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

[Brackets] enclose editorial explanations. Small ·dots· enclose material that has been added, but can be read as though it were part of the original text. Occasional •bullets, and also indenting of passages that are not quotations, are meant as aids to grasping the structure of a sentence or a thought. Every four-point ellipsis . . . . indicates the omission of a brief passage that seems to present more difficulty than it is worth. Longer omissions are reported between brackets in normal-sized type.

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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**.

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 successive impressions—and the law that makes associations stronger in proportion to the pleasurable or painful character 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 reasoning. . . .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

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]

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[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

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[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

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[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

252

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,

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[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

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[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

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[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-

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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 off-spring;
> •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

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[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

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[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 $1/216. \ldots$

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 percep-
> tible, 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

286

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

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[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 circumstances.

With that in mind, now consider the case of a generalisation 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 concerning 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 estimate *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

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[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

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[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.

305

§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

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[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 un-
> less 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 un-
> less 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

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

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[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

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[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.

321

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 as-serted 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!

322

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

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[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

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[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.