# 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. Three-point ellipses ... indicate omissions by Mill in passages he quotes from others.

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# Glossary

**agree:** When Mill speaks of the respects in which two things 'agree', he means the respects in which they 'are alike'. In this version, 'agree' will be allowed to stand sometimes, but it will often be replaced, for aesthetic reasons, by the language of 'likeness' and 'resemblance'. Mill also speaks of a thing as 'agreeing' with a general conception, and he clearly means this to be a comparison also; in the present version he often says instead that the object 'fits' the conception.

**art:** Any practical activity that is governed by rules and (same thing?) requires skill. Mill's 'science or art' might be illustrated by 'physics or engineering'.

**circumstance:** Mill often uses this word as we do, but he also has a different use: the 'circumstances' of an event or state of affairs may be its features, details, facts about it. In those uses the present version usually replaces the word by something more familiar, but sometimes—e.g. in the opening paragraphs of chapter 5—there's no way of doing that.

**colligation:** Collecting, gathering together, bundling.

**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.

**definite distinction:** A distinction that can be captured in a definition. It ought also to be 'definite' in our ordinary sense—clear, sharp, unambiguous etc.—but the emphasis is on definability (unlike, say, the distinction between cows and horses).

**denote:** Refer to. The phrase 'Raoul Wallenberg' denotes a certain man; so does 'that man standing near the window' when said by someone as he points to a man standing by a window; and the abstract noun 'humanity' denotes a certain property.

**disinterested:** What this has meant for centuries, and still means when used by literate people, is 'not *self*-interested'.

**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.

**fact:** Mill uses this word rather loosely, variously meaning 'state of affairs', 'alleged state of affairs', and 'proposition asserting the existence of a state of affairs'.

**frame:** To 'frame' a class or a conception is to create it, set it up, give it its conceptual structure.

**oblate spheroid:** A slightly flattened globe.

**proximate:** Next. The 'proximate natural group' relative to class C is the larger group, *one step up*, that contains C.

**question-begging:** Until fairly recently, to 'beg the question' was to offer a 'proof' of P from premises that include P. It now means 'raise the question'. It seems that illiterate journalists (of whom there are many) encountered the phrase, liked it, guessed at its meaning, and saw no reason to check on the guess. For Mill's lovely statement about this, see the indented passage on page 358.

**type:** 'The general form, structure or character distinguishing a particular group or class of things' (OED).

**vulgar:** Ordinary not very intelligent and not very educated people. On page 346 Mill says that it includes 'all who have not accurate habits of thought'.

# Book IV: Operations Subsidiary to Induction

## Chapter 1. Observation and description

**§1.** The inquiry that occupied us in Books II and III has led us to an apparently satisfactory solution of Logic's chief problem, according to my conception of Logic. We have found that the mental process that Logic involves—the operation of ascertaining truths by means of evidence—is always a process of induction, even when appearances point to a different theory of it. And we have looked separately at the various types of induction, and obtained a clear view of the principles that it must obey if it is to lead to reliable results.

But there's more to induction than the direct rules for performing it; something also has to be said about other mental operations that are either •presupposed in all induction or •instrumental in the more difficult and complicated inductive processes. The present Book will examines these subsidiary operations, starting with the ones that are indispensable preliminaries to *all* induction.

Induction is simply this:

> extending to a class of cases something that has been •observed to be true in some members of the class.

So •observation is the first subsidiary operation to be looked at. But we shan't be laying down rules for observing well; those are within the province not of logic but of intellectual education. Where observation connects with logic is in the evaluation of evidence. We shan't be asking •How are we to observe? or •What should we observe? but rather •When is observation to be relied on? What is needed for a report of a supposed observation to be safely accepted as true?

**§2.** The answer to this question is very simple, at least in its first aspect [Mill's phrase]. The only condition is that what is supposed to have been observed shall really have been *observed*, i.e. that it was an observation and not an inference. In almost every act of our perceiving faculties, observation and inference are intimately blended. What we're said to observe is usually a compound result, sometimes one-tenth observation and nine-tenths inference.

. . . .I affirm that *I saw my brother at 9 am this morning*. Isn't this something that I know through the direct testimony of my senses? The ordinary answer would be a confident 'Yes!', but in fact that answer is wrong. All I saw was a certain coloured surface; or rather I had visual sensations of a kind that is usually produced by a coloured surface; and on the basis of my previous experience of such sensations I concluded that I saw my brother. I could have had sensations just like those when my brother wasn't there—perhaps seeing some other person very like him. I might have been asleep and have dreamed that I saw him; or in a state of nervous disorder that brought his image before me in a waking hallucination. In all these ways many people *have* been led to believe that they saw familiar friends or relatives who weren't actually these. If any of these suppositions had been true, the statement that *I saw my brother* would have been false, though the direct perception—the visual sensations—would have been real. The only trouble would have been that my inference was ill-grounded; I would have assigned a wrong cause for those sensations.

Countless instances might be given, and analysed in the same manner, of what are vulgarly [see Glossary] called 'errors of sense'. None of them are strictly errors of sense—they are

erroneous inferences from sense. When I look at a candle through a multiplying glass, I seem to see a dozen candles instead of one; and if the set-up were skillfully disguised, I might suppose that there were really that number—that would be what is called an 'optical deception'. In the kaleidoscope there really is that deception; when I look through the instrument, the appearance presented to me is not •a casual arrangement of coloured fragments but rather •a single combination of coloured bits repeated several times in a symmetrical arrangement around a point. The delusion is caused by my having the same sensations that I *would* have had if such a symmetrical combination had really been presented to me. If I cross two of my fingers and bring a marble (say) into contact with both at points that aren't usually touched simultaneously by one object, I can hardly help believing—if my eyes are shut—that there are two marbles rather than one. But what gets deceived is not my sense of touch (in this case) or of sight (in the other), but rather my judgment, and that's true even if the deception is only momentary. All I get from my senses are the sensations, and *they* are genuine. I have been accustomed to having sensations like those whenever a certain arrangement of outer objects is present to my sense-organs, and not at any other time; so I've formed the habit of instantly inferring the existence of that state of outer things whenever I experience such sensations. This habit has become so powerful that the •inference, performed with the speed and certainty of an instinct, is taken to be an intuitive •perception. When its conclusion is correct, I'm not aware that it ever needed proof; and even when I know that it's incorrect, it's quite difficult for me to abstain from conducting the inference that leads to it. To become aware that the judgment in question is made not by instinct but by an acquired habit, I have to think about •the slow process through which I learned to judge by the eye concerning many things that I now appear to perceive directly by sight; and about •the reverse process that persons learning to draw go through—the difficult and laborious task of shedding their acquired perceptions and learning afresh to see things as they appear to the eye.

From these examples (and I could give ever so many more) we can see that the individual facts from which we derive our inductive generalisations are hardly ever obtained by observation alone. Observation extends only to **the sensations by which we recognise objects**; but the propositions that we make use of in science or in common life mostly relate to **the objects themselves**. In every act of 'observation' there's at least one inference—from the sensations to the presence of the object, i.e. from the marks or pointers to the entire phenomenon. That leads to a seeming paradox:

> A general proposition derived from particulars is often more certainly true than any one of the particular propositions that it was inferred from by an act of induction.

Each of those particular propositions involved an inference from •the impression on the senses to •the fact that caused this impression; and this inference may have been wrong in any one of the instances, but can hardly have been wrong in all of them, provided there were enough of them to eliminate chance. It follows that the general proposition may deserve more complete reliance than could safely be given to any one of the inductive premises.

So all there is to the logic of observation is a correct discrimination between what is really perceived and what is inferred from that. The inferential part of this falls under the rules of induction which I have already discussed and needn't go into again. What I do have to confront is this question: 'When all the inference is taken away what remains?' Well, there's this:

> the mind's own feelings or states of consciousness—its outward feelings or sensations and its inward feelings—its thoughts, emotions, and volitions.

Another question arises: 'Is that *all* that remains? Is that the sole basis for our inferences to other things? Or is our mind capable of directly perceiving or grasping something other than the states of its own consciousness?' That is a problem of metaphysics that won't be discussed here. But after setting aside all questions on which metaphysicians differ, it remains true that for most purposes what we need in practice is to distinguish •sensations or other feelings (ours or those of others) from •inferences drawn from them. And that seems to be all that needs to be said in the present work regarding the theory of observation.

**§3.** If in the simplest cases of what we ordinarily count as 'observation' there's a large part that is not observation but something else, so also in the simplest description of an observation there must always be much more asserted than is contained in the perception itself. [The last 10 words are verbatim from Mill; they are imperfect, but what he is deriving at becomes clear right away:] We can't describe a fact [see Glossary] without implying more than the fact. The perception is only of one individual thing, but describing it involves affirming a connection between it and every other thing that is either *denoted* or *connoted* by any of the terms used [see Glossary for those two words]. There couldn't be a more elementary example, than this: I have a visual sensation which I try to describe by saying 'I see something white'. In saying this I don't merely •affirm my sensation—I also •classify it. I assert that the thing I see resembles and all the things that I and others are accustomed to calling 'white'. I say that it resembles them in the respect in which they all resemble one another—the respect that is the basis for calling them 'white'. This isn't just one way of describing an observation; it's the only way. If I want to record my observation (for my own future use or to inform others) I *must* assert a resemblance between the fact which I have observed and something else. It is inherent in a description to be the statement of a resemblance, or resemblances.

So we see that. . . .we can't *speak of* an observation—can't bring it under *language*—without declaring more than that one observation, which we do by assimilating it to other phenomena already observed and classified. Whewell regards this ·going-from-one-to-many· process as characteristic of induction. But ·he is wrong·: this identification of an object—this recognition of it as having certain known characteristics—has never been muddled with induction. It's a perception of resemblances, obtained by comparison; it precedes all induction, and supplies it with its materials.

These resemblances aren't always grasped directly, by merely comparing the object observed with some other present object or with our memory of an object that isn't present. They are often learned. . . .deductively. In describing some new kind of animal I say that it is ten feet long from the forehead to the tip of the tail. I didn't learn this from my unassisted eye. I had a two-foot ruler which I applied to the object and measured it; and rather than being wholly manual the measuring operation was partly mathematical, involving the two propositions *Five times two is ten* and *Things that are equal to the same thing are equal to one another*. So the fact that the animal is ten feet long is not an immediate perception, but a conclusion from reasoning; the observation of the object provides only the other premises of the inference. But we call this an observation, or a description of the animal, not an induction concerning it.

Now for a very complex example: I affirm that *the earth is globular*. The assertion isn't based on direct perception;

indeed, we can't directly perceive the shape of the earth (though the assertion wouldn't be true unless there were conceivable circumstances in which its truth could be perceived directly). That the earth is globular in shape is inferred from certain marks, such as:

- its shadow thrown upon the moon is circular;
- on the sea or any large plain our horizon is always a circle;

each of which is incompatible with any shape except that of a globe. I then go further, and say that the earth is an oblate spheroid [see Glossary], which is one kind of globe [and he gives a very unclear statement of the evidence for this]. But each of these propositions—*The earth is globular* and *The earth is an oblate spheroid*—asserts an individual fact which could be perceived by the senses if we had the required sense-organs and the needed viewpoint; so each could properly enough be called a 'description' of the earth's shape, even though it has been inferred rather than seen. But it wouldn't be proper to call either of these assertions an 'induction' from facts about the earth. They aren't general propositions inferred from particular facts, but particular facts inferred from general propositions. They are conclusions deduced from premises originating in induction, ·but don't think 'So this is all a matter of induction after all, with observations of the earth as its basis'·: some of those premises weren't obtained by observation of the earth and had no special reference to it.

Why should the truth about the shape of the earth's orbit be an induction if the truth about its own shape is not? The two cases differ in this:

- The shape of the earth was established by reasoning from facts that were signs of ellipticity;
- The shape of the earth's orbit was established by first *guessing* that it was an ellipse and then finding that empirical observations confirmed that hypothesis.

According to Whewell, however, this process of guessing and verifying our guesses is induction, and is indeed the whole of induction: no other exposition (he thinks) can be given of that logical operation. Well, the whole Book III of the present work has, I hope, shown that another account *can* be given; and I have tried in chapter 2 of that Book to show that the process by which the ellipticity of the planetary orbits was learned is not induction at all. Now, however, I can go deeper into the heart of the matter and show not merely what that process *is not* but what it *is*.

**§4.** I remarked in III.2 that the proposition 'The earth moves in an ellipse' can be taken in either of two ways:

- as a **description** which serves for the colligation [see Glossary] of actual observations—i.e. merely says that the observed positions of the earth can be correctly represented by points along an imaginary ellipse; or
- as an **induction** which says that positions of the earth that haven't yet been directly observed would be found to correspond to the remaining points on the same ellipse.

The induction is one thing and the description another; but we're in a much better position to conduct the induction if we already have the description. That is because the description—like all descriptions—implies a resemblance between the thing described and something else; in pointing out a resemblance among •the observed places of the earth, it points out something in which •all its places ·may· agree [see Glossary]. . . . So we have, by the same process that gave us the description, obtained what we need for an induction by the Method of Agreement ·that I introduced in III:8.1·. Considering the observed places of the earth as effects, and its motion as the cause that produces them, we find that those effects are all in an ellipse and conclude

that the remaining effects—the places that haven't yet been observed—are all in that ellipse and that the law of the earth's motion is motion in an ellipse.

Thus the colligation of facts by means of hypotheses. . . .takes its proper place among operations subsidiary to induction. All induction presupposes that we have already compared the required number of individual instances and discovered the respects in which they agree, and this preliminary operation is the colligation of facts. Kepler tried in vain to connect the observed places of a planet by various hypotheses of •circular motion, and then at last he tried the hypotheses of an •ellipse and found that it squared with the phenomena. What was really going on here was an attempt—at first unsuccessful and then successful—to discover the respect in which each planet's observed positions agreed [see Glossary] with one another. Then he connected another set of observed facts, the times it took the different planets to complete their orbits, by the proposition that *the squares of the times are proportional to the cubes of the distances*, thus ascertaining the property in which the periodic times of all the different planets. [Amplifying Mill's short-hand a little: for each planet the relevant 'distance' is half the length of the longest straight line through that planet's ellipse.]

All that is true and relevant in Whewell's doctrine of 'conceptions' can be fully expressed by the more familiar term 'hypotheses'; and his 'colligation of facts by means of appropriate conceptions' is just the ordinary process of comparison that I have been describing. So I could have confined myself to those better understood expressions ·and left Whewell out of the discussion·. I'd have been glad to do that, staying with my policy in the present work of avoiding ideological discussions and treating •the mechanism of our thoughts to be irrelevant to •the principles and rules by which the trustworthiness of the results of thinking is to be estimated. But such ideological considerations are the sole basis for a theory of induction in a work—·Whewell's *Novum Organum Renovatum*·—which makes very large claims and has indeed much real merit; so those who come after him have to claim for themselves and their doctrines whatever position may properly belong to them on the same metaphysical ground. That is the aim of the next chapter.

## Chapter 2. Abstraction or the formation of conceptions

**§1.** The metaphysical inquiry into the nature and composition of what have been called 'abstract Ideas', i.e. the notions that correspond in the mind to classes and to general names, belongs not to logic but to a different science; and my purpose here doesn't require me to enter upon it. My only concern is with something everyone accepts, namely that such general notions or conceptions do exist. The mind can conceive a multitude of individual things as one assemblage or class; and general names do really suggest to us certain ideas or mental representations, otherwise we couldn't use the names with consciousness of a meaning. Whether the idea called up by a general name is

- •composed of the various respects in which all the individuals denoted by the name are alike, and of no others (Locke, Brown, and the conceptualists), or
- •the idea of some one of those individuals, clothed in its individual features and accompanied by the knowledge that those features aren't properties of the

class (Berkeley, Bailey, and the modern nominalists), or

- the idea of a miscellaneous assemblage of individuals belonging to the class (James Mill), or
- any one or any other of all these, according to the accidental circumstances of the case,

it is certain that *some* idea or mental conception is suggested by a general name whenever we either hear it or use it ourselves with consciousness of a meaning. And this general idea represents in our minds the whole class of things to which the name is applied. Whenever we think or reason concerning the class, we do so by means of this idea. And our mind's ability to attend to one part of what is present to it and neglect the rest enables us to keep our reasonings and conclusions regarding the class unaffected by anything in the idea that isn't really, or at least that we don't really believe to be, common to the whole class.

So there are such things as general conceptions, or conceptions through which we can think generally; and when we bring a set of phenomena into a class—i.e. compare them with one another to discover what they agree in—some general conception is implied in this mental operation. Given that such a comparison is a necessary preliminary to induction, it is most true that induction couldn't happen without general conceptions.

**§2.** But it doesn't follow that these general conceptions must have existed in the mind before the comparison. It isn't a law of our intellect that

> when we compare things and notice their similarities, we're merely recognising instances in the outer world of something that we already had in our minds.

The conception originally found its way to us as a *result of* such a comparison; we acquired it by *abstraction* (a metaphysical term) from individual things. These may be things that we perceived or thought of on earlier occasions, but they may be the things we are perceiving or thinking of on the very occasion. When Kepler compared the observed positions of Mars and found that they agreed in being points on an ellipse, he was applying a general conception that was already in his mind because he had derived it from his previous experience. But this is by no means always the case. When we compare some objects and find them to be alike in being white, or compare the various species of cud-chewing animals and find them to be alike in being cloven-footed, we have just as much a general conception in our minds as Kepler had in his: we have the conception of *a white thing* or *a cloven-footed animal*. But no-one thinks that we have to bring these conceptions with us and apply them to the facts from the outside; because in these simple cases everyone sees that •the very act of comparison that leads us to connect the facts by means of the conception may be •the source from which we derive the conception. If we had never seen any white object (or any cloven-footed animal) we would at the same time and by the same mental act •acquire the idea and •use it for the colligation of the observed phenomena. Kepler, on the other hand, really had to bring the idea with him and apply it to the facts from the outside; he couldn't evolve it out of the facts—couldn't have acquired it by comparing the planet's positions. But this inability was a mere accident, ·a contingent feature of this particular case·: Kepler *could have* been acquired the idea of an ellipse from the paths of the planets if those paths hadn't happened to be invisible; if a planet had left a visible track, and he had been placed so that he could see it at the proper angle, he could have abstracted his original idea of an ellipse from the planet's orbit. Indeed, any conception which can be used as an instrument for connecting a set of facts could have been originally developed

out of those very facts. The conception is a conception *of* something; and what it's a conception of is really *in* the facts and could—in some supposable circumstances, or by some supposable extension of our actual faculties—have been detected in them. And not only is this always in itself possible, but it actually happens in most cases where it's difficult to obtain the right conception. If no new conception is required—if the job can be done by a conception that is already familiar to mankind—almost anyone might happen to be the first to think of the right one, at least in the case of a set of phenomena that the whole scientific world are trying to connect. The honour, in Kepler's case, was that of the accurate, patient, laborious calculations by which he compared •the results that followed from his different guesses with •the observations of Tycho Brahe. Guessing an ellipse wasn't a great achievement: it would have been guessed long before his time if there hadn't been an obstinate *a priori* prejudice that the heavenly bodies must move in a circle or some combination of circles.

The really hard cases are the ones where the conception destined to create light and order out of darkness and confusion has to be looked for among the very ·dark and confused· phenomena that it then serves to arrange. Why, according to Whewell, did the ancients fail to discover the laws of mechanics, i.e. the laws of equilibrium and of the communication of motion? Because they didn't have clear ideas or conceptions of •pressure and resistance, •momentum, and •uniform and accelerating force. Where could they have acquired these ideas from if not from the very *facts* of equilibrium and motion? The late development of several of the physical sciences—e.g. optics, electricity, magnetism—and the higher generalisations of chemistry Whewell ascribes to the fact that mankind didn't yet have the idea of *polarity*, i.e. of opposite properties in opposite directions. But what was there to suggest such an idea, until the separate pursuit of several of these sciences revealed that some of the facts of each of them did present the curious phenomenon of opposite properties in opposite directions? This was obvious on the surface in only in two cases, those of the magnet and of electrified bodies; and there the conception was cluttered ·and somewhat hidden· by the fact that the opposition of properties seemed to be inherent in material *poles*, fixed points in the body itself. The first comparison and abstraction led only to this conception of *poles*; and if anything corresponding to *that* conception had existed in the phenomena of chemistry or optics, the great difficulty would have been extremely small. What created the difficulty was the fact that

> the polarities in chemistry and optics

were distinct species from

> the polarities in electricity and magnetism,

though of the same genus. To bring all these domains under a single theory, it was necessary to compare a polarity without poles (e.g. the polarisation of light) with the (apparent) poles that we see in the magnet; and to recognise that these different polarities have something in common, namely the character that is expressed by the phrase 'opposite properties in opposite directions'. It was from the result of such a comparison that scientists formed this new general conception. To get to •that from •the first confused feeling of an analogy between some of the phenomena of light and those of electricity and magnetism took a long time and much work and more or less clever suggestions by many superior minds.

So the conceptions we use for collecting and organising facts don't grow from within but are impressed upon the mind from the outside; they are never obtained except through comparison and abstraction, and in most cases—including the most important ones—they are evolved by

abstraction from the very phenomena that it is their role to gather together. I don't deny •that it's often very difficult to do this abstraction well, or •that the success of many inductive operations depends mainly on how well the abstraction was done. Bacon was quite justified in designating 'general conceptions wrongly formed' as one of the chief obstacles to good induction. [Mill also quotes Bacon's Latin, *notiones temere a rebus abstractæ*, which more strictly means 'notions abstracted carelessly (or casually) from things'.]. . . .

**§3.** As I try to show show what the difficulty in this matter really is, and how it is overcome, *please* bear this in mind: when I'm discussing a different school of philosophy I'm willing to adopt their language, so that I'll speak of

> 'connecting facts through the instrumentality of a conception',

this technical terminology means neither more nor less than what is commonly called

> comparing the facts with one another and determining what they agree in.

And the technical expression doesn't even have the advantage of being metaphysically correct. The facts aren't 'connected' except in a metaphorical sense of the word. The *ideas of* the facts may become connected, i.e. we may be led to think of them together; but this could be the result of any casual association. What really happens is more philosophically expressed, I think, by the common word 'comparison' than by the phrase 'to connect'. . . . We acquire the general conception by •comparing particular phenomena, and then, once we have it, we apply it to other phenomena by •comparison. . . . We get the conception of an animal (for instance) by comparing different animals, and when we then see a creature resembling an animal we compare it with our general conception of an animal; and if it fits that general conception we include it in the class ·of animals·. The conception becomes the type [see Glossary] of comparison.

And when you consider what it is to *compare* things, you'll see that when indefinitely many objects are being compared—and even when only three are being compared—the comparison can't be done without a *type* of some sort. When we have to arrange and classify very many objects according to their agreements and differences, we don't make a confused attempt to compare each of them with all the others. We know that the mind can't easily attend to more than two things at a time; so we fix on one of the objects (either randomly or for a reason) and, taking this as our standard ·or type·, compare it with one object after another. If we find a second object that agrees remarkably with the first, leading us to class them together, the question instantly arises: In what particular respect do they agree? Answering that is already a first stage of abstraction, giving rise to a general conception. Having gone that far, we now attend to a third object, and ask: Does the third object agree with the first in the same respect in which the second did? That is, does it fit the general conception that has been obtained by abstraction from the first and second? This shows the tendency of general conceptions, once we have them, to serve as types in place of whatever individual objects we previously used in that way in our comparisons. If we find that not many objects fit this first general conception, we drop it and start again. . . . Sometimes we find that a conception will serve if we leave out some of its details; and by this higher effort of abstraction we obtain a still more general conception. I have given an example of this: the scientific world's ascent from the conception of •poles to the general conception of •opposite properties in opposite directions. . . .

These brief remarks contain, I believe, all that is solid in the theory that the conception that the mind arranges and

unifies phenomena by must be provided by the mind itself, and that we find the right conception by trying first one and then another until we hit the mark. The conception isn't provided *by* the mind until it has been provided *for* the mind; and the facts that supply it are mostly (not exclusively) the very facts we are trying to arrange by it. It's true, though, that in trying to arrange the facts we never advance three steps without forming •a general conception, more or less clear and precise; and that •this becomes the ·type or· standard in terms of which we then compare the rest of the facts. If we aren't satisfied with the agreements that we find among the phenomena by comparing them with this type, or with some more general conception that we can form from this type by a further stage of abstraction, we change our path and look out for other agreements. We re-start the comparison from a different starting-point, and so generate a different set of general conceptions. This is the tentative process that Whewell speaks of, and it's not surprising that it has suggested the theory that the conception is supplied by the mind itself. Whenever the mind puts a conception to work in comparing two things, it's either •a conception that it already has from its previous experience or •one that was supplied to it in the first stage of this very comparison; so that in the later parts of the process the conception presents itself as something compared with the phenomena, not evolved from them.

**§4.** If this is a correct account of the 'instrumentality of general conceptions' in the comparisons that necessarily precede induction, I can now translate into plain language what Whewell means by saying that if a conception is to be useful in induction it must be 'clear' and 'appropriate'.

If the conception corresponds to a real likeness among the phenomena—if the comparisons we have made among of a set of objects has led us to class them according to *real* resemblances and differences—the conception that does this is bound to be 'appropriate' for some purpose or other. Appropriateness is a relative matter: it depends on what we are trying to do. As soon as our comparisons show us something that can be predicated of a number of objects, we have a basis on which an inductive process could be founded. [But it's a further question, Mill goes on to say, whether the induction will be worth doing. He contrasts classifying animals by colour with classifying them by skeletal structure. He continues:] Agreements and differences in respect of skeletal structure are not only more important in themselves ·than colour is·, but they are marks of agreements and differences in many other important features of animals. If the latter features are what we want to study, the conceptions generated by skeletal-structure comparisons are far more 'appropriate' than those generated by comparisons in respect of colour. This is *all* that can be meant by the 'appropriateness' of a conception.

When Whewell says that philosophers of this or that school missed discovering the real law of a phenomenon because the conception they brought to bear on it was 'inappropriate', he can only mean that in comparing various instances of the phenomenon so as to discover what those instances agreed in they missed the important points of agreement, and (at best) fastened onto ones that were comparatively trifling. . . .

Aristotle distinguished two sorts of motion, which he called 'natural' and 'violent' respectively.

- •**Natural** motions apparently take place spontaneously—bodies fall to the ground, flame ascends, bubbles of air rise in water, etc.
- •**Violent** motions never occur without external incitement, and tend spontaneously to cease.

In comparing the 'natural' motions with one another, Aristotle thought that they agreed in one respect, namely, that the body that moved (or seemed to move) spontaneously was *moving toward its own place*, meaning the place it originally came from or the place where a great quantity of matter similar to itself was assembled. In 'violent' motions on the other hand, e.g. when bodies are thrown up in the air, they are moving *from* their own place. This conception of a body moving toward its own place can fairly be called 'inappropriate', for three reasons. It does express a feature that really is found in some of the most familiar instances of apparently spontaneous motion, but

**(1)** in many cases of such motion that feature is absent, e.g. the motion of the earth and planets.

**(2)** In many cases where the feature is present, the motion turns out not to be spontaneous. For example, when air rises in water it doesn't rise by its own nature but is pushed up by the superior weight of the water pressing on it.

**(3)** The spontaneous motion often occurs in the opposite direction to what the theory regards as the body's 'own place'—e.g. when a fog rises from a lake, or when water dries up.

So the agreement that Aristotle selected as his principle of classification didn't cover all cases of the phenomenon he wanted to study, spontaneous motion; and did cover cases where the motion is not spontaneous. The conception, in short, was 'inappropriate'. I would add that in this case no conception would be appropriate: there's no agreement running through all the cases of spontaneous or apparently spontaneous motion and no others; they can't be brought under one law; this is a case of plurality of causes.

**§5.** So much for the first of Whewell's conditions, that conceptions must be 'appropriate'. The second is that they must be 'clear': let us consider what this implies. A conception that doesn't correspond to a real agreement is irrelevant to our purposes; so let us suppose that among the phenomena that we are trying to connect by means of conception C •there really is an agreement and •C is a conception of it. For C to be clear, then, all that is needed is for us to know exactly what the agreement consists in—for us to have carefully observed and accurately remembered it. We are said not to have a 'clear' conception of the resemblance among a set of objects when we have only a general feeling that they resemble, without having analysed their resemblance—i.e. perceived what details it consists in—and fixed in our memory an exact recollection of those details. This lack of clearness, which we could call this vagueness, in the general conception may come from **(a)** our having no accurate knowledge of the objects themselves or merely from **(b)** our not having carefully compared them. Thus a person may have no clear idea of a ship because **(a)** he has never seen one or has only a faint and sketchy memory of the ones he has seen, or because **(b)** he has perfect knowledge and memory of many ships of various kinds, frigates included, but has no clear idea of a frigate because he hasn't taken in and remembered what the differences of detail are between frigates and other ships.

Still, you can have a clear idea without knowing all the common properties of the things which we use it to class together. That knowledge would involve having a conception of the class that was complete as well as clear. All that is needed ·for clarity· is •that we never class things together without knowing exactly why we do so—without having settled exactly what agreements we're going to include in our conception, and •that after thus fixing our conception we don't vary from it by including in the class anything

that lacks those common properties or excluding from it anything that has them. A 'clear' conception is a determinate conception—one that doesn't fluctuate, that isn't one thing today and another tomorrow, but remains fixed and invariable except when we consciously add to it or alter it because of something we have learned. . . .

What are mainly needed for clear conceptions, therefore, are •habits of attentive observation, •extensive experience, and •a memory that takes in and retains an exact image of what is observed. The more someone has of those virtues in relation to a particular class of phenomena, the clearer his conceptions of them will be. He must also never use general names without a precise connotation, but that will naturally result from those other endowments.

As the clarity of our conceptions mainly depends on how *careful* and *accurate* our observing and comparing faculties are, their appropriateness—or rather our chance of hitting on the appropriate conception in any given case—mainly depends on how *active* those same faculties are. If someone has, by habit based on sufficient natural aptitude, become skilled in accurately observing and comparing phenomena, he will perceive so many more agreements, and will perceive them so much faster than other people, that he has a much greater chance of perceiving, in any instance, the agreement that the important consequences depend on.

**§6.** It is so important that the topic of this chapter should be rightly understood that I think I should restate in a somewhat different way the results I have arrived at.

We can't discover general truths, i.e. truths about classes, unless we have formed the classes in such a way that general truths *can* be true of them. Forming any class involves conceiving it *as a class*, i.e. conceiving certain features as being those that characterize the class and distinguish its members from all other things. When we know exactly what these features are, we have a clear idea (or conception) of the class and of the meaning of the general term that names it. The main requirement for having this clear idea is that the class really *is* a class; that it corresponds to a real distinction; that the things it includes really do agree with one another in certain respects and differ (in those same respects) from all other things. A person without clear ideas is one •who habitually classes together under the same general names things that have no common properties, or none that aren't possessed also by other things; or •who, if the usage of other people prevents him from actually misclassing things, can't state to himself the common properties on the basis of which he classes them rightly.

But there's more to a good classification than merely picking out a real class framed [see Glossary] by a legitimate mental process. Some ways of classifying things are more useful to us than others, and our classifications aren't well made unless the things they bring together don't just

- •agree with each other in something that distinguishes them from all other things, but also
- •agree with each other and differ from other things in the very respects that are of primary importance for the purpose we have in view.

In other words, even our clear conceptions are not appropriate for our purposes unless the properties we build into them are ones that will help us toward our goal—i.e. that go deepest into the nature of the things, if we're trying to understand that, or that are most closely connected with the particular property we are trying to investigate. [Twice in this paragraph Mill specifies that all this covers not only 'speculative' goals and pursuits but also 'practical' ones, distinguishing the sciences on the one hand from the likes of morals and politics on the other.]

So we can't frame good general conceptions beforehand. 'This conception that I have obtained, is it the one I want?': you can't answer that until you have done the work you want it for, i.e. until you completely understand the general character of the phenomena, or the conditions of the particular property that you are studying. General conceptions formed without this thorough knowledge are Bacon's *notiones* temere a rebus abstractæ, but we must be continually making up such premature conceptions in our progress to something better. They aren't a drag on the progress of knowledge unless they are *permanently* accepted. When it has become our habit to group things in wrong classes—in groups that aren't really classes, having no distinctive points of agreement (absence of clear ideas), or aren't classes of which anything important to our purpose can be predicated (absence of appropriate ideas)—and when, believing that these badly made classes are sanctioned by nature, we •refuse to exchange them for others and •can't or won't make up our general conceptions from different materials, *then* all the evils that Bacon ascribes to his *notiones temere abstractæ* really occur. This is what the ancients did in physics, and what the world in general does in morals and politics even today.

I therefore don't think it is right to say that we have to obtain appropriate conceptions *before* we start generalising. All through the process of comparing phenomena for the purpose of generalisation, the mind is trying to make up a conception; but the conception it's trying to make up is that of the really important respect in which the phenomena agree. As we learn more about the phenomena themselves, and the conditions their important properties depend on, our views about this naturally alter; and thus we advance from a less to a more 'appropriate' general conception as our investigations progresses.

But don't forget that the really important agreement can't always be discovered by mere comparison of the very phenomena in question, without help from a conception acquired elsewhere. We saw this with the planetary orbits.

The search for the agreement of a set of phenomena is in truth very similar to the search for a lost or hidden object. At first we get into a commanding position and look around from there. If we don't see the object, we ask ourselves where it might be hidden, so as to look for it there; and so on, until we imagine the place where it really is. In this procedure we need a previous conception, or knowledge, of those different places. This illustrates the philosophical operation in which we first try to find the lost object or recognise the common attribute, without conjecturally invoking the aid of any previously acquired conception, i.e. of any hypothesis. Having failed in this, we call on our imagination for some hypothesis of a possible place, or a possible respect of resemblance, and then look to see whether the facts fit the conjecture.

For such cases something more is required than a mind accustomed to accurate observation and comparison. It must be a mind already stored with general conceptions that have some relation to the subject of the particular inquiry. And much will also depend on the natural strength and acquired culture of what has been termed the scientific imagination—i.e. the ability to mentally arrange known elements into new combinations that haven't yet been observed in nature but don't conflict with any known laws.

But the variety of intellectual habits, the purposes they serve, and the ways they can be developed, are themes belonging to the art of education, a subject far wider than logic, and one that this treatise doesn't claim to discuss. So the present chapter can properly close here.

## Chapter 3. Naming, as subsidiary to induction

**§1.** I don't need here to dwell on the importance of language for expressing sympathy and giving information. And I need only a passing mention of language's power to form and rivet associations among our ideas, this being the ultimate source of their functions as intellectual instruments. [Mill's 'passing mention' of this is a long quotation from that 'able thinker' Alexander Bain, writing about the services language does for us: anchoring and storing things that might otherwise slip out of out minds, prodding us to notice likeness that we might have overlooked, and so on. Bain concludes:] 'The number of general names in a language, and the degree of their generality, provides a test of the knowledge of the era and of the intellectual insight that is the birthright of anyone born into it.'

What I have to discuss, however, is not the functions of names, considered generally, but only the topic of *how* and *how much* they are directly instrumental in the investigation of truth, i.e. in the process of induction.

**§2.** Observation and Abstraction, which have had a chapter each, are indispensable to induction; it can't be done without them. Some thinkers have held that the same is true of Naming. . . . In their view, names or at least some kind of artificial signs are necessary for reasoning: there could be no inference, and thus no induction, without them. But if I was right in my account of reasoning in Book II, then this opinion must be regarded as an exaggeration, though of an important truth. If reasoning is from particulars to particulars, and if it consists in recognising one fact as a mark of another or as a mark of a mark of another, nothing is needed to make reasoning possible except senses and association; •senses to perceive that two facts are conjoined, and •association as the law by which one of those two facts raises up the idea of the other. There is evidently no need of language for these mental phenomena, or for the belief or expectation that follows them;. . . .and this inference of one particular fact from another is a case of induction. The lower animals are capable of this sort of induction; it's the sort that uncultivated minds nearly always conduct; and we all do so in the cases where familiar experience forces our conclusions upon us without any active inquiry on our part, and cases where the belief or expectation follows the evidence with the speed and certainty of an instinct.[1]

**§3.** Although inductive inference without the use of signs is *possible*, it couldn't get far beyond the very simple cases I have just described—cases that almost certainly form the limit of the reasonings of animals that don't have conventional language. Without language or some equivalent of it, there could be only as much reasoning from experience as can take place without the aid of general propositions. Strictly speaking, we *can* reason from past experience to a fresh individual case without going through a general proposition, but without general propositions we would seldom remember •what past experience we have had or •what conclusions it warrants. [If what Mill wrote next concerns *general propositions* it is utterly unclear. After it he writes clearly about *language*:] The experience by which we're to

[1] [Mill has two footnotes to this paragraph; one quoting another writer saying the same thing; the other correcting someone who had thought that Mill, by what he wrote here about 'association', was committing himself to the view that 'belief is nothing but an irresistible association'. Mill says:] I express no theory about the ultimate analysis of reasoning or of belief.

guide our judgments may be other people's experience, which we can't know much about except through language; and when the experience is our own, it is generally experience long past, so that in most cases little of it would be retained in the memory unless it were recorded with artificial signs. (The other cases are ones involving our intenser sensations or emotions, or things that we think about daily and hourly.) I hardly need to add that when an inductive inference requires (as most of them do) comparisons among several observations or experiments in varying circumstances, we can't move a step without the artificial memory that words provide. If we often see A and B in immediate and obvious conjunction, we don't need language to be led to expect B whenever we see A; but

- to discover their conjunction when it isn't obvious, or
- to discover whether it is really constant or only casual, and
- whether there's reason to expect it under any given change of circumstances

is far too complex a process for us to perform without some contrivance to give accuracy to our memory of our own mental operations. Now, language is such a contrivance. When that instrument is called to our aid, the difficulty shrinks to that of accurately remembering the meanings of words. . . .

[This section ends with a repetition in slightly different language of what it has said.]

**§4.** Some eminent thinkers have said that what leads us to use general names is the infinite multitude of individual objects; we can't have a name for each, so we're compelled us to make one name serve for many.

This is a very limited view of the function of general names. Even if we had a name for every individual object, we would still need general names as much as we now do. Without them we couldn't express the result of a single comparison, or record any one of the uniformities existing in nature; and our inductions would hardly be in better shape than if we had no names at all. With only names of individuals—i.e. proper names—we. . . .couldn't assert any proposition except the unmeaning ones formed by predicating two proper names one of another [e.g. 'Cicero is Tully']. It is only through general names that we can convey any information, predicate any attribute of an individual, let alone a class. We could in theory manage with no general names except the abstract names of attributes; all our propositions would then be of the form 'Individual object X possesses attribute A' or 'Attribute $A_1$ is always (or never) conjoined with attribute $A_2$'. In fact, though, mankind have always given general names to objects as well as to attributes, and indeed *before* attributes; but the general names given to objects imply attributes, derive their whole meaning from attributes, and are chiefly useful as means for predicating the attributes that they (the general names) connote.

Now we must consider what principles are to be followed in giving general names so that the purposes of induction are best served by these names and the general propositions in which they occur.

## Chapter 4. What is needed for a philosophical language. The principles of definition

**§1.** [In this chapter and the next, 'philosophical language' means 'language suitable for use in science'.] If we are to have a language perfectly suitable for investigating and expressing general truths, there are two main requirements (and several minor ones). **(i)** Every general name should have a meaning, steadily fixed, and precisely determined. When the names that we have are in this way fitted for the work they are to do, the next requirement (and the second in order of importance) is that **(ii)** we should have a name wherever one is needed—wherever there's something to be designated by it that it is important to express. The present chapter will be entirely concerned with **(i)**; ·I shall come to **(ii)** in chapter 6·.

**§2.** Every general name must have a certain and knowable meaning. Now the meaning of a general connotative name lies in the connotation [see Glossary]. . . . Thus, the name 'animal' being given to everything that has the attributes of •sensation and •voluntary motion, the word connotes those attributes exclusively, and they are the whole of its meaning. If the name is abstract, its denotation is the same as the connotation of the corresponding concrete name—it *directly designates* the attribute that the concrete term *implies*. [To make sure that that's clear: The attribute of *femininity* has a name, an **abstract** word that denotes or directly designates it, namely 'femininity'. The **concrete** word 'woman' implies or connotes that attribute, meaning that an item gets the name 'woman' because it has femininity. In that explanation, 'woman' could have been replaced by 'feminine'; Mill's category of 'concrete' terms includes adjectives as well as nouns.] To give a precise meaning to general names is to fix steadily the attribute(s) connoted by each concrete general name, and denoted by the corresponding abstract. Abstract names are created before concrete ones, as is proved by the fact that they are almost always derived from them; so we can consider the meaning of each as determined by and dependent on the meaning of the corresponding concrete name; so that the problem of •making our general language distinct [here = 'clear'] is included in the problem of •giving a precise connotation to all our concrete general names.

This isn't hard in the case of new names—technical terms created by scientific inquirers for the purposes of science or art [see Glossary]. It's harder when when a name is in common use; the problem then is not that of electing a convenient connotation for the name but that of discovering and fixing the connotation it is already used with. That this can ever be a matter of doubt is a sort of paradox. But when the vulgar apply the same name to a number of different things they seldom know exactly what assertion they intend to make, what common property they mean to express. [Mill explains that he takes 'the vulgar to include 'all who don't have accurate habits of thought'.]

When *they* apply a name to an object, all it expresses is a confused feeling of resemblance between that object and other things they have been accustomed to denote by that name. They have applied 'stone' to various objects; then they see a new object that appears to them somewhat like the previous ones, and they call it a 'stone', without asking themselves

- •in what respect is this like the stones I have encountered previously? or
- •for a thing to count as a 'stone', how closely must it resemble other stones?

They don't know how the second question would be answered by the best authorities; they don't even know how

they themselves would answer it. But this rough general impression of resemblance is made up of particular features of resemblance; and it is the logician's business to analyse it into them, i.e. to discover what points of resemblance among the different things commonly called 'stones' have given ordinary people this vague feeling of likeness that has dictated the uses of the word 'stone'.

But though general names are applied by the vulgar on the basis of a mere vague resemblance, in due course people assert general propositions in which predicates are applied to *all* the things that are denoted by the name. Each of these propositions predicates some more or less vaguely conceived attribute; the ideas of these various attributes come to be associated with the name, and in a sort of uncertain way [Mill's phrase] it comes to connote them; people hesitate to apply the name to anything that doesn't have all the attributes commonly predicated of the class. In this way, the propositions that common minds are in the habit of hearing or uttering concerning a class make up in a loose way a sort of connotation for the class-name. Take the word 'civilised'. Even among the most educated persons you won't find many who would undertake to say exactly what 'civilised' connotes. Yet those who use the word *feel* that they're using it with a meaning; and this meaning is made up, in a confused way, of everything they have heard or read about civilised men and civilised communities.

It's probably at this stage in the career of a concrete name that the corresponding abstract name generally comes into use. Under the notion that the concrete name must convey a meaning, i.e. that there's some property common to all the things it denotes, people give a name to this common property. From the concrete 'civilised' they form the abstract 'civilisation'. But since most people have never considered the different things that are called by the concrete name, comparing them so as to ascertain what properties (if any) they have in common, each person is thrown back on the marks by which he himself has customarily been guided in his application of the term; and these, being merely vague hearsays and current phrases [Mill's five-word phrase], are not the same in any two persons or in one person at different times. Hence the word that professes to designate an unknown common property conveys different ideas to almost any two minds. Think about 'civilised' again! No two persons agree about what items are civilised; and when something is called 'civilised', no-one else knows what he means to assert, and the speaker doesn't know exactly. This uncertainty shows up even more strikingly with many other words—consider 'honour' and 'gentleman'.

I hardly need to say that if no-one can tell exactly what the proposition P means, it can't have been brought to the test of a correct induction. [It's not clear what Mill means by that, is it? But at least it's clear that he includes the statement that P can't be used in a correct induction. He continues:] Whether a name is to be used as an instrument of thinking or as a means of communication, it is imperative to •determine exactly the attribute(s) that it is to express—i.e. to •give it a fixed and ascertained connotation.

**§3.** It would be a complete misunderstanding of a logician's role in dealing with terms already in use if we were to think that

> because a name doesn't now have an ascertained connotation, anyone is free to give it such a connotation *at his own choice*.

The meaning of a term actually in use is not •an arbitrary quantity to be fixed but •an unknown quantity to be sought.

[Mill's opening paragraph on this topic points out—at perhaps more length than is needed—the disadvantages of

giving to a word in common use a meaning that creates a 'rupture' with its its present meaning. He concludes:] The fixed and precise connotation that the word receives should be. . . .in agreement (as far as it goes) with the vague and fluctuating connotation that the term already had.

To fix the connotation of a concrete name, or the denotation of the corresponding abstract name, is to define the name. When this can be done without conflicting with any generally accepted assertions, the name can be defined in accordance with its existing common use. Instead of 'defining the name', this is vulgarly called 'defining the thing', meaning 'defining the ·relevant· class of things'—for nobody talks of defining an individual. What is meant by this improper way of talking is: *define the name subject to the condition that it shall denote those things*. This presupposes a comparison of the things, feature by feature and property by property, to discover what attributes they agree in; and often enough it also involves a strictly inductive operation to discover some unobvious agreement that is the cause of the obvious ones.

In order to give a connotation to a name, consistently with its denoting certain objects, we have to select from among the attributes in which those objects agree. So the first logical operation is to discover what they do agree in; and then the question arises: Which of these common attributes should we select to associate with the name? In many cases the common properties. . . .are extremely numerous. Our choice is narrowed down first by the preference to be given to properties that are well known and commonly predicated of the class; but even these are often too numerous to be all included in the definition, and anyway the most generally known properties may not be the best ones to mark out the class from all others. So we should select from among the common properties the ones (if there are any) on which it has been discovered by experience or proved by deduction that many others depend; or at least that are sure marks of many others. We thus see that to frame a good definition of a name already in use is a matter not of •choice but of •discussion; and discussion not merely about linguistic usage but also about the properties of things, and even about the origin of those properties. Every enlargement of our knowledge of the objects the name is applied to is liable to suggest an improvement in the definition. It is impossible to frame a perfect set of definitions on any subject until the theory of the subject is complete; and as science progresses so do its definitions.

**§4.** When the discussion of Definitions turns not on the use of words but on the properties of things, Whewell calls it 'the explication of conceptions'; and the act of learning more about what detailed resemblances a classification is based on he calls—in his technical phraseology—unfolding the general conception in virtue of which those things are so classed. His terminology appears to me to have a darkening and misleading tendency, but several of his remarks are so good that I shall take the liberty of transcribing them.

He observes that many of the controversies that have loomed large in the formation of the existing body of science have had 'the form of a battle of definitions'. He continues:

> 'For example, the inquiry into the laws of falling bodies led to the question whether the proper definition of *uniform force* is that it generates a velocity proportional to the *space* or to the *time* from rest. The controversy about the *vis viva* was about the proper definition of *the measure of force*. A principal question in the classification of minerals is: what is the definition of a *mineral species*? Physiologists have tried to throw light on their subject by defining *organisation*, or some similar term.'

Questions of the same nature were long open, and are not yet completely closed, concerning the definitions of *specific heat*, *latent heat*, *chemical combination*, and *solution*. He goes on:

> 'These controversies have never been questions of insulated and *arbitrary* definitions, as men seem often tempted to think they were. In all cases there's a tacit assumption of some proposition that •is to be expressed by means of the definition and •gives it its importance. The dispute about the definition thus acquires a real value, and becomes a question of true and false. In the discussion of the question "What is a uniform force?" it was taken for granted that gravity is a uniform force. In the debate on the *vis viva* it was assumed that in the interaction of bodies the whole effect of the force is unchanged. In the zoological definition of *species* (that it consists of individuals that have or may have come from the same parents), it is presumed that individuals so related resemble each other more than those that are excluded by such a definition; or perhaps that species so defined have permanent and definite differences. A definition of *organisation* or any other term that wasn't used to express some principle would have no value.
>
> 'So the establishment of a right definition of a term can be a useful step in the explication of our conceptions, but only when we are thinking of some proposition in which the term is used. For then the question really is: how must we understand and define the conception so that the proposition comes out true?
>
> 'To unfold our conceptions by means of definitions has never been serviceable to science, except when it has been associated with an immediate *use* of the definitions. The attempt to define *uniform force* was combined with the assertion that gravity is a uniform force; the attempt to define *accelerating force* was immediately followed by the doctrine that accelerating forces can be compounded; the process of defining *momentum* was connected with the principle that momenta gained and lost are equal; naturalists would have given in vain the definition of *species* that I have quoted if they hadn't also given the characters of species so separated. . . Definition may be the best way of explaining our conception, but there would be no point in explaining it in any way if it weren't to be used in expressing truth. When a definition is propounded to us as a useful step in knowledge, we are always entitled to ask what principle it serves to enunciate.'

In giving, then, an exact connotation to the phrase 'uniform force' it was understood that the phrase should continue to denote gravity. So the discussion regarding the definition came down to the question: What uniformity is there in the motions produced by gravity? By observations and comparisons it was found that what was uniform in those motions was •the ratio of the velocity acquired to •the time elapsed, equal velocities being added in equal times. A *uniform force* was therefore defined as *a force that adds equal velocities in equal times*. Similarly in defining *momentum*: it was already an accepted doctrine that when two objects collide the momentum lost by one is equal to that gained by the other. It was thought necessary to preserve this proposition because it was felt to contain a truth; even a superficial view of the phenomena left no doubt that in any collision there was something of which one body gained precisely the amount that the other lost; and the word 'momentum' had been invented to express this unknown something. (Sometimes a definition is required to preserve

a proposition that is firmly fixed in popular belief; but not in this case, because the proposition in question had never been heard of by any but the scientifically instructed.) What was needed to settle the definition of *momentum*, therefore, was the answer to the question: When one body sets another body in motion, it loses exactly as much *what* as the other gains? When experiments had shown that the answer is 'the product of the velocity of the body by its mass or quantity of matter', this became the definition of *momentum*.

The following remarks of Whewell's are therefore perfectly correct:

> 'The business of definition is part of the business of discovery. . . To make a definition that has any scientific value one needs a great deal of acuteness in discovering the truth. . . When it's clear to us what ought to be our definition, we know pretty well what truth we have to state. . . . The writers on logic in the middle ages made *definition* the last stage in the progress of knowledge; and this view of theirs, at least, is confirmed by the history of science and the philosophy derived from that history.'

For in order to judge finally how the name that denotes a class may best be defined, we must know all the properties common to the class, and all the relations of causation or dependence among those properties.

The most felicitous kind of definition is the one where the properties that are fittest to be selected as marks of other common properties are obvious and familiar, especially if they also contribute greatly to giving people the general impression of resemblance that led to the formation of the class in the first place. But a class often has to be defined by some property that isn't familiarly known, though it needs to be the best mark of those that *are* ·familiarly· known. De Blainville, for instance, based his definition of *life* on the process of decomposition and recomposition that continually occurs in every living body, so that the particles composing it are never for two instants the same. This is not one of the most obvious properties of living bodies; an unscientific observer might miss it altogether. Yet great authorities. . . . have thought that no other property satisfies so well the conditions required for the definition.

**§5.** Having set out the principles that we ought usually to follow when we are trying to give a precise connotation to a term in use, I must now add that it isn't always practicable to stick to those principles, and that even when it is practicable it isn't always desirable.

There are many cases where it's impossible to comply with all the conditions of a precise definition of a name in agreement with usage. In many cases a word W can't be given one connotation that will make W denote everything that is customarily denotes, or that makes true all the propositions that it customarily enters into and that have any foundation in truth. Independently of accidental ambiguities, where the different meanings have no connection with one another, it often happens that a word is used in two or more senses derived from each other yet radically distinct. So long as a term is vague—i.e. doesn't have a permanently fixed connotation—it is constantly liable to be applied by extension from one thing to another, until it reaches things which have little if any resemblance to those that were first designated by it.

[Mill quotes Dugald Stewart, a Scottish philosopher and mathematician, describing this process as happening along a series of objects so that they all fall under a single general name although no three of them have anything in common and the first is so unlike the last that] 'no stretch of imagination can conceive how thoughts were led from one to the

other. Yet the transitions may have been all so easy and gradual that if they were successfully detected by a theorist, we would instantly recognise not only the likelihood but the truth of the conjecture ·about how a single general word travelled along the series·; in the same way that we are confidently certain of the well-known etymological process that connects the Latin preposition *e* or *ex* with the English noun "stranger" as soon as the intermediate links of the chain—*e*, *ex*, *extra*, "extraneous", *étranger*, "stranger"—are present for our examination.'

Stewart uses the adjective 'transitive' for the applications that a word acquires through this gradual extension of it from one set of objects to another. After briefly illustrating ones that are the result of local or casual associations, he proceeds as follows:

> 'But although most of the transitive or derivative applications of words depend on casual and unaccountable whims of the feelings or the imagination, there are certain cases where they open a very interesting field of philosophical speculation. They are the ones in which an analogous transference of the corresponding term can be seen in many or most other languages, this being a uniformity that must be ascribed to the essential forces of human nature. But even in these cases the explanation doesn't usually lie in *similarities* amongst the objects the word is applied to; it often comes from *associations of ideas* based on the common faculties, common organs, and common condition of the human race. . . The resulting meaning-pattern will vary according to how intimate and how strong the underlying associations are. Where the association is slight and casual, the various meanings will remain distinct from each other, and will often come to look like capricious varieties in the use of the same arbitrary sign. Where the association is so natural and habitual as to become virtually unbreakable, the transitive meanings will coalesce in one complex conception; and every new transition will become a more comprehensive generalisation of the term in question.'

I call attention to the law of mind expressed in that last sentence; it's the source of the perplexity so often experienced in detecting these transitions of meaning. Ignorance of that law is the shoal on which some of the most powerful intellects that have adorned the human race have been stranded. Plato's inquiries into the definitions of some of the most general terms of moral theory are described by Bacon as being closer to a true inductive method than any of the other ancients achieved; and they are indeed almost perfect examples of the preparatory process of comparison and abstraction; but because he didn't know of the law just mentioned, he often wasted the powers of this great logical instrument on inquiries in which it couldn't produce any result because the phenomena whose common properties he so elaborately tried to detect didn't have any common properties. Bacon himself fell into the same error in theorising about the nature of heat, in which he jumbled together—under the name 'hot'—classes of phenomena that have no property in common. . . . Aristotle and his followers were well aware of ambiguities in language, and delighted in distinguishing them. But they never suspected ambiguity in the cases where (as Stewart remarks) the association from which the transition of meaning arose is so natural and habitual that the two meanings blend together in the mind, and a •real transition becomes an •apparent generalisation. They took endless trouble trying to find a definition that would serve for several distinct meanings at once; as in an instance that Stewart mentions, the meaning of 'causation'. The

corresponding Greek word prompted them to look for the the common idea that runs through an effect's efficient cause, material cause, formal cause, and final cause [see Glossary on **efficient**]. Stewart adds: 'Other philosophers have produced idle generalities about the ideas of the *good*, the *fit*, and the *becoming*—all this arising from the same undue influence of popular epithets on the speculations of the learned.'

Stewart considers 'beautiful' to be one of the words that have undergone so many transitions of meaning that there's no longer any trace of a property possessed by all and only the objects the word is applied to. And I can't help feeling (though this isn't a question in logic!) a considerable doubt whether the word 'beautiful' connotes the same property when we speak of

- •a beautiful colour,
- •a beautiful face,
- •a beautiful scene,
- •a beautiful character, and
- •a beautiful poem.

No doubt the word was extended from one of these objects to another on account of a resemblance between them, or (more likely) between the emotions they aroused; and, by this progressive extension it has at last reached things very remote from the visible objects that it was originally applied to. It is at least questionable whether the things that would ordinarily be called 'beautiful' have any property in common except *agreeableness*. The term certainly does connote this, but that can't be all that people usually mean by 'beautiful' because many agreeable things are never called beautiful. If that is right, the word 'beautiful' can't be given any fixed connotation that will make it denote all and only the things that it now denotes in common use. But it ought to have a fixed connotation; as long as it doesn't it is unfit to be used as a scientific term and is a perpetual source of false analogies and erroneous generalisations.

This illustrates my remark that even when there is a property common to all the things denoted by a name, it's not always desirable to make that property the definition and exclusive connotation of the name. The various things called 'beautiful' are certainly alike in being agreeable; but to make this the definition of *beauty*, and thus extend 'beautiful' to all agreeable things, would be •to drop a portion of meaning that the word really though unclearly conveys, and •to contribute to making ourselves overlook and forget those qualities of the objects that the word previously, though vaguely, pointed at. In such a case where we want to give a fixed connotation to a term, it is better to do this by restricting its use than by extending it. It is better •to exclude from the ·range of the· adjective 'beautiful' some things that it is commonly applied to than •to leave out of its connotation any of the qualities that may—even if they are occasionally lost sight of—have guided people's minds in the commonest and most interesting applications of the term. When people call anything 'beautiful' they certainly think they are saying more than merely that it is agreeable. They think they're ascribing a special *sort* of agreeableness, analogous to what they find in some other of the things that they are also accustomed to calling 'beautiful'. So if there is any special sort of agreeableness that is common to (if not all, at least) the principal things that are called 'beautiful', limiting the denotation of the term to those things is better than leaving that kind of quality without a word to connote it, thereby diverting attention from its special features.

**§6.** Here is a rule of terminology that is of great importance though it has hardly been recognised as a rule except by a few thinkers of the present century:

> In trying to rectify the use of a vague term by giving it a fixed connotation, take care not to discard any portion of the connotation that the word previously had, however unclearly; unless you are doing this advisedly, on the basis of a deeper knowledge of the subject.

Otherwise language loses one of its inherent and most valuable roles, as conservator of ancient experience, keeper-alive of thoughts and observations of former ages that may be alien to the tendencies of the passing time. This function of language is so often overlooked or undervalued that a few observations on it appear to be extremely required.

There's a constant tendency for any word, through familiar use, to lose some of its connotation; this happens even when its connotation has been fixed, but still more if it has been left as a vague unanalysed feeling of resemblance. It is a well-known law of the mind that

> a word originally associated with a very complex cluster of ideas doesn't call up all those ideas in the mind every time it is used; it calls up only one or two, from which the mind runs on by fresh associations to another set of ideas without waiting for the suggestion of the ·omitted· remainder of the complex cluster.

If this weren't so, our processes of thought couldn't be anywhere near as fast as they are. Very often, indeed, when we're using a word in our mental operations we run on to new trains of ideas by other associations that the mere *word* starts off, without having brought into our our imagination *any part whatever* of the complex idea corresponding to the *meaning* of the word. When that happens we are using the word in an almost mechanical manner, which we can do even when we using it well and accurately in carrying on important processes of reasoning. (Some metaphysicians, generalising from the extreme instances of this, have fancied that *all* reasoning is merely the mechanical use of a set of terms according to a certain form.) We can discuss and settle the most important interests of towns or nations by applying general theorems or practical maxims previously laid down, without giving any thought to the houses and green fields, the busy market-places and domestic hearths, that those towns and nations consist of and are part of what 'town' and 'nation' mean.

Since general names come in this way to be used (and even to do some of their work well) without suggesting to the mind their whole meaning, and often suggesting only a very small part or *no* part of that meaning, it's not surprising that these words become unable to suggest any of ideas belonging to them except those with which the association is •most immediate and strong, or •most kept up by the incidents of life. . . . Words naturally retain much more of their meaning to persons of active imagination, who habitually represent things to themselves with the detail that belongs to them in the actual world. For minds of a different kind, the only antidote to this corruption of language is to have a habit of using the name in connection with *all* the properties that it originally connoted, thus keeping up the association between the name and those properties.

But it can't do this unless the predicates retain *their* association with the properties they connote. The propositions can't keep the meanings of the words alive if *their* meanings die! It often happens that propositions are mechanically repeated, mechanically held in the memory, and their truth confidently assented to and relied on, while •they bring no meaning clearly to the mind, and •the matter of fact or law of nature that they originally expressed is lost sight of and plays no part in the person's thinking. In subjects that are both familiar and complicated—especially moral and

social subjects—it is commonly noticed that many important propositions are believed and repeated from habit by people who couldn't say what they mean and whose speech and other conduct aren't affected by their supposed truth. That is why the traditional maxims of old experience, though seldom questioned, often have so little effect on the conduct of life; it's because most people don't really feel their meaning until personal experience brings it home to them. It is also why so many doctrines of religion, ethics, and even politics, so full of meaning and reality to first converts. . . ., show a tendency to degenerate rapidly into lifeless dogmas, a tendency that all the efforts of an education expressly and skillfully directed to keeping the meaning alive are barely sufficient to counteract.

. . . .It is natural and inevitable that in every age a certain portion of our recorded and traditional knowledge, not being continually suggested by the pursuits and inquiries mankind are *at that time* absorbed in, should fall asleep, as it were, and fade from the memory. What saves it from being totally lost is this:

> The propositions or formulas arising from previous experience still remain; they are only forms of words, but of words that once had a meaning and are still supposed to do so; and this (suspended) meaning can be historically traced, and may be recognised by sufficiently able minds as still being matter of fact, or truth.

While the formulas remain, the meaning may at any time revive. . . .and be announced to mankind not as a discovery but as the meaning of something that they have been taught and still profess to believe.

Thus there's a perpetual oscillation in spiritual truths, and in spiritual doctrines of any significance even if they aren't true: their meaning is almost always in a process either of being lost or of being recovered. Look at the history of mankind's more serious convictions—the opinions by which they do (or think they should) regulate their lives—and you'll see that even when recognising verbally the same doctrines, they differ through the years in how much meaning, and even what kind of meaning, to attach to them. [Mill repeats that each age drops the parts of the meaning that don't concern it; but 'any mind duly prepared' can recover the lost meaning, revive it, and get it back into the common meaning.]

The arrival of this satisfactory upshot can be significantly delayed by the shallow conceptions and incautious proceedings of mere logicians. It happens like this:

> When a word W has lost part of its significance and hasn't yet begun to get it back, persons arise whose favourite idea is *the importance of clear conceptions and precise thought* and thus the necessity of definite language. When they examine the old formulas containing W they easily sere that it is used in them without any meaning; and if they aren't capable of rediscovering the lost meaning, they naturally enough dismiss the formulas and define W without reference to them.

In doing this they •fasten W down to what it connotes in common use at the time when it conveys the smallest quantity of meaning, and •introduce the practice of consistently and uniformly using it according to that connotation. In this way W acquires a much wider denotation than it had before; it becomes applicable to many things to which it was previously refused, and that earlier refusal now looks arbitrary. Of the propositions in which W was formerly used, those that were true because of the forgotten part of its meaning are now, by the clearer light shining from the definition, seen not to be true according to the definition; yet the definition is the recognised and sufficiently correct expression of all that

is in the mind of anyone who uses W at the present day. The ancient formulas are thus treated as prejudices; and people are no longer taught to believe that there is truth in them. In the general mind they are no longer surrounded by respect, and ready at any time to suggest their original meaning. Whatever truths they contain are rediscovered far more slowly, and when they *are* rediscovered they look like novelties, and that in some degree at least counts against them.

Here is an example. The minds of thinking persons have always been concerned with the question 'What is virtue?' or 'What is a virtuous character?' (except where moral thinking has been suppressed by outward compulsion, and where the feelings that prompt it are still satisfied by the traditional doctrines of an established faith). Of the different answers that have at various times met with some acceptance each has been a perfect mirror-image of the age that gave it birth. One answer was this: virtue consists in **correctly calculating our own personal interests**, either in this world only or also in another world. To make this theory plausible, it was of course necessary that the only beneficial actions that people in general were accustomed to see. . . .were results of a prudential concern for self-interest, or at least could plausibly be supposed to be so; with the result that in ordinary usage the words connoted no more than was set down in the definition.

Suppose now that the friends of this theory managed to introduce a consistent and regular use of 'virtue' according to this definition. Suppose that they succeeded in •banishing the word 'disinterested' [see Glossary] from the language, and •driving out of the language all expressions frowning on selfishness or commending self-sacrifice or implying that generosity or kindness is anything but giving a benefit in order to receive a greater personal advantage in return. The flouting of the old formulas for the sake of preserving clear ideas and consistency of thought would obviously be a great evil. . . .

[The next sentence refers to Samuel Taylor Coleridge—poet, critic, and philosopher.] The Coleridge school hold that

> the language of any people who have a long history of culture is a sacred deposit, the property of all ages—something that no one age should consider itself empowered to alter.

Put like that it is exaggerated; but it is based on a truth that is frequently overlooked by the class of logicians who •think more of having a clear meaning than of having a comprehensive one, and who •see that every age is adding to the truths that it has received from its predecessors, but •fail to see that a contrary process of losing truths already possessed is also constantly going on isn't easy to counteract. Language is the depository of the accumulated body of experience to which all former ages have contributed and which is the inheritance of all yet to come. . . . However much we may be able to improve on the conclusions of our forefathers, we ought to be careful not to let any of their premises slip through our fingers. It may be good to alter the meaning of a word, but it is bad to let any part of the meaning drop. Anyone wanting to introduce a more correct use of a word W that has important associations should be required to possess an accurate acquaintance with the history of W and of the opinions that in different ages it served to express. To be qualified to define W we must know all that has ever been known of the properties of the class of objects which are, or originally were, denoted by it. If we give it a meaning according to which some proposition that was once generally held to be true comes out false, we ought to be sure that we know how W was understood by those who believed the proposition.

## Chapter 5. The natural history of the variations in the meaning of terms

**§1.** I have described just one way in which words in common use are liable to shift their connotation. The truth is that the connotation of such words is perpetually varying ·in other ways too·, as might be expected given how they acquire their connotation in the first place. A technical term *invented* for purposes of art or science possesses from the outset the connotation given to it by its inventor; but a word W that is in everyone's mouth before anyone thinks of defining it gets its connotation only from the facts that are habitually brought to mind when W is used. Looming large among these facts are the properties common to the things denoted by W; they would be the whole story if language were regulated by convention rather than by custom and accident. But besides these common properties, which if they exist are certainly present whenever W is used, any other circumstance [see Glossary] may be found along with it, •casually but •often enough to become associated with W in the same way, and as strongly, as the common properties themselves. As this association develops, people give up using W in cases where those casual circumstances don't exist. They prefer using some other word, or W with some qualifier rather than using an expression that will call up an idea they don't want to arouse. The originally casual circumstance thus becomes regularly a part of the connotation of the word.

This continual incorporation of accidental circumstances into the permanent meanings of words has two upshots worth noting. **(i)** There are very few exact synonyms. **(ii)** As everyone knows, the dictionary meaning of a word is a very imperfect account of its real meaning. The dictionary meaning is marked out in a broad, blunt way, and probably includes everything that was originally required for the correct use of the word; but in the course of time so many extra associations stick to words that anyone who tried to use them with no guide except the dictionary would muddle up a thousand little distinctions and subtle shades of meaning that dictionaries ignore. We see this in the conversation or writing of a foreigner who isn't thoroughly master of the language. The history of a word, by showing the causes that determine its use, is in these cases a better guide to its use than any definition; for definitions can only show the word's meaning at the particular time, or at most its series of meanings through time, but its history can show the law the series was governed by. For example, a dictionary would be no guide to the correct use of the word 'gentleman'. Originally it meant simply a man born in a certain rank. From this it gradually came to connote all the qualities or casual circumstances that were usually found to belong to persons of that rank. This at once explains why in one of its common meanings it means •anyone who doesn't have to work for a living, in another •anyone who doesn't have to do manual labour for a living, and in its more elevated meaning it has at every time signified •*anyone* whose conduct, character, habits, and outward appearance were—*at that time*—typical of (or thought to be typical) of persons born and educated in a high social position.

It often happens that of two words whose dictionary meanings are the same or only slightly different, $W_1$ is the proper word to use in one set of circumstances and $W_2$ in another set, though we can't show how the custom of so using them originally arose. The accident that $W_1$ and not $W_2$ was used on a particular occasion or in a particular social circle will create such a strong association between $W_1$ and

some specialty of circumstances [Mill's phrase] that mankind abandons the use of it in any other case, and the specialty becomes part of its meaning. The tide of custom first drifts $W_1$ onto the shore of a particular meaning, then retires and leaves it there.

[Mill gives the example of the word 'loyalty', which first meant 'fair, open dealing, and fidelity to engagements' and now means 'fidelity to the throne'. He doesn't know how the change came about, but offers a guess.]

**§2.** In many cases a circumstance that at first casually came into the connotation of a word that originally had no reference to it eventually comes to supersede the original meaning and becomes not merely a part of the connotation but the whole of it. An example is the word 'pagan', *paganus* [Latin]. This was originally equivalent to 'villager'—the inhabitant of a *pagus* or village. At a particular era in the spread of Christianity over the Roman empire the •adherents of the old religion were nearly the same group of individuals as •the villagers or country people, because the inhabitants of the towns were the earliest converted to Christianity. . . . From this casual coincidence the word *paganus* carried with it, and began ever more steadily to suggest, the idea of someone who worships the ancient divinities; until at last it suggested that idea so forcibly that people who didn't want to suggest that idea avoided using that word. But when *paganus* had come to connote heathenism, the very unimportant detail about not living in a city was soon disregarded in the use of the word. Because there was seldom any need to say something about *heathens who lived in the country* there was no need for a separate word to denote them; so 'pagan' came not merely to include 'heathen' in its meaning but to mean that exclusively.

Another example is the word 'villain' or 'villein'. In the middle ages this term had a connotation as strictly defined as a word could have, being the proper legal label for persons who were the subjects of the less burdensome forms of feudal bondage. The scorn of the semi-barbarous military aristocracy for these abject dependants of theirs made the act of likening someone to this class of people a mark of the greatest contempt; and that same scorn led them to ascribe to the same people all sorts of hateful qualities. . . . These circumstances combined to attach to the term 'villain' ideas of crime and guilt so forcibly that the application of the epithet even to those to whom it legally belonged became an insult and was abstained from whenever no insult was intended. From that time guilt was part of the connotation; and it soon became the whole of it because mankind had no urgent reason to continue using their language to distinguish •bad men low down on the social scale from •bad men of any other rank in life.

Examples like these, where the original meaning of a term is totally lost—another and an entirely distinct meaning first being grafted onto the former, and eventually replacing it—present examples of the double movement that is always occurring in language: two counter-movements,

- •one of *generalisation*, by which words are constantly losing portions of their connotation, and coming to have less meaning and ·therefore· more general acceptation;
- •the other of *specialisation*, by which words—perhaps even the same ones—are continually taking on fresh connotation, acquiring additional meaning by being restricted in their range of application.

This double movement is important enough in the natural history of language. . . .to justify dwelling a little longer on its nature and causes.

**§3.** To begin with the movement of generalisation. It might seem unnecessary to dwell on meaning-changes that arise merely from a word's being used in a looser and wider sense than belongs to it, by ignorant people who haven't properly mastered its accepted connotation. But this ·is an important topic, because it· is a real source of alterations in the language: if a word W is often used in cases where one of the qualities it connotes doesn't exist, it stops suggesting that quality with certainty; then even those who know the proper meaning of W prefer to express that meaning in some other way, leaving the original word to its fate. . . . This gives us insight into the way languages have degenerated at times when literary culture was *suspended*; and we're now in danger of experiencing a similar evil through the *superficial extension* of that same culture. [The next sentence is verbatim from Mill.]

> So many persons without anything deserving the name of 'education' have become writers by profession that written language may almost be said to be principally wielded by persons ignorant of the proper use of the instrument, and who are spoiling it more and more for those who understand it.

Vulgarisms, which creep in nobody knows how, are daily depriving the English language of valuable ways of expressing thought. The verb 'transpire' used to mean 'become known through unnoticed channels'—to exhale, as it were, into the public arena like a vapour or gas. But recently people have started to use this word as a supposedly more decorative synonym of 'happen': 'the events which have transpired in the Crimea', meaning the incidents of the war. This vile specimen of bad English is already seen in the dispatches of noblemen and viceroys; and before long, it seems, nobody will understand 'transpire' if used in its proper sense. Some words come to be used in senses unknown to genuine English not because the writers love decoration but simply because they are uneducated. The use of 'aggravating' for 'annoying', in my boyhood a vulgarism of the nursery, has crept into almost all newspapers, and into many books; and when the word is used in its proper sense [namely, 'making worse'], as when writers on criminal law speak of 'aggravating' and 'extenuating' circumstances, they are probably misunderstood, even today. These corruptions of language *do harm*. Those who are struggling to express themselves clearly with precision (knowing from experience how hard it is to do this) find their resources continually narrowed by illiterate writers who seize and twist from its purpose some word or phrase that once served to convey briefly and compactly an unambiguous meaning. [Mill then offers a page full of further examples and further protests.]

There is also a tendency for the meanings of names to be generalised not because of ignorant misuse but because the number of names we have doesn't keep up with the number of things we want to talk about. It is usually very difficult to bring a new name into use (except for new scientific terminology that laymen don't meddle with). And quite apart from that it's natural to prefer giving a new object a name that at least expresses its resemblance to something already known, because an entirely new name would at first convey no information. [Mill gives examples: 'salt', 'oil', 'glass', 'soap'. He then says that this kind of language-change happens even more with words that express the complicated phenomena of mind and society. Historians, travellers, and others who also speak or write about moral and social phenomena that they aren't closely acquainted with are the great agents in this process. Such people (apart from those who are unusually well-educated and thoughtful) have an eminently scanty vocabulary for such subjects. They have a small set of words that they are accustomed to and that

they use to describe widely different phenomena, because they have never sufficiently analysed the facts those words correspond to in their own country to have attached perfectly definite ideas to the words. The first English conquerors of Bengal, for example, took the phrase 'landed proprietor' into a country where the rights of individuals over the soil were extremely different in degree and even in kind from those recognised in England. Applying the phrase, with all its English associations, in Bengal:l

- •to one who had only a limited right they gave an absolute right,
- •from one who didn't have an absolute right they took away all right,
- •whole classes of people were driven to ruin and despair, and
- •the whole country was filled with banditti, creating a feeling that nothing was secure.

In this way, with the best of intentions, they produced more disorganisation of society than had been produced by the most ruthless of its barbarian invaders. . . .

**§4.** Two opposite processes go on together: **(i)** Names come to be applied to more things because the rapid growth of ideas ·of things· outstrips the growth of names. **(ii)** names become on the contrary restricted to fewer occasions, by acquiring extra connotation from circumstances that weren't originally in the meaning but have become connected with it in the mind by some accidental cause. We saw in the words 'pagan' and 'villain' remarkable examples of •word-meanings that became specialised because of casual associations, as well as •meaning-generalisations in new directions that often follow this.

Similar specialisations have occurred often in the history even of scientific terminology. [After presenting many examples of this, all contained in a long quotation from J. A. Paris's *Pharmacologia*, Mill continues:]

A generic term is always liable to become limited in this way to a single species, or even a single individual, if people have occasion to think and speak of that species or individual much oftener than of anything else contained in the genus. Thus

- •for a stage-coachman 'cattle' are horses;
- •for agriculturists 'beasts' are oxen; and
- •for some sportsmen 'birds' are all partridges.

The law of language that operates in these trivial instances was also at work when Christianity named the single object of its worship by borrowing the words Θεος, *Deus*, and 'God' from Polytheism. Almost all the terminology of the Christian Church is made up of words originally used in with much more general meanings. [He lists 14 examples, and adds:] It would be interesting to trace the process through which 'author' came in its most familiar sense to mean 'writer'. . . .

Our ideas of pleasure and pain—and of things that we always think of as sources of our pleasures or pains—are the most liable to cling by association to anything they have ever been *close to*. So the extra connotation that a word most quickly and easily takes on is that of agreeableness or painfulness in their various kinds and degrees:

- •good or bad thing;
- •desirable or to be avoided;
- •object of admiration/hope/love or hatred/dread/contempt.

Almost every word that •expresses a moral or social fact and •is apt to arouse strong favourable or hostile feelings carries with it, decidedly and irresistibly, a connotation of those strong feelings or at least of approval or disapproval. So that if you use any of those names in conjunction with others expressing the opposite feelings you'll produce the effect of a paradox or even a contradiction in terms. A connotation

acquired in this way has a poisonous effect on prevailing habits of thought, especially in morals and politics, as Bentham has often pointed out. It gives rise to the fallacy of 'question-begging [see Glossary] names'. We're inquiring whether a thing x possesses a property P or not, but P has become so associated with the name of x as to be part of its meaning, so that by merely uttering the name we assume the conclusion we were looking for; most apparently self-evident propositions are like that.

. . . .The logician, faced with such changes of meaning, should submit to them with a good grace once they have settled into place and can't be dislodged; and if a definition is needed, define the word according to its new meaning, retaining the older as a second meaning in case of need, if there's any chance of being able to preserve it either in the language of philosophy or in common use. Logicians can't *make* the meanings of any but scientific terms; the meanings of all other words are made by the collective human race. But logicians can get a clear view of whatever it is that has surreptitiously guided the general mind to a particular use of a name; and when they have found it they can clothe it in such distinct and permanent terms that mankind will *see* the meaning which before they only *felt*, and from then on they won't let it be forgotten or misapprehended.

## Chapter 6. The principles of a philosophical language further considered

**§1.** Up to here I have discussed only one of the requirements of a language adapted for the investigation of truth, namely that each of its terms shall have a determinate and unmistakable meaning. There are other requirements, one of which is fundamental and nearly as important—if not *as* important—as the quality I have discussed at such length. For a language to be fitted for its purposes, it should be the case not only that •every word perfectly expresses its meaning but also that •every important meaning has its word. Whatever we have occasion to think of often, and for scientific purposes, ought to have a name assigned to it.

This requirement of philosophical language involves three conditions, which I'll discuss in the next three sections.

**§2.** **First**, there ought to be all the names needed for recording individual observations in such a way that the words of the record show exactly what fact it is that has been observed. In other words, the language should have **an accurate descriptive terminology**.

The only things we can observe directly are our own sensations or other feelings, so a complete descriptive language would contain a name for every kind of elementary sensation or feeling. Combinations of these can always be described if we have a name for each of the elementary feelings that compose them; but it is a big help if the language has distinctive names not only for the elements but also for every combination of elements that recurs frequently. Why? Because this makes for brevity and thus for clearness, which often depends very much on brevity. On this topic I can't do better than quote some of the excellent things Whewell has said about it:

> 'The meaning of [descriptive] technical terms can be fixed in the first instance only by convention, and can be made intelligible only by presenting to the senses whatever it is that the terms are to signify. The knowledge of a colour by its name can only be taught through the eye.' [Whewell then says that

when a descriptive term T is explained in terms of a sense-presentation, *that* is what the learner must be reminded of by T—not any meaning that T or some part of it has in common language. If someone is introduced to 'apple-green' as the name of a kind of sensation, his later uses of the label should refer back to that sensation, avoiding irrelevant thoughts about what apples usually look like. Whewell continues:]

'It is most important to remember this in connection with the •simpler properties of bodies such as colour and shape, but it's equally true with for •more compound notions. In all cases the term is fixed to a particular meaning by convention; and a student wanting to use the word must be completely familiar with the convention, so that he has no need to rely on risky guesses based on the word itself.' [He gives the example of 'papilionaceous', an adjective that marks off a kind of flower that resembles a butterfly [Latin *papilio* = 'butterfly']; but the conventional meaning brings in only a very small selection of butterfly-features; and a user of the word has to know *those*, i.e. to know the convention.]

In these cases where the thing named is a combination of simple sensations, the meaning of the name W can be learned without going back to the sensations themselves; the learner may get W's meaning through other words—which is to say that W can be *defined*. The names of elementary sensations, or elementary feelings of any sort, can't be defined; the only way to make their meaning known is to •make the learner experience the sensation or to •direct his thought somehow to his memory of having experienced it before. Hence the only things that can be handled in an exact descriptive language are the impressions on the outward senses and inward feelings that are connected with outward objects in a very obvious and uniform way. It would be useless to try to name the countless variety of sensations arising from diseases or from special physiological states: no-one can judge whether my sensation is the same as his, so there can't be a name for it that has real community of meaning between the two of us. This also holds to a considerable extent for purely mental feelings. But in some of the sciences dealing with external objects the scientific language has been developed to a level of perfection that it's scarcely possible to improve on.

[Mill now quotes a long passage in which Whewell writes almost rapturously about the basic descriptive means of *botany*, which build on fundamental names for parts of plants, the parts being identified through their role in the life of the plant. The quoted passage ends thus:] 'Other characters. . . .are also conveyed with similar precision: colour by means of a classified scale of colours. . . This was done with most precision by Werner, whose scale of colours is still the most usual standard of naturalists. Werner also introduced more exact terminology for other characters that are important in mineralogy, e.g. "lustre", "hardness". But Mohs went further, with a numerical scale of hardness in which talc is 1, gypsum 2, calc spar 3, and so on. . . .'

**§3.** So much for language needed for recording our observation of individual instances. But when we proceed from observation to induction, or rather to the comparison of observed instances that is the preparatory step toward induction, we come to a **second** requirement, ·namely **general names for properties that are likely to be central in high-level theories**·.

Sometimes for purposes of induction we have to introduce some new general conception (Whewell's phrase); that is, sometimes the comparison of a set of phenomena leads us to

recognise in them some common feature that is to us a *new* phenomenon because this is the first time our attention has been directed to it. Whenever this happens it's important to give a name to this new conception, this new result of abstraction; especially if the feature it involves •leads to many consequences or •is likely to be found also in other classes of phenomena. In most cases the meaning might be conveyed by combining several words already in use. But when a thing has to be often spoken of, there are more reasons than merely the saving of time and space for speaking of it in the most concise manner possible. What darkness would be spread over geometrical demonstrations if wherever the word 'circle' is used the definition of *circle* were used instead. In mathematics and its applications, where the nature of the processes demands that the •attention should be strongly concentrated. . . ., the importance of concentration also in the •expressions has always been felt; and a mathematician no sooner finds that he will often want to speak of the same two things together than he at once creates a term to express them whenever combined; just as in his algebraical operations he substitutes for $(a^m + b^n)\frac{p}{q}$, or for $\frac{a}{b} + \frac{b}{c} + \frac{c}{d}+$ etc., the single letter P, Q, or S; not solely to shorten his formulae but also to simplify the purely intellectual part of his operations by enabling the mind to focus on the relation between the quantity S and the other quantities that enter into the equation, without being distracted by thinking unnecessarily of the parts of which S is itself composed.

But there's also another reason for giving a brief and compact name to each of the more considerable results of abstraction that come up in our intellectual procedures. By naming them, we fix our attention on them; we keep them more constantly before the mind. The names are remembered, and being remembered they suggest their definition; whereas if instead the meaning had been expressed by putting together a number of other names, that particular combination of

> words already in common use for other purposes

would have had nothing to make itself remembered by. If we want to make a particular combination of ideas permanent in the mind, nothing clinches it like a name specially devoted to expressing it. If mathematicians had had to speak of

> •'that to which a quantity, in increasing or diminishing, is always approaching nearer, so that the difference becomes less than any assignable quantity, but to which it never becomes exactly equal'

instead of expressing all this by the simple phrase

> •'the **limit** of a quantity'

we would probably have long remained without most of the important truths that have been discovered through the relation between quantities of various kinds and their limits. If physicists had had to speak of

> •'the product of the number of units of velocity in the velocity by the number of units of mass in the mass'

instead of speaking of

> •'**momentum**'

many of the truths of dynamics now grasped by means of this complex idea would probably have escaped notice because the idea itself wouldn't have come to mind with sufficient readiness and familiarity. And on subjects closer to the topics of popular discussion, if you want to draw attention to some new or unfamiliar distinction among things you won't find a better way to do it than by inventing or selecting suitable names for the special purpose of marking it.

A volume devoted to explaining what the writer means by 'civilisation' won't convey as vivid a conception of it as will the single expression: *Civilisation is a different thing from Cultivation*. The compactness of that brief designation for the contrasted quality is an equivalent for a long discussion.

If we want to impress forcibly on the understanding and memory the distinction between the two different conceptions of a representative government, we can't do it better than by saying that *Delegation is not Representation*. Hardly any original thoughts on mental or social subjects ever make their way among mankind, or get their proper importance in the minds even of their inventors, until aptly-selected words or phrases have nailed them down (as it were) and held them fast.

**§4.** Now we come to the **third** requirement for a philosophical language, ·namely **a name for every Kind**·. By a Kind, you will remember, I mean

> a class that is distinguished from all others not by one or a few definite properties, but by an unknown multitude of them; the combination of properties on which the class·ification· is based being a mere pointer to indefinitely many other distinctive attributes.

The class *horse* is a Kind, because the things that agree in having the characters by which we recognise a horse agree in a great many other properties that we know about and also—it can't be doubted—in many more than we know. *Animal* is a Kind, because no definition that could be given of the name 'animal' could •include the properties common to all animals or •supply premises from which the remainder of those properties could be inferred. A combination of properties that doesn't give evidence of the existence of any other independent special features doesn't constitute a Kind. So *white horse* is not a Kind, because horses that agree in whiteness don't agree in anything else except •the qualities common to all horses and •whatever causes or effects whiteness has.

On the principle that there should be a name for everything that we have frequent occasion to make assertions about, there obviously ought to be a name for every Kind. It is the very meaning of 'Kind' that the individuals composing a Kind have indefinitely many properties in common; from which it follows that the Kind is a subject to which many predicates (if and when we have them) will have to be applied. And so we have the third requirement for a philosophical language: there must be a name for every Kind, i.e. there must not only be a •terminology but also a •nomenclature.

As far as I know, Whewell is the first writer who has regularly given different meanings to the words 'nomenclature' and 'terminology'. But the distinction he has drawn between them is real and important, so his example is likely to be followed by those who come **after** him; and. . . .a vague sense of the distinction can be seen to have influenced the use of the terms in common practice, **before** he pointed out the advantages of discriminating them philosophically. When Lavoisier and Guyton-Morveau reformed the language of chemistry, everyone would say, their reform consisted in the introduction of a new •nomenclature, not a new •terminology. 'Linear', 'lanceolate', 'oval', or (descriptions of leaves) 'oblong', 'serrated', 'dentate', and 'crenate' belong to the terminology of botany, while the names *Viola odorata* and *Ulex Europaeus* belong to its nomenclature.

We can define 'nomenclature' thus:

> The collection of the names of all the Kinds that any branch of knowledge deals with; or (more properly) of all the lowest Kinds, or *infimæ species*—the species that can be subdivided but not into Kinds.

These lowest Kinds generally fit with what in natural history are termed simply 'species'. Science has two splendid examples of a systematic nomenclature; •that of plants and animals, constructed by Linnæus and his successors, and •that of chemistry, which we owe to the illustrious group of chemists who flourished in France toward the close of the

eighteenth century. In these two departments, not only does every known 'species' or lowest Kind have a name assigned to it, but when new lowest Kinds are discovered names are at once given to them on a uniform principle. In other sciences the nomenclature is not at present constructed on any system, either •because (as in geometry) the species to be named are not numerous enough to require one or •because (as in mineralogy) no-one has yet suggested a suitable principle for such a system—and this lack of a scientifically constructed nomenclature is what is mainly slowing the progress of mineralogy.

**§5.** A word that carries on its face that it belongs to a nomenclature *seems* at first sight to differ from other concrete general names in this:

> Its meaning doesn't reside in its •connotation, i.e. in the attributes implied in it, but in its •denotation, i.e. in the particular group of things it is appointed to designate; so it can't be unfolded by a definition but must be made known in another way.

But this appears to me to be wrong. A word belonging to a nomenclature differs, as I see it, from other words mainly in this:

> Besides its ordinary connotation it also has a special one; besides connoting certain attributes, it also connotes that those attributes mark off a Kind.

The term 'peroxide of iron' belongs by its form to the systematic nomenclature of chemistry, so it bears on its face that it's the name of a particular Kind of substance. It also connotes—as does the name of any class—some portion of the properties common to the class; in this instance the property of *being a compound of iron and the largest dose of oxygen that iron will combine with*.... When we say of the substance before us that it is 'the peroxide of iron', we are saying **(i)** that it is a compound of iron and a maximum of oxygen and **(ii)** that this compound is a particular Kind of substance.

This second part of the connotation of a word belonging to a nomenclature is as essential as the first part, while the definition only declares the first; which is what makes it appear as though the meaning of such terms can't be conveyed by a definition. But this appearance is fallacious. The name *Viola odorata* denotes a Kind, a certain number of whose features—sufficient to distinguish it—are stated in botanical works. This list of features is surely, as in other cases, a definition of the name. Some say:

> •'No, it's not a definition. The name *Viola odorata* doesn't mean •those features; it means •that particular group of plants, and the features are selected from a much greater number merely as marks to recognise the group by.'

I reply that the name doesn't mean that group, because it applies to that group only for a long as the group is believed to be an *infima species*; if we discovered that several different Kinds have been muddled together under this one name, no-one would go on applying the name *Viola odorata* to the whole group; if we didn't just drop it, we would apply it to one only of the Kinds in the group. What is imperative, therefore, is not that the name shall denote one particular collection of objects, but that it shall denote a Kind, a lowest Kind. The form of the name declares that, come what may, it is to denote an *infima species*; and that therefore the properties it connotes—the one expressed in the definition—are to be connoted by it only for as long as we believe that those properties in combination •indicate a Kind and •aren't to be found in more than one Kind.

With the addition of this special connotation that is implied in the form of every word that belongs to a systematic

nomenclature, the set of features that is used to discriminate each Kind from all other Kinds (and is a real definition) constitutes. . . .the whole meaning of the term. It may be objected:

> 'As often happens in natural history, the set of features may be replaced by another set that is better suited for the purpose of distinction, yet the word, still denoting the same group of things, isn't thought have changed its meaning.'

But this can happen with any other general name: in reforming a name's connotation we may leave its denotation untouched; and it is generally desirable to do so. This doesn't stop the connotation from being the real meaning, because wherever the characters set down in the definition are found we immediately apply the name—and anything that exclusively guides us in applying the name must constitute its meaning. If we find that the characters are *not* exclusive to one species as we had thought it to be, we stop using the term co-extensively with the characters; but that's because the failure of the other part of the connotation, namely the condition that the class must be a Kind. The connotation, therefore, is still the meaning; the set of descriptive characters is a true definition; and the meaning is unfolded not indeed (as in other cases) by the definition alone, but by the definition and the form of the word taken together. [You may have noticed that Mill's response to the indented passage above ignores its final clause 'the word isn't thought to have changed its meaning'.]

**§6.** I have now analysed what is implied in the two principal requisites of a philosophical language: •precision, or definiteness [chapters 4 and 5] and •completeness [the present chapter up to here]. Further remarks about how to construct a nomenclature must wait until I come to Classification, because the way of naming the Kinds of things must be subordinate to the way of arranging those Kinds into larger classes. Some of the minor requirements of terminology are well stated and illustrated in Whewell's 'Aphorisms regarding the Language of Science' in his *Philosophy of the Inductive Sciences*. I shan't discuss these because they are of secondary importance from the point of view of Logic; and shall discuss only one more quality which, next to the two already treated, appears to be the most valuable the language of science can possess. A general notion of it can be gathered from the following aphorism:

> Whenever the nature of the subject permits our reasoning processes to be safely carried on mechanically, the language should be constructed on principles that are as mechanical as possible; and when that is not the case, the language should be constructed in such a way as to place the greatest possible obstacles to a merely mechanical use of it.

I'm aware that this maxim requires much explanation, which I now proceed to give. First, what is meant by 'use a language mechanically'? We have the complete or extreme case of this when language is used without any consciousness of a meaning, and with only the consciousness of using certain visible or audible marks in conformity with technical rules previously laid down. This extreme case occurs only in the figures of arithmetic, and (still more) in the symbols of algebra—a language unique in its kind, and perhaps coming as close to perfection for its intended purpose as any creation of the human mind. It is perfect because of how completely it is adapted to a purely mechanical use. The symbols are mere counters, without even the semblance of a meaning apart from the convention that is renewed each time they are used, and is altered at each renewal, the same symbol $a$ or $x$ being used on different occasions to represent things that have nothing in common except

that they can be numbered. So there's nothing to distract the mind from the mechanical operations that are to be performed on the symbols, such as squaring both sides of the equation, multiplying or dividing them by the same or by equivalent symbols, and so forth. Each of these operations corresponds to a syllogism—represents one step of a process of reasoning not about the symbols but about the things they signify—but as it has been found possible to create a technical form such that by conforming to it we can reason correctly, we can completely achieve what we're aiming at without our ever thinking of anything but the symbols. Being intended to work merely as mechanism, these symbols have the qualities that mechanism ought to have: they are of the least possible bulk, so that they take up very little room and waste no time in their manipulation; and they are compact, and fit together so closely that in almost every case the eye can take in all at once the whole operation they are being used to perform.

These admirable properties of the symbolic language of mathematics have led many thinkers to to regard the symbolic language in question as the ideal type [see Glossary] of philosophical language generally; to think that names in general or (as they like to say) *signs*

> are fit for use in thinking in proportion as they can be made to approximate to
>
> > the compactness, the entire unmeaningness, and the ability to be used as counters without a thought of what they represent
>
> that are characteristic of the *a* and *b*, the *x* and *y*, of algebra.

This has led to optimistic views about the acceleration of the progress of science by means which (in my view) •can't possibly contribute to that end and •is a part of the exaggerated estimate of the influence of *signs* that has done a lot to prevent the real laws of our intellectual operations from being rightly understood.

For one thing, a set of signs by which we reason without thinking of their meaning can't be of any use except in our •deductive reasoning. In our direct •inductions we can't for a moment dispense with a distinct mental image of the phenomena, because the whole operation depends on a perception of the respects in which those phenomena agree and differ. Furthermore, this reasoning by counters is suitable for only a very limited portion even of our deductive processes. In our reasonings about numbers, the only general principles we ever need to introduce are:

- •Things that are equal to the same thing are equal to one another, and
- •The sums or differences of equal things are equal,

with their various corollaries. No hesitation can ever arise about the applicability of these principles, because they are true of all magnitudes whatever; and, what's more, every application that can be made of them can be reduced to a technical rule.... But when the symbols represent something other than mere numbers—e.g. straight or curved lines—we have to •apply theorems of geometry that aren't true of *all* lines and •select the ones that are true of the lines we are reasoning about. And how can we do this unless we keep completely in mind what particular lines these are? Since additional geometrical truths may be introduced into our deductive reasoning at any stage of its progress, we mustn't allow ourselves, even for an instant, to use the names mechanically....without an image attached to them. It is only after establishing that the solution of a question about lines •can be made to depend on a previous question about numbers—i.e. (in technical terms) •can be reduced to an equation—that the unmeaning signs become available and the nature of the facts we are investigating

can be dismissed from the mind. Until the establishment of the equation, the language in which mathematicians do their reasoning doesn't differ in character from that of close reasoners on any other kind of subject.

I don't deny that when any correct reasoning is put into the syllogistic shape it is conclusive from the mere form of the expression, provided that none of the terms is ambiguous; and this is one of the facts that have led some writers to think that if all names were constructed and defined so skillfully that there was no ambiguity, this improvement in language would not only •make the conclusions of any deductive science as certain as those of mathematics, but would also •reduce all reasonings to the application of a technical form, and enable their conclusiveness to be rationally assented to after a merely mechanical process, as is undoubtedly the case in algebra. But apart from geometry, the conclusions of which are already as certain and exact as they can be made, the science of *number* is the only one where the practical validity of a bit of reasoning can be apparent to anyone who looks only at the reasoning itself. Whoever has assented to what I said in Book III about •the Composition of Causes and •the superseding of one set of laws by another is aware that geometry and algebra are the only sciences whose propositions are **categorically** true; the general propositions of all the other sciences are true only **hypothetically**, supposing that no counteracting cause happens to interfere. So even a conclusion derived from admitted laws of nature by a formally correct deduction won't have more than a hypothetical certainty. At every step we must make sure that no other law of nature has superseded, or intermingled its operation with, the laws that are the premises of the reasoning; and how can *this* be done by merely looking at the words? We must be constantly •thinking of the phenomena themselves and indeed constantly •studying them, learning what the special features are of every case to which we try to apply our general principles.

The algebraic notation, considered as a philosophical language, is perfect for the subjects in which it is commonly used, namely those where the investigations have already been reduced to discovering a relation between numbers. But while it is admirable for that purpose, the properties that make it so do not qualify it as the ideal model of philosophical language in general; indeed, the more nearly the language of any other branch of science approaches to the algebraic notation, the *less* fit that language is to do its own work. On all other subjects, instead of devices to prevent us from being distracted by thoughts of the meaning of our signs, we ought to want devices to make it impossible that we should ever lose sight of that meaning even for an instant.

With that as our aim we should put as much meaning as possible into the formation of the word itself, making the aids of derivation and analogy available to keep us in mind of all that the word means. In this respect there's an immense advantage for languages like German that form their compounds and derivatives from *native* roots rather than from those of a foreign or dead language, as do English, French, and Italian. The best are those that form their compounds according to fixed analogies corresponding to the relations among the ideas to be expressed. All languages do this to some extent, especially—among modern European languages—German; but even that is inferior to Greek, in which the relation between the meaning of •a derivative word and that of •its primitive is in general clearly marked by its how it is formed. . . .

But words, however well constructed originally, are always tending (like coins) to have their inscription worn off by passing from hand to hand; and the only possible way of reviving it is to keep stamping it afresh by habitually thinking

about the phenomena themselves and not resting in our familiarity with the words that describe them. [Mill develops this thought, saying that for someone who is 'content to live among these formulae' the formulae themselves will gradually lose their meaning. He concludes:] It is as necessary in every non-mathematical subject that the things we are reasoning about should be conceived by us concretely and 'clothed in details' as it is in algebra that we should keep all individualising special features carefully out of view.

That is all I shall say here about the Philosophy of Language. [That is the only occurrence of the phrase 'philosophy of language' in the entire *System of Logic*, except in one footnote.]

## Chapter 7. Classification, as subsidiary to induction

**§1.** As I have said before, there's a kind of classification of things that is inseparable from giving them general names. Every name connoting an attribute automatically divides all things whatever into two classes—those that have the attribute and those that don't, those that the name can be applied to and those that it can't. And this divides all things that actually exist, whether known to us or not, and indeed all that can be imagined to exist.

I have nothing more to say about this kind of classification. The classification that needs to be discussed is altogether different; it is a separate act of the mind. In the first kind of classification the arrangement of objects in groups and the distribution of them into compartments is a mere side-effect of the use of names given simply to express some of their qualities. In this second kind of classification the arrangement and distribution are the main object; naming has to be made to •conform itself to this more important operation—it doesn't •govern it.

This kind of classification is a device for •the best possible ordering of the ideas of objects in our minds; •for causing the ideas to accompany or succeed one another in a way that will give us the greatest command over the knowledge we already have, and lead most directly to the acquisition of more. Given that these are its purposes, the general problem of classification may be stated thus: To bring it about that things are thought of in such groups, and those groups in such an order, as will best favour their laws' being discovered and remembered.

Unlike the other, this kind of classification refers only to •real objects and has nothing to do with •objects that are merely imaginable. It aims to organise our thinking about things whose properties we are already acquainted with—those things and no others. But it embraces *all* really existing objects. We can't constitute any class properly except in reference to a general division of the whole of nature; we can't determine which group object x can most conveniently be assigned to without taking into consideration all the varieties of existing objects, all at least which have any degree of affinity with x. No one family of plants or animals could have been rationally constituted except as part of a systematic arrangement of all plants or animals; and no such general arrangement could have been properly made without first determining the exact place of plants and animals in a general division of nature.

**§2.** There is no property of objects that we can't, if we please, use as the basis for a classification or mental grouping of those objects; and in our first attempts we're likely to select properties that are simple, easily conceived, and

perceptible on a first view that isn't prepared for by any previous process of thought. Thus Tournefort's arrangement of plants was based on the shape and divisions of the corolla; and what's commonly called the Linnæan (though Linnæus also suggested another and more scientific arrangement) was based mainly on the number of the stamens and pistils.

But these classifications, which are initially attractive because they make it easy to discover what class any individual belongs to, are usually not much good for the purpose of Classification that I am now talking about. The Linnæan arrangement is good for getting us to think together of all those kinds of plants that have the same number of stamens and pistils; but thinking of them in that way isn't much use because we seldom have anything to say about all and only the plants with a given number of stamens and pistils. . . . And as this property is of little importance or interest, remembering it accurately doesn't matter either. And ·it's worse than that·: by habitually thinking of plants in •those groups we're prevented from habitually thinking of them in •groups with more properties in common; so that this classification, if we stick to it, must do harm to our habits of thought.

The purposes of scientific classification are best served when a classification x surpasses any possible rival classification y because of two ways in which

> •the general propositions that can be made about x's groups surpass •the general propositions that can be made about y's groups,

namely that

> •there are more of them,. and
> •they are more important.

So objects should be classified (if possible) according to properties that are •causes of many other properties, or at least are sure •marks of them. Causes are preferable, as being the surest and most direct of marks, and also as being themselves the properties that it's most useful for us to focus on. But the property that is the •cause of the main special features a class is—unfortunately—usually not fitted to serve also as the ·mark or· diagnostic of the class. Instead of the cause, we usually have to select some of its more prominent effects, which may serve as marks of the other effects and of the cause.

A classification formed in this way is strictly scientific or philosophical, and is commonly called a 'natural' classification or arrangement, as against a 'technical' or 'artificial' one. The phrase 'natural classification' seems most appropriate for arrangements that correspond to the spontaneous tendencies of the mind, placing together in groups the objects most similar in their over-all appearance; in contrast to the 'technical' systems which, by grouping things according to their agreement in some arbitrarily selected feature, often put into the same group objects that have no over-all resemblance and put into different and remote groups objects that are very much alike. ·That is a remark about 'natural classification' considered intuitively as a phrase in everyday speech; but it also has a bearing on what is scientifically proper·. The claim of a classification to count as 'scientific' can be strongly supported by its being a 'natural classification' in this ·everyday informal· sense also. Why? Because the test of its scientific character is the number and importance of the properties shared by all objects included in a group; and properties on which the over-all appearance depends are (if only because of that dependence) •important, and in most cases •numerous. But although everyday naturalness is a strong recommendation it isn't something that a scientific classification *has* to have: it can happen that the most obvious properties of things may be of trifling importance compared with others that are

not obvious. The Linnæan classification has been accused of 'absurdity' in grouping the violet with the oak; actually, it *doesn't* do that, but it certainly does cut across natural affinities, and groups together things quite as unlike as the oak and the violet are. But the apparently wide difference that makes that grouping such a convincing example of a bad arrangement depends—to the layman's eye—mainly on mere size and texture; now if we insisted on finding the classification that would involve the least risk of similar groupings, we would return to the obsolete division into •trees, •shrubs, and •herbs. That is of primary importance with regard to over-all appearance, but it corresponds to so few differences in the other properties of plants. . . .that a classification based on it would be as completely artificial and technical as the Linnæan one—quite apart from the unclarity of its lines of demarcation.

Our natural groups, therefore, must be based on the *un*obvious properties of things when these are of greater importance. But in such cases there *must* be some other property or set of properties, more easily recognisable by the observer, that co-exist with the properties that are the real basis of the classification and can be used as marks of them. (A natural arrangement of animals must be mainly based on their internal structure, but (as Comte remarks) it would be absurd if we couldn't determine the genus and species of an animal without first killing it!) So De Blainville's zoological classification is probably the one we should favour: it is based on the differences in animals' coverings—skin, shell, rind, etc.—which correspond much more accurately than you might think to the really important differences, both in the other parts of the structure and in the habits and history of the animals.

This shows more strongly than ever *how much* we have to know about objects' properties if we are to make a good classification of them. One of the things that make such a classification useful is that by drawing attention to the properties that •it is based on and (if the classification is good) •are marks of many others, it helps us to discover those others; so we see how •our knowledge of things and •our classification of them tend to go on improving each other.

I said just now that the classification of objects should follow those of their properties that indicate not only the most numerous but also the most important special features. What does 'important' mean here? It's a relational term: a given feature is 'important' for one purpose, not for others; so the same objects may be classifiable in several different ways, each of them legitimate. Each science or art classifies things according to the properties that fall within its special realm or that it must take account in order to accomplish its special practical end. A farmer doesn't divide plants (like a botanist) into dicotyledons and monocotyledons, but into useful plants and weeds. A geologist divides fossils not (like a zoologist) into families corresponding to those of living species, but into fossils of the paleozoic, mesozoic, and tertiary periods, above the coal and below the coal, etc. Are whales fish? That depends on why we're thinking about them. Whewell again:

> 'If we are speaking of the internal structure and physiology of the animal, we must not call them "fish", for in these respects they deviate widely from fishes; they have warm blood, and produce and suckle their young as land quadrupeds do. But this wouldn't stop us from speaking of the "whale-fishery", and calling such animals "fish" on all occasions connected with this use; for what matters here is the animal's living in the water and being caught in a manner similar to other fishes. A plea that human laws that mention fish do not apply to whales would be rejected at once by an intelligent judge.'

These classifications are all good for the purposes of their own particular departments of knowledge or practice. But when we're studying objects not for any special practical end but for the sake of extending our knowledge of the whole of their properties and relations, we must consider as the most important attributes those that •contribute most, either by themselves or through their effects, to make the things like one another and unlike other things; •that give to the class composed of them the most marked individuality; •which fill (as it were) the largest space in their existence [Mill's phrase] and would most impress the attention of a spectator who knew all their properties but wasn't specially interested in any of them. Classes formed on this principle may be called, in a more emphatic way than any others, 'natural groups'.

**§3.** On the subject of these groups Whewell lays down a theory, based on an important truth, that he has expressed and illustrated very felicitously in some respects, but with what seems to me to be some admixture of error. Here is what he says:

> 'Natural groups are given by Type, not by Definition. . . And this accounts for the indefiniteness and indecision that we frequently find in the descriptions of such groups, and that must appear so strange and inconsistent to anyone who thinks they describe groups that the botanist has arbitrarily chosen. Thus we're told that in the family of the rose-tree the *ovules* are *very rarely* erect, the *stigmata* are *usually* simple. You might ask "What's the use of such loose accounts as these?" The answer is that they aren't inserted to •distinguish the species but to •describe the family, and the total relations of the ovules and the stigmata of the family are better known by this general statement. . . .
>
> 'These views—of classes determined by characters that can't be expressed in words—of propositions that state what happens not •in all cases but only •usually—of particulars that are included in a class though they don't fit the definition of it—may surprise you. They are contrary to many generally accepted opinions about the use of definitions and the nature of scientific propositions, and will probably strike many people as illogical and unphilosophical. But that's because the mathematical and mathematico-physical sciences have largely determined men's views of the general nature and form of scientific truth, while Natural History hasn't yet had time or opportunity to exert its influence on habits of philosophising. Its classifications and definitions are *much less* indefinite and inconsistent than any others apart from those of mathematics; and the ways in which approximations to exact distinctions and general truths have been made in Natural History may be worth studying, even for the light they throw on the best ways of pursuing truth of all kinds.
>
> 'Though in a natural group of objects a definition can no longer be of any use as a regulative principle, classes aren't left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary-line around it but by a central point within; not by what it strictly excludes, but by what it eminently includes; by an example, not by a rule; in short, we are directed not by a definition but by a Type. [As Whewell will show shortly, 'eminently' here means 'in a marked and prominent manner'; our topic here is Types, and 'eminently' goes with 'typically'.]

'A Type is an example of any class (e.g. a species of a genus) that is considered as eminently possessing the character of the class. The species that have a greater affinity with this type-species than with any others jointly form the genus, and are arranged around it, deviating from it in various directions and different degrees. Thus a genus may consist of several species that approach very near the type and have an obvious right to be in the genus, while there may be other species that straggle further from this central knot and yet are clearly more connected with it than with any other. There might be some species whose place is dubious—ones that appear to be equally bound to two generic types—but it's easy to see that this wouldn't destroy the reality of the generic groups, any more than the scattered trees of the intervening plain prevent us from speaking intelligibly of the distinct forests of two separate hills.

'The type-species of every genus. . . ., then, is one that possesses all the characters and properties of the genus in a marked and prominent manner. The type of the Rose family has alternate stipulate leaves, lacks albumen, has non-erect ovules and simple stigmata; and in addition to these features that distinguish it from the exceptions or varieties of its class, it has the features that make it prominent in its class. It is one of those that clearly possess several leading attributes. Thus, even when we can't say of any one genus that it must be the type of the family, or of any one species that it must be the type of the genus, we still know something about it: the type must be connected by many affinities with most of the others of its group; it must be near the centre of the crowd, not one of the stragglers.'

. . . .Whewell here states one of the principles of a Natural Classification; he does it very clearly and forcibly but without (I think) making all necessary distinctions. What this principle is, what its limits are, and how Whewell seems to me to have overstepped them, will appear after I have laid down another rule of Natural Arrangement, one that appears to me still more fundamental.

**§4.** You'll be familiar by now with something that I keep repeating because of the great confusion it is commonly involved in, namely the general truth that there are in nature

> distinctions of Kind; not •consisting in a given number of definite properties plus their effects but •running through the whole nature of the things so distinguished—running through their attributes generally.

Our knowledge of the properties of a Kind is never complete. We are always discovering, and expecting to discover, new ones. Where the distinction between two classes of things is not one of Kind, we expect to find their properties alike except where there's some reason for them to be different. In contrast with that, when the distinction is one of Kind, we expect to find the properties different unless there's some cause for their being the same. All knowledge of a Kind must be obtained by observation and experiment on the Kind itself. What about inferring some of its properties from the properties of other things that aren't connected with it by Kind? The most you'll get from that is the sort of presumption usually called an 'analogy', and generally a weak one at that.

The members of a true Kind have **countlessly many** common properties; there's no getting to the end of them; so the same is true of the general assertions that can be made about that kind, or that are certain to be made later on as our knowledge extends. ·Now put that beside this·: The

very first principle of natural classification is to form the classes so that the objects composing each class may have the **greatest number** of properties in common. This principle tells us that every natural classification should recognise and adopt into itself every distinction of Kind that exists among the objects it professes to classify. To •pass over any distinctions of Kind, •replacing them by definite distinctions [see Glossary] that don't point to underlying unknown differences would be to •pass over classes with more attributes in common, •replacing them by classes with fewer; and this would undermine the Natural Method of Classification.

That is why natural arrangements have always conformed to distinctions of Kind so far as these were known at the time; this has been true even when the makers of the arrangement •didn't have any sense of the distinction of Kinds and •were merely pushing ahead with their own inquiries.

The species of plants are not only real Kinds but are probably all real lowest Kinds, *infimæ species* [Latin]. We are free to subdivide any of them into smaller sub-classes, but the subdivision would necessarily be founded on *definite* distinctions, not pointing (apart from what may be known of their causes or effects) to any difference beyond themselves.

Because a natural classification is based on real Kinds, its groups are certainly not conventional: they don't depend on an arbitrary choice by the naturalist. But it doesn't follow, and I think it isn't true, that these classes are determined by a •type and not by •characters. To determine them by a type is as sure a way of missing the Kind as if we selected a set of characters arbitrarily. They are determined by characters, but these are not arbitrary. The problem is to find a few definite characters that point to the multitude of indefinite ones. Kinds are classes separated by an impassable barrier; we have to look for marks that will tell us which side of the barrier an object belongs on. The characters that will do this best are the ones we should choose; if they are also important in themselves, so much the better! When we have selected the characters we sort out the objects according to those characters and not, I think, according to their resemblance to a type. We don't compose the species Ranunculus acris out of all plants that are suitably like a model buttercup, but out of plants with •certain characters selected as marks by which we might recognise the possibility of a common parentage; and the list of •those characters is the definition of the species.

All Kinds are classes, of course, but it is *not* the case that all the classes in a natural arrangement must be Kinds. There aren't enough distinctions of Kinds to make up the whole of a classification. Very few of the genera of plants, or even of the families, can be pronounced with certainty to be Kinds. The great plant-distinctions

- •Vascular and Cellular,
- •Dicotyledonous and Monocotyledonous,
- •Exogenous or Endogenous,

are perhaps differences of kind: the lines of demarcation dividing those classes seem (though even on this I'm not sure) to go through the whole nature of the plants. But the different species of a genus, or different genera of a family, usually have only a limited number of characters in common. A Rose doesn't seem to differ from a Rubus, or the Umbelliferæ from the Ranunculaceæ, in anything much except the characters botanically assigned to those genera or those families. Unenumerated differences certainly do exist in some cases; there are families of plants which have their own special chemical composition, or yield products having special effects on the animal economy. The Cruciferæ and Fungi contain an unusual proportion of nitrogen; the Labiatæ are the chief sources of essential oils; the Solaneæ are very commonly narcotic, etc. In these and similar cases

there are possibly distinctions of Kind; but it's by no means indispensable that there should be. Genera and Families can be eminently natural although they are marked out from one another by a limited number of properties, provided •those properties are important and •the objects contained in each genus or family resemble each other more than they resemble anything outside the genus or family.

After recognising and defining the *infimæ species*, the next step is to arrange these into larger groups, making the groups correspond to Kinds wherever possible, but in most cases without any such guidance. And in doing this we are usually guided, as is natural and proper, by resemblance to a type. We form our groups around certain selected Kinds, each of which serves as a sort of exemplar of its group. But though the groups are suggested by types, I can't agree with Whewell

> •that when a group has been formed, it is determined by the type; •that in deciding whether a species belongs to the group we refer to the type and not to the characters; •that the characters 'can't be expressed in words' [see page 371].

This ·indented· assertion is inconsistent with Whewell's own statement of the basic principle of classification, namely that 'general assertions shall be possible' [quoted from Whewell's book, but not from the passage Mill quoted a few pages back]. If the class didn't have any characters in common, what general assertions about it could there be? *Nothing* could be predicated of the class except that they are all more like one another than like anything else. zx The truth is, on the contrary, that every genus or family is framed [see Glossary] with distinct reference to certain characters, and is composed first and principally of species that agree in having all those characters. To these are added, as a sort of appendix, the few other species that have nearly all the properties selected; some lack one property, some lack another, and while they agree with the rest almost as much as these agree with one another, they don't as closely resemble any other group. Our conception of the class continues to be based on the characters; and the class might be defined as 'things that either •have that set of characters or •resemble the things that do so more than they resemble anything else'.

This resemblance isn't an ultimate unanalysable fact like the resemblance between simple sensations. Even distant resemblances are created by the possession of common characters. Whatever resembles the genus Rose more than it resembles any other genus does so because it possesses more of the characters of that genus than of any other genus. And there can't be any real difficulty in representing by a list of characters the nature and degree of the resemblance that strictly entitles any object to be included in the class. There are always some properties common to all the included things. There will often be properties that some included things *don't* have; but the objects that don't have one property do have the others; the resemblance that fails in some particulars must be made up for in others. So the class is constituted by the possession of •**all** the characters that are universal and •**most** of the others, i.e the ones that allow exceptions. If a plant had erect ovules, divided stigmata, no albumen, and no stipules, it might not be classed among the Rosaceæ. But it could lack any one (or perhaps more than one) of these characteristics and still not be excluded. The purposes of a scientific classification are better served by including it. Because it agrees so nearly, in its known properties, with the sum of the characters of the class, it is likely to resemble that class more than any other in such of its properties as haven't yet been discovered.

So natural groups are determined by characters just as much as artificial classes are. . . . But what are relevant are not merely •the characters that are rigorously common to all the objects included in the group, but •the entire body of characters such that: most members of the class have all of them, and all members of the class have most of them. That's why our conception of the class—the image in our minds that represents it—is that of a specimen that has *all* the characters; most naturally a specimen which, by having them all in the highest degree they are ever found, is the best fitted to show clearly and decisively what they are. It is by a mental reference to this standard that we usually and advantageously determine whether any individual or species belongs to the class; but this standard isn't a substitute for the definition of the class—it's an illustration of it. And this, as it seems to me, is all the truth there is in the doctrine of Types. . . .

**§5.** A Nomenclature in science is (I repeat) a system of the names of Kinds. These names, like other class-names, are defined by the list of characters distinctive of the class. The only merit a set of names can have beyond this is for them to be constructed in such a way as to give as much information as possible: so that a person who knows the thing can get all the help the name can give in remembering what he knows; while someone who doesn't know it can get—merely by being told its name—as much knowledge about it as the case admits of.

There are two ways of giving the name of a Kind this sort of significance. The better of them is when the word can be made to indicate by its formation the very properties it is designed to connote. The name of a Kind doesn't (of course) connote all its properties because these are inexhaustible; it connotes enough of its properties to distinguish it—ones that are sure marks of all the rest. Now, it rarely happens that one property, or even two or three, can do this. To distinguish the common daisy from all other species of plants would require a very long list of characters, and a usable name can't indicate more than a very small number of these by its etymology or mode of construction. So it's likely that only one ideally perfect Nomenclature is even *possible*; and it's one that we fortunately have an approximation to—namely the Nomenclature of elementary Chemistry. The substances, whether simple or compound, that chemistry deals with are Kinds, and therefore the properties distinguishing each of them from the rest are innumerable; but in the case of compound substances there is one property, the chemical composition, that is by itself sufficient to distinguish the Kind, and it is (with certain reservations not yet thoroughly understood) a sure mark of all the other properties of the compound. (As for simple chemical substances: there aren't enough of them to require a systematic nomenclature.) All that was needed, therefore, was to make the name of every compound declare its chemical composition *on the first hearing*, i.e. to form the name of the compound in a uniform way from the names of the simple substances that are its elements. This was done skillfully and successfully by the French chemists, though their nomenclature has become inconvenient for naming the very complicated compounds now known to chemists. The only thing their nomenclature didn't indicate was the exact proportion in which the elements were combined; and the establishment of the atomic theory made it possible to fill this gap.

But where too many characters are needed to pin down the Kind, and where no one of them is so important as to justify its being singled out to be so indicated [the last clause is verbatim from Mill], there's a subsidiary procedure that can help us out. Though we can't indicate the distinctive

properties of the Kind, we may be able to indicate the Kinds that are most like it by incorporating into its name the name of the proximate [see Glossary] natural group of which it is one of the species. This is the basis for the admirable binary nomenclature of botany and zoology, in which the name of every species consists of •the name of the genus or natural group next above it, with •a word added to distinguish the particular species. The second part of the compound name is •sometimes taken from one of the features in which that species differs from others of the genus;. . . .sometimes from a detail of an historical nature;. . . . and sometimes the word is purely conventional. . . . It doesn't matter much which of these is used, because the second part—the 'specific name'—could at most express, independently of convention, no more than a very small portion of the connotation of the term. . . .

This principle of nomenclature helpfully gives to the names of species the greatest amount of independent significance that the circumstances of the case admit of, and further helps us by immensely economising the use of names and saving us from an intolerable burden on the memory. . . .

## Chapter 8. Classification by series

**§1.** Up to here I have considered the principles of scientific classification only in relation to the formation of natural groups; and this has been the stopping-point for Whewell and most of the others who have attempted a theory of natural arrangement. But there's another part to the theory, concerning the arrangement of the natural groups into a natural series. This is as important as the first part, yet as far as I know it hasn't been systematically discussed by anyone except Comte.

As I have already said, the purpose of Classification as an instrument for the investigation of nature is to make us group in our minds objects that •have the greatest number of important common properties and therefore •need to be considered together when we are performing inductions. Our ideas of objects are thus ordered in the way that is best for the successful conduct of inductive inquiries **generally**. But when the purpose is to conduct some **particular** inductive inquiry, more is required. To be helpful with *that*, a classification must bring together those objects the simultaneous contemplation of which is likely to throw most light on the particular subject. Because that subject is the laws of some phenomenon or some set of connected phenomena, that phenomenon or set of phenomena must be chosen as the basis for the classification.

If a classification is to help the study of a particular phenomenon it must **(i)** bring into one class all Kinds of things that exhibit that phenomenon in some form and some degree, and **(ii)** arrange those Kinds in a series according to the degree in which they exhibit it, from those that exhibit most of it right down to those that exhibit least. The principal example of such a classification, so far, is presented by comparative anatomy and physiology; and that's where my examples will come from.

**§2.** Suppose we want to investigate the laws of animal life. After forming the clearest conception of the phenomenon itself that is possible in the present state of our knowledge, we must **(i)** erect into one great class (that of animals) all the known Kinds of beings where animal life presents itself,

in whatever degree and whatever combinations with other properties. Because some of these Kinds manifest the general phenomenon of animal life in a very high degree, and others in a low degree that is barely sufficient for recognition, we must next **(ii)** arrange the various Kinds in a series, following one another according to the degrees in which they exhibit the phenomenon—starting with man and ending with the most imperfect kinds of zoophytes.

This is merely to say that we should put the instances that the law is to be inductively collected from into the order implied in the fourth method of experimental inquiry discussed in Book III, namely the method of Concomitant Variations. As I said back there, this is often the only method we can use with assurance of a true conclusion in cases where we can't do much in the way of artificial experiments to separate features that are usually conjoined. The principle of the method is that

> facts that increase or diminish together, and disappear together, are either •cause and effect or •effects of a common cause.

When we know that that relation really does hold between the variations, we can confidently assert that the facts themselves are connected, this being either a law of nature or a mere empirical law, according to circumstances.

It is simply *obvious* that the use of this method must be preceded by the formation of a series such as I have described; and the mere arrangement of a set of objects in a series according to the degrees in which they exhibit some fact whose law we are seeking is too naturally suggested by the needs of our inductive operations to require much illustration here. But in some cases the arrangement required for the •special purpose becomes the determining principle of the classification of the same objects for •general purposes. This will naturally and properly happen when the laws that the •special inquiry aims to discover play such a big part in the •general character and history of the objects whose laws they are—exercise so much influence in determining all the phenomena of which the objects are either the agents or the theatre[1]—that all other differences among the objects should be regarded as mere modifications of the one phenomenon sought, effects caused by the interplay between some incidental circumstance and the laws of that phenomenon. Thus the differences between one class of animals and another may reasonably be considered as mere modifications of the general phenomenon *animal life*, modifications arising either

- •from the different degrees in which that phenomenon is manifested in different animals, or
- •from the intermixture of the effects of incidental causes that are special to the nature of each;

in the latter case the effects are produced by the general laws of life, which still exercise a predominant influence over the result. If the picture I have drawn is correct, no inductive inquiry about animals can be successful unless it is subordinate to the great inquiry into the universal laws of animal life; and the classification of animals best suited to that one purpose is the most suitable to all the other purposes of zoological science.

**§3.** To establish a classification of this sort, or even to understand it after it has been established, we need to be able to recognise the essential similarity among all the items that are classed together, including the lower degrees and obscurer forms of it; i.e. to identify with one another all phenomena that differ only in •degree and in •properties

[1] [This phrase refers to two ways in which an object may be crucial to a sequence of events: •as the cause of the events or •as the stage on which the events run their course.]

that we suppose to be caused by difference of degree. In order to recognise this identity—i.e. this exact similarity—of quality, we have to have a *type-species*. We must consider as the *type* [see Glossary] of the class the one amongst its member Kinds that exhibits *in the highest degree* the properties that constitute the class, regarding the other varieties as. . . .deviations from it because of their inferior intensity of the characteristic properties. Other things being equal, every phenomenon is best studied where it exists in the greatest intensity. That's where the effects that depend either on •it or on •its causes will also exist in the highest degree. It's the only place where those effects can become fully known to us, enabling us to learn to recognise their lower degrees, or even their mere rudiments, in cases where direct study of them would have been difficult or impossible. Not to mention that the phenomenon in its higher degrees may have effects or collateral details that don't occur at all in its lower degrees. . . . Consider for example *man*, the species in which the phenomena *animal* and *organic life* exist in the highest degree. Many subordinate phenomena develop in the course of man's animated existence but don't appear at all in the lower animals; yet knowledge of these properties may greatly help us to discover the conditions and laws of the general phenomenon of *life* that man shares with the lower animals. . . .

**§4.** The remaining task in this Book is to consider what internal distribution of the series is best—i.e. how best to divide it into Orders, Families, and Genera.

The main principle of division must of course be natural affinity; **(a)** the classes formed must be natural groups; and I have already said enough about how these are to be formed. But the principles of natural •grouping must be applied in subordination to the principle of a natural •series: the groups must not be constituted in a way that places together in one group things that ought to be at different points on the general scale. The precaution we have to take is this:

> **(b)** The primary divisions must be based not on •all distinctions indiscriminately but on •those that correspond to variations in the degree of the main phenomenon.

The *animated nature* series should be broken into parts at the points where the variation in the degree of intensity of the main phenomenon (as marked by its principal characters, Sensation, Thought, Voluntary Motion, etc.) starts to show up in conspicuous changes in the various properties of the animal. Such well-marked changes occur where the class Mammalia ends; at the points where Fishes are separated from Insects, Insects from Mollusca, etc. When the primary natural groups are formed in that way they'll compose the series by mere juxtaposition, without redistribution; each of them corresponding to a definite portion of the scale. Similarly, each family should, if possible, be subdivided in such a way that one part of it stands higher and the other lower, though of course contiguous, in the general scale; and only when this is impossible is it allowable to base the remaining subdivisions on characters that have no determinable connection with the main phenomenon.

Where the principal phenomenon is vastly more important than all other properties a classification could be based on—as it is in the case of *animated existence*—any considerable deviation from rule **(b)** is in general sufficiently guarded against by the principle **(a)** that the groups are to be ·natural, i.e.· formed according to the most important characters. All attempts at a scientific classification of animals, since their anatomy and physiology were first successfully

studied, have been made with a certain degree of instinctive reference to a natural series, and have agreed more than they have disagreed with **(b)** the classification that would most naturally have been based on such a series. But the agreement hasn't always been complete; and there's still discussion about which of several classifications best fits the true scale of intensity of the main phenomenon. [Mill gives the example of a classification based on how animals get their nourishment, and agrees with the criticism that] carnivorous and herbivorous animals are found at almost every degree in the scale of animal perfection. . . .

**§5.** *Animals* constitute the only large part of nature that it has been found practically possible to classify in conformity with principles **(a)** and **(b)**. In the case of vegetables [= plants generally] the natural arrangement hasn't been taken beyond **(a)** the formation of natural groups. Naturalists haven't been—and probably won't ever be—able to **(b)** form those groups into any series whose terms correspond to real gradations in the phenomenon of vegetative or organic life. Such a difference of degree can be traced between •vascular plants and •cellular plants; the latter class includes lichens, algæ, and other substances whose organisation is simpler and more rudimentary than that of the higher order of vegetables, which brings them nearer to mere inorganic nature. But when we rise much above this point, we don't find any big differences in the degree to which different plants have the properties of organisation and life. The dicotyledons have a more complex structure and somewhat more perfect organisation than the monocotyledons; and some dicotyledonous families, such as the *Compositæ*, are rather more complex in their organisation than the rest. But the differences are not conspicuous and don't promise to throw any particular light on the conditions and laws of vegetable life and development. If they did, the classification of vegetables would have to be made, like that of animals, with reference to **(b)** the scale or series indicated.

The scientific arrangements of organic nature provide us with our only complete example, so far, of the true principles of rational classification in the formation **(a)** of groups and **(b)** of series; but those principles are applicable to *all* cases where mankind are called on to get a mentally orderly grasp of *any* extensive subject. When our concern is with art or business, the principles are as much to the point when we are doing science. The proper arrangement of a code of laws, for example, depends on the same scientific conditions [Mill's phrase] as the classifications in natural history; and there couldn't be a better preparation for that important ·legal· task than the study of the principles of a natural arrangement not only •in the abstract but also •in their actual application to the class of phenomena for which they were first elaborated, and which are still the best school for learning their use. Bentham was perfectly aware of this the great authority on classification; and his early *Fragment on Government*—an admirable introduction to a series of writings unequalled in their department—contains clear, sound views (as far as they go) on the meaning of a natural arrangement. Such views could scarcely have occurred to anyone who lived before the time of Linnæus.