which is the desire to astonish—operate more strongly in favour of this kind of assertion than of the other kind. To that extent there's clearly more reason to discredit an alleged coincidence than to discredit a statement which isn't in itself more probable but which if it were made would not be thought remarkable. Sometimes, however, the presumption on this ground would be the other way. There are some witnesses who, the more extraordinary an occurrence might appear, would be the more anxious to check it with utmost care before venturing to believe it, and still more before asserting it to others.

§6. Laplace contends that a coincidence is not credible on the same amount of testimony as would justify us in believing an ordinary combination of events; and he bases this merely on the general ground that testimony is fallible, quite apart from any special chances of lying because of the nature of the assertion. To do justice to his argument I'll need to illustrate it by the example chosen by himself.

If, says Laplace, there were 1000 ·numbered· tickets in a box, and one has been drawn out, then if an eye-witness says that the number drawn was 79 we find this credible even though the chances against it were 999:1. Its credibility is equal to the antecedent probability of the witness's veracity. But if there were in the box 999 black balls and only one white, and the witness reports that the white ball was drawn, the case (according to Laplace) is very different—the credibility of his assertion is only a small fraction of what it was in the previous case. Laplace's account of why occupies the next paragraph.

The nature of the case requires that the credibility these witnesses falls materially short of certainty. Let us suppose, then, that the credibility of the witness in the case we are considering is $9/10$—that is, let us suppose that in every ten statements the witness makes, nine on an average are correct and one incorrect. Let us now suppose that there have been enough drawings to exhaust all the possible combinations, with our witness reporting on each outcome. In one case out of every ten in all these drawings he will have made a false announcement. But in the case of the thousand tickets, these false announcements will have been distributed impartially over all the numbers, and of the 999 cases in which 79 was not drawn, there will have been only one case in which it was announced. On the other hand, in the case of the thousand balls (the announcement being always either 'black' or 'white'), if white wasn't drawn and there was a false announcement, that false announcement must have been 'white'; and since by the supposition there was a false announcement once in every ten times, 'white'

will have been announced falsely in one-tenth of all the cases in which it wasn't drawn, i.e. one-tenth of 999 cases out of every thousand. White, then, is drawn on an average exactly as often as ticket 79, but it is announced without having been really drawn 999 times as often as ticket 79; so the announcement requires much more testimony to make it credible.[1]

To make this argument valid we must suppose that the witness's reports are average specimens of his general veracity and accuracy; or at least that they are neither more nor less so in the case of the black and white balls than in the case of the thousand tickets. But this assumption is not justified. A person is far less likely to go wrong if he has only one form of error to guard against than if he has 999 different errors to avoid. For instance, a messenger who might make a mistake once in ten times in reporting •the number drawn in a lottery might not err once in a thousand times if sent simply to observe •whether a ball was black or white. Laplace's argument, therefore, is faulty even as applied to his own case. And that case is far from adequate as a stand-in for all cases of coincidence. Laplace has so contrived his example that though black answers to 999 distinct possibilities and white only to one, the witness has no bias that can make him prefer black to white. The witness didn't know that there were 999 black balls in the box and only one white; or if he did, Laplace has taken care to make all the 999 cases so alike that any cause of falsehood or error operating in favour of any of them would almost certainly operate in the same way if there were only one. Alter this supposition, and the whole argument falls to the ground. Let

---

[1] But not 999 times as much testimony, as you might think. A complete analysis of the cases shows that (always assuming the veracity of the witness to be $9/10$) in 10,000 drawings the drawing of ticket 79 will occur nine times and be announced incorrectly once; so the credibility of the announcement of ticket 79 is $9/10$; while the drawing of a white ball will occur nine times, and be announced incorrectly 999 times. So the credibility of the announcement of white is $9/1008$, which makes it only about 100 times more credible than the other, not 999 times.

the balls, for instance, be numbered, and let the white ball be 79. Considered in respect of their **colour**, there are only **two** things that the witness •can be interested in asserting, or •can have dreamed or hallucinated, or •has to choose from if he answers at random, namely black and white; but considered in respect of the **numbers** attached to them, there are **a thousand**; and if his interest or error happens to be connected with the numbers, though the only assertion he makes is about the colour, the case becomes precisely assimilated to that of the thousand tickets. Or instead of the balls suppose a lottery with 1000 tickets and only one prize, and that I hold ticket 79; because that's all I am interested in, I ask the witness not 'What number was drawn?' but 'Was ticket 79 drawn?' There are now only two cases, as in Laplace's example; but surely he wouldn't say that if the witness answered '79', the assertion would be enormously less credible than if he gave the same answer to the same question asked in the other way. . . .

Suppose a regiment of 1000 men, 999 Englishmen and one Frenchman, and that one of these has been killed and I don't know which. I ask the question and the witness answers 'It was the Frenchman'. This was as improbable *a priori* as the drawing of the white ball, and is also as striking a coincidence as that. But we would believe it as readily as if the answer had been 'It was John Thompson'. The 999 Englishmen were all alike in the respect in which they differed from the Frenchman, but they weren't indistinguishable in every other respect, as the 999 black balls were; and because they were all different there were as many chances of interest or error regarding them as if each man had been of a different nation; and if a lie was told or a mistake made, the misstatement was as likely to fall on any Jones or Thompson of the set as on the Frenchman.

D'Alembert's example of a coincidence—sixes thrown on a pair of dice ten times in succession—belongs to this sort of case rather than to ones like Laplace's. The coincidence here is much more remarkable, because of far rarer occurrence, than the drawing of the white ball. But though the improbability of its really occurring is greater, the greater probability of its being announced falsely can't be established with the same evidentness. The announcement 'black' represented 999 cases, but the witness may not have known this, and even if he did, the 999 cases are so exactly alike that there's really only one set of possible causes of mendacity corresponding to the whole. The announcement 'sixes not drawn ten times,' represents, and is known by the witness to represent, a great multitude of contingencies every one of which is unlike every other, so that there can be a different and a fresh set of causes of mendacity corresponding to each.

It appears to me therefore that Laplace's doctrine is not strictly true of any coincidences, and is thoroughly false of most; and that to know whether a coincidence needs more evidence to make it credible than an ordinary event, we must refer in every instance to first principles, and estimate afresh what the probability is that the given testimony would have been given in that instance if the fact it asserts isn't true.

With those remarks I close the discussion of the grounds of disbelief and, along with it, as much exposition of the logic of induction as space admits and I have it in my power to provide.