4 Almost the first book which I seriously studied that offered any difficulty was Kant's Critik der reinen Vernunft. Somebody is likely to tell you that I betray my ignorance in writing "Critik" instead of "Kritik." I shall not defend myself. I spell it as Kant himself spelled it in his first edition, — which in some respects I thought was the better of the two, — and I studied both until I very nearly knew both by heart. But I particularly like the old-fashioned spelling Critik, because it calls to mind the source from which Kant got the word, namely from the English of Locke, who used "critick" to mean the science of criticism, and Kant in his long preface is most careful to explain that that is what he means by "Critik," and that he does not mean by it a critique, which has not prevented ~~to~~ the title of the work being translated "Critique of Pure Reason,"

page 1

Milford, Pa.
1911 June 22

Dear Mr. Kehler:

Yours of the 20th inst has reached me this afternoon. It informs me that the Hon. Lady Welby has written you many letters and has so often been kind enough to mention me that you would like to know who I am and what I have done. I am a person 72 years old. I have accomplished nothing in particular.

But since Lady Welby seems, from what you say, to desire that you and I should be acquainted, and I think ~~anybody~~ a gentleman to whom she has written many letters must be a very desirable acquaintance, I dare say that my 72 years have equipped me with sufficient garrulity to develope a voluminous epistle by way of conveying to you a notion of the particular variety of nonentity that I can boast myself.

When I was thirteen years old, being one day in the room of my elder brother, I picked up from his table a copy of Whately's Logic, and asked him what

2a

logic" was. Being answered, I stretched myself out on the carpet with the book, and in a few days had mastered all the Archbishop had to say. From that week until I had reached my three score years and ten the central passion of my being was to find out, — not by any means what passed in my organism and my consciousness when I thought, but something anterior to all such knowledge, namely, what are the fundamentally different ways of reasoning, what kind and degree of assurance each could supply, and under precisely what conditions, and by what methods to proceed in order to gain such knowledge as is possible for human beings. The more I studied this subject the more and more deeply I felt the shocking levity and looseness of thought with which these basic questions had been treated, and although in modern times men have reasoned much better than

3

ever before concerning whatever subjects they really have seriously investigated, and although if a man works soberly and zealously enough over any subject, and has the means of detecting the errors into which he will get seduced into, he will become very expert in reasoning about that particular subject, or any other sufficiently like that, yet outside the particular corner of science that ~~they have~~ each one has learned how to make a particular kind of investigation, the stupendous errors into which they have fallen about reasoning and the slap-dash fashion ~~with~~ in which they have taken up opinions which if they had only questioned them and tried to doubt them they would soon have seen to be utterly false, — all these errors seem to be more and more glaring the more triumphantly truth is pursued in those branches in which it has been seriously, ardently pursued.

and call one of them the <u>value</u> of any assertion that is true, and the other the <u>value</u> of any assertion that is not altogether true. Let v (for <u>verum</u>) be the former and f (for falsum) be the latter. These two numbers being different, of course, no assertion, x, can be equal to v and also equal to f. But it must have one or the other value; and this principle can be represented most simply by writing

$$(v-x)(x-f)=0.$$

For the product of the two factors $v-x$ and $x-f$ being zero, one or other of them must be zero. That is either $v-x=0$ and $x=v$, or else $x-f=0$ and $x=f$. So that this equation may be adopted as the general expression of the principle that every assertion is either true or false. Here, then, is a machinery by which a certain kind of reasoning may be tested. For instance, if x and y represent two states of things, then $(y-f)(v-x)=0$ will express that if y is true x must be true; or, in other words, if y is not false, x must be true. In like manner, $(z-f)(v-y)=0$ will express that if z is true y must be so. Now from the two equations let us eliminate y. For that purpose, multiply the first by $(z-f)$ and the second by $(v-x)$. We so obtain

where there are two errors; for another is representing Kant as criticising <u>reason</u>. That is downright ridiculous. It is not <u>reason</u> that he thinks is unreasonable, but only <u>The</u> reason, i.e. the human faculty & its effort to attain reason. It was a stupendous work, though naturally in great part erroneous. But the strange illustration it affords of the truth of what I was saying in my three first pages of this letter is that although he makes the great abstract ideas which he calls "categories" to be derived from the logical forms of assertions, which he calls in "Prolegomena zur Metaphysik" §21, and elsewhere "die verschiedenen Momente des Verstandes in Urtheilen," it is evident that, according to him, the correctness of his results depends entirely on his getting his list of these "moments" or "functions", or logical forms of assertion rightly constituted. Well, I am tolerably confident that I cannot be mistaken

6 in saying that Kant never read a book of logic in his life. For it is well known that he read nothing of that sort in his later life; and when — I think it was when he was first appointed Privat-Docent, — in order (quite evidently to my mind,) to attract students to lectures on an uninviting subject, he got out a pamphlet called "Ueber die falsche Fitzspindigkeit der vier syllogistischen Figuren," and it is truly comical to see how that — skit, I should call it, — has been lauded to the skies by philosophical Germans. Kant evidently fancied it was quite original. In fact, it is not only entirely given in Aristotle's Analytics, but it happens to relate to a branch of the subject that throughout the middle ages from the middle of the XIII th century was dilated upon by every Doctor of the schools who touched upon logic; & the whole doctrine is detailed in the "Summulae logicales" which was put into every boy's hands as soon as he was through with grammar. So that to suppose Kant was not a conscious "faker" it is necessary to conclude that he was utterly ignorant of these best known parts of Aristotelian logic.

Now I was saying how essential it was to Kant's purpose to get the forms of judgments right. But he dismisses the subject in half a dozen

7 very short pages, & in arguments so weak that it needs all that one knows of the extraordinary power of Kant's thought not to speak of it with unqualified contempt. He doesn't really come within sight of the real difficulties of the subject. I spent two years, absolutely solid, on nothing but the study of Kant, — chiefly the C.d.r.V. I read every medieval scholastic work that I could procure, after I had read everything of a logical or philosophical nature that has been preserved of the Greeks. These people seem to have been the only ones who ever constructed an original logical doctrine; and they constructed several, none of which, however, are thoroughly sound. I also read all I could get hold of of a logical nature of modern origin. But all of this, except what originated in mathematical thought, is so loosely thought as to tempt contempt. The mathematical genius George Boole, who, like the rest, only reasons accurately when he has mathematical machinery to hold him straight, invented a method of very limited applicability that has real merit. I will make a statement here of the essence of it freed from the dross. Boole would not have approved of the statement. Yet it brings out his one just logical idea. Suppose we take ~~two~~ any two numbers

12 In order to make the lines of identity in their connection with shading and its absence perfectly perspicuous, I must provide you with a bit or two of nomenclature. By an "area", then, I mean the whole of any continuous part of the surface on which graphs are scribed that is alike in all parts of it either shaded or unshaded. By a "graph" I mean the way in which a given assertion is scribed. It is the general ~~ch~~ kind not a single instance. For example there is in English but one single "word" that serves as definite article. It is the word "the". It will occur some twenty or more times on an average page; and when an editor asks for an article of so many thousand "words" he means to count each of those instances as a distinct word. He speaks loosely ~~speaking~~ of instances of words as words, which they are not. Now in like manner a graph is one thing and a "graph instance" is another thing. Any expression of an assertion in this particular diagrammatic syntax is an Existential Graph, of which I use the single word

9

$$(z-f)(y-f)(v-x)=0$$

$$(z-f)(v-y)(v-x)=0$$

Adding these two equations, we get

$$(z-f)(v-f)(v-x)=0$$

But the middle factor, $v-f$, cannot be zero, since v and f are different numbers. Consequently, we have a right to divide by this middle factor; and doing so we get

$$(z-f)(v-x)=0$$

which means, If z is true, x is true, the correct conclusion from the premisses.

Boole's book Laws of Thought in which his system is developed and is applied to probabilities, is a work of genius, in which much is true and much false or confused to the point of meaninglessness.

One of my earliest works was an enlargement of Boole's idea so as to take into account ideas

10 of relation, — or at least of all
ideas of existential relation ...
By an existential relation, I
mean any relation, R, such tha[t]
anything that is R to X (where
X is some particular kind o[f]
object) is non-existent in ca[se]
X is non-existent. Thus love
of women of bright green complex[ion]
are non existent in case the[re]
are no such women.

I invented several differ[ent]
systems of signs to deal with ...

11 rules) makes erasures and insertions, he will read before his eyes ~~the~~ a necessary conclusion from those premisses. This sys[tem] That is so simple that I will describe it. Every word makes ... assertion. Thus —man means "there is a man" in what[ever] universe the whole sheet refers to. The dash before "man" ... "line of identity" man / eats—man this means "Some man ... a man." To deny that ... is any phoenix, we shade that assertion ~~the~~ which we deny ... a whole ~~phoenix~~ Thus what I have just scribed means "It is false that there is a phoenix." But the following ... only means "there is something that is not identical with any phoenix." ~~Fig~~. 3. denies Fig 4, which asserts that it ~~sometimes~~ thunders without lightening. ~~Cause~~ For a denial shades the unshaded and unshades the shaded. Consequently Fig. 3 means "~~If~~ it ~~lightens, it thunders~~ thunders, it lightens."

thunder
lightening
Fig. 4.

thunder
lightening
Fig. 3.

10 of relation, — or at least of all ideas of existential relation. By an existential relation I mean any relation, R, such that anything that is R to x (where x is some particular kind of object) is non-existent in case x is non-existent. Thus lovers of women of bright green complexions are non existent in case there are no such women.

I invented several different systems of signs to deal with relations. One of them is called the general algebra of Relations, and another the algebra of dyadic relations, ~~[illegible]~~ which I was finally led to prefer to what I call a diagrammatic syntax. This is a way of setting down on paper any assertion, however intricate, and if one so sets down any premisses, and then (guided by 3 simple

11 rules) makes erasures and insertions, he will read before his eyes a necessary conclusion from these premisses. This syntax is so simple that I will describe it. Every word makes an assertion. Thus —man means "there is a man" in whatever universe the whole sheet refers to. The dash before "man" [illegible] "line of identity"

man
eats—man

this means "Some man [illegible] a man." To deny that [illegible] is any phoenix, we shade that assertion which we deny as a whole

~~—phoenix~~

Thus what I have just scribed means "It is false that there is a phoenix. But the following only means "there is something that is not identical with any phoenix. Fig. 3. denies Fig 4, which asserts that it ~~sometimes~~ thunders without lightening. ~~Cause~~ For a denial shades the unshaded and unshades the shaded Consequently Fig. 3 means "If it ~~lightens, it thunders~~ thunders, it lightens."

16 The rule of interpretation which necessarily follows from the diagrammatization is that the interpretation is "endoporeutic" (or proceeds inwardly) That is to say a ligature denotes "something" or "anything not" according as its <u>outermost</u> <u>part</u> lies on an unshaded or a shaded area respectively.

There are three simple rules for modifying premisses when they have once been scribed in order to get any sound necessary conclusion from them. Of course I do not count among these rules two recommendations which are nevertheless of the highest importance. One is to be sure to scribe every premiss that is really pertinent to the conclusion one aims at. The other is to scribe them with sufficient analysis of their meaning, and not by any means to neglect <u>abstractions</u> which modern philosophers think most <u>foolishly</u> are of little or no importance or are even <u>unreal</u> because they are of the nature of <u>signs</u>. They tell us that it is we who create the laws of nature! That is <u>Real</u> which is true just the same whether you or I or any collection of persons <u>think</u> or otherwise <u>think</u> it true or not. The planets were always accelerated toward the sun for millions of years before any finite mind was in being to have any opinion on the subject. Therefore the law of gravitation is a <u>Reality</u>.

13 "<u>graph</u>" as a commodious abbreviation as long as I have nothing to do with another kind of graph. A graph then may be complex or indivisible. Thus

male
human
African

is a graph-instance composed of instances of three indivisible graphs which assert "there is a male" "there is something human" and "there is an African." The syntactic junction or point of teridentity asserts the identity of something denoted by all three. Indivisible graphs usually carry "<u>pegs</u>" which are places on their periphery appropriated to denote, each of them, one of the subjects of the graph. A graph like "thunders" is called a "<u>medad</u>" as having no peg (though one might have made it mean "<u>some time</u> it thunders" when it would require a peg.) *

~~A graph or graph instance of pegs~~

A graph or graph instance having 0 peg is a <u>Medad</u>
" " " " " " 1 peg <u>Monad</u>
" " " " " " 2 pegs <u>dyad</u>
" " " " " " 3 " <u>triad</u>,

Every indivisible graph instance must

14 be wholly contained in a single area.
The line of identity can be regarded as ~~as~~ a graph composed of any number of dyads "—is—" or as a single dyad. But it must be wholly in one area. Yet it may abut upon another line of identity in another area. Thus Fig. 5 denies that there is a man that will not die, that is, it asserts that Every man (if there be such an animal) will die.

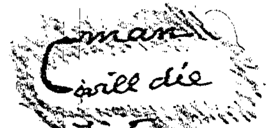


Fig 5

It contains two lines of identity. It denies Fig. 6. which asserts, "There is a man that is something that is something that is not anything that is anything unless it be something that will not die." I state the meaning in this way to show how the ~~continu~~ identity is continuous regardless of shading; and this is necessarily the case. It is the nature of identity. That is its entire meaning. For the shading denies the whole of what is in its area but not each part except disjunctively. ~~There~~ Fig 6 may be read, "There is a man that is identical with something that is not identical with anything or only with something that is not identical with anything unless it will not die."

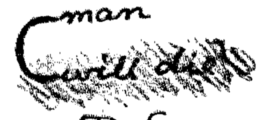


Fig. 6.

15 I dwell on these details which from our ordinary point of view appear unspeakably trifling, — not to say idiotic, — because they go to show that this syntax is truly diagrammatic, that is to say that its parts are really related to one another in forms of relation analogous to those of the assertions they represent, and that consequently in studying this syntax we may be assured that we are studying the real relations of the parts of the assertions and reasonings; which is by no means the case with the syntax of speech.

A line which is composed of two or more lines of identity abutting on one another is called a "ligature." Of course it is not a graph, of itself. Or it may be regarded as a graph meaning either "nothing is anything that is anything that is" (in case the shaded end is exterior to the unshaded end) or "something is identical with something that is not identical with anything but what" (in case the shaded end lies in an area enclosed by the unshaded area where the other end is, 20 They might try to crawl out of this absurdity by saying that they do not state the ~~syllogisms~~ premisses as

Any Phoenix there may be is a bird
Any Phoenix there may be rises from its ashes

but

Every Phoenix there is is a bird, etc.

But the reply to that ~~is that Aristotle~~ is that (passing over the fact that Sir Wm. Hamilton, lauded as the highest of authorities insists that Any and not Every is the right word) is that by "Contradictories" they mean two propositions which, by their very meaning, can neither both be true nor both false, and they all agree that every simple proposition has a simple contradictory, and that the contradictory of "Some S is not P" is "Any, all, or every (Greek πᾶς) S is P." Now if this latter implied the existence of some S, Every S is P and Some S is not P could both be false by there not existing any S. That would be a much graver fault with their logic than that which I charge against it. For I only charge that two "moods" or species of syllogism are false (i.e. not necessary, as they profess to be.) And curiously Aristotle never mentions these with examples, as he does in all other cases; but merely says ——— But this letter will be long enough without discussing Aristotle and the Greek commentators, a subject on which I should soon tire you, interesting as it is to me.

So I will break off that and just give an illustration or two of how this Syntax of Existential Graphs works. But before doing that I wish to draw your attention, in the most emphatic way possible, to the purpose this Syntax is intended to subserve; since anybody who did not pay attention to that statement would be ~~sure~~ all but sure, not merely to mis-

I do not say that Newton's formulation of the law is quite right, because when Newcomb was at work on the Inferior planets, Mercury & Venus, I wrote to him and called his attention to the fact that certain motions of Mercury, etc. show that the attraction is not precisely inversely as the 2nd power of the distance but is rather proportional to the −2.01 power or thereabouts; and I see that in his Tables not only of Mercury but also of Venus he has introduced such a correction; [He says he introduces it to make his tables accord with observation with Newton's Law. But does not say that the cause of the discrepancy of observation [illegible] that is the only way [illegible]] I had not supposed it would be perceptible in so circular an orbit as that of Venus. No doubt all our other formulations of laws are merely approximate, but the laws, as they really are, are Real.

I will now state what modifications are permissible in any graph we may have scribed.

1st Permission. Any graph-instance may be erased on an unshaded area, and on a shaded area that already exists, any graph-instance may be inserted. This includes the right to cut any line of identity on an unshaded area and to prolong one or join two on a shaded area. [The shading itself must not be erased, of course, because it is not a graph-instance.]

18 <u>2nd Permission</u>. Any graph-instance may be <u>iterated</u> (i.e. duplicated) in the same area or in any area enclosed within that, provided the new lines of identity so introduced have identically the same connexions they had before the iteration. And if any graph-instance is already duplicated in the same area or in two areas one of which is included (whether immediately or not) within the other, their connexions being identical, then the inner of the instances (or either of them if they are in the same area) may be erased. This is called the Rule of Iteration and Deiteration.

3rd <u>Permission</u>. Any ring-shaped area which is entirely vacant may be suppressed by extending the areas within and without it so that they form one. And a vacant ring shaped area may be created in any area by shading or by altering shading so as to separate two parts of any area by the new ring shaped area.

It is evident that neither of these

19 three principles will ever permit one to assert more than he has already asserted. I will give examples the consideration of which will suffice to convince you of this.

boy
industrious

Fig. 7

boy
industrious

Fig. 8.

known for certain
communication with

Fig. 9

communication with
deceased
known for certain

Fig. 10.

Fig. 7 asserts that some boy is industrious. By the 1st permission it can be changed to Fig. 8, which asserts that there is a boy and that there is an industrious person. This was asserted in Fig. 7, together with the identity of some case. Fig. 9 asserts either there is nothing known for certain or else there is no communication with anybody. By the same permission this can be changed to Fig. 10 which asserts that no communication with anybody deceased is known for certain. But this is fully included in the state of things asserted in Fig. 9.

In illustrating the application of the Second Permission, I am obliged to notice one of the faults of the system of logic which has been taught to every generation of young men for some sixty odd generations. One of the syllogisms that they have all been taught is a sound apodictic argument ~~calle~~ called <u>Darapti</u>, and whose validity nobody has questioned furnishes a fair sample of the quality of intellect of the Doctors and Regents of the world's most famous and proudest Universities. Here is a sample of it.

Any Phoenix would be a bird
Any Phoenix rises from her own ashes
∴ Some bird rises from its own ashes.

~~take~~ the intention of this sytax, but to think that intention as CONTRARY to what it really is as well he could. Namely he would suppose the object was to reach the conclusion from given premisses with the utmost facility and speed, while the real purpose is to dissect the reasoning into the greatest possible number of distinct steps and so to force attention to every requisite of the reasoning. The supposed purpose would be of little consequence, and it is the business of the mathematicians to furnish inventions to attain it; but the real purpose is to supply a real and crying need, although logicians are so stupid as not to recognize it and to put obstacles in the way of meeting it.

I will now, by way of an example of the way of working with this syntax, ~~and~~ show how by successive steps of inference to pass from the premisses of a simple syllogism to its conclusion. Fig. 11 shows the two premisses "Any M is P" and "Any S is M."

Fig. 11

23 By the space that I have occupied in explaining this syntax, you will surely think it is my chief work. On the contrary, it is one of the smallest; but it is the only one of which I could put you into a position to gain some understanding without writing a book about it.

You will ask perhaps "If all one has to does to avail oneself of those 3 permissions, how is it that mathematics (which is nothing but deductive reasoning) is so difficult and demands high genius? There are several circumstances which go to clearing up this. The first of these is that the mathematician is not supplied in advance with a definite list of premisses; nor is he asked whether or not a definite conclusion can or cannot be drawn. His usual first approach to a problem is something like the following entirely fanciful situation which serves to illustrate what one of his difficulties is like. An astronomer comes to a mathematician and says, "I want to consult you about something." — But hold! I can perfectly well substitute a historical case about which I am fully informed. Toward the end of October 1604 the astronomer Tycho Brahe died and left a mass of observations including continual measurements of the apparent places of the planet Mars extending over 15 years. Keppler who was a remarkable mathematician and who had had the advantage of training in observations under Tycho, had possession

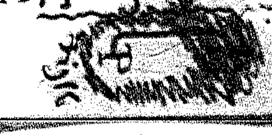
No 22

The first step consists in raising to Fig. 12 by the 2nd Permission

Fig 12 Fig. 13 Fig 14 Fig 15

The second step is simply to erase by the 1st Permission. The third step is to join the two ligatures by the 1st Permission as shown in Fig 13.

It will be observed that in iterating the major premiss, I had a right to put the new graph instance at any part of the area into which I put it; and I took good care to have the ligature of the minor premiss touch the shaded area of the iterated graph instance. Now by the 1st Permission I have a right to insert what I please into a shaded area, and without making the new line of junction leave the shaded area, I make it touch the unshaded line of identity of the minor premiss. This gives me a right in the fourth step to deiterate M, so as to give Fig. 14 by the second permission. The fifth step is to delete the M on an unshaded field giving Fig. 15 while the Sixth step authorized by permission the third consists in getting rid of the empty ring shaped shaded area round the P, giving Fig. 16



moon's longitude) that it requires a
most capacious brain to embrace
the question. Finally there comes a diffi-
culty in many problems which has led
me to divide mathematical reasonings
into the corollarial deductions and
the theorematic deductions. The terms
"corollary" and "theorem" have no defi-
nite meanings and never did have. The
original "theorems" of geometry were
those propositions that Euclid proved,
while the corollaries were simple
deductions from the theorems inserted
by Euclid's commentators and editors.
They are said to have been marked by the
figure of a little garland (or corolla,) in
the margin. But I use the adjectives
which I form from the words "theorem" and
"corollary" with exact meanings. The ulti-
mate premisses of geometry are called by present
day geometers "hypotheses", because the
mathematicians, as such, do not accept
any responsibility for their truth. They are
of three kinds, definitions, axioms, and
postulates. The axioms are, in my opinion,
all false, if one insists on their rigid accuracy,
in all cases. The "postulates" were originally
understood to be premisses expressing
that certain lines could be drawn, though
everybody knew they could not, exactly.
But in my opinion it is far better to consider
them as statements that space contains cer-
tain kinds of places. For instance, the two old
postulates that a straight line can be drawn

of the U.S.S. and had continued the
observations of Mars some years
longer so as to make the series extend
over 20 years; and it devolved upon
him to take these measurements
of the Latitude and Longitude of Mars
(remarkably fine observations consi-
dering they were made with the naked
eye) and by means of them to construct
tables by which the Lat. and Long. of ♂
could be calculated for any future
time. Of course it was assumed
that Mars would continue to move
as it had been moving and therefore
one could calculate just what its Lat.
and Long. were at any instant during
those 20 years, except at those times
of each year when it had been too
close to the sun to be observed. I note
that John Stuart Mill in his Logic says
that Keppler only had to make a gene-
ral description of facts known in the
shape of observation. But Mill was a
constant writer of reviews who had at the same time
almost the responsibility of governing India
on his shoulders & it would have been be-
yond human powers for a man every three
months to turn out an article of high literary
excellence in the Westminster Review, and
conduct the business of India House, and add
to that any profound study of Logic. He
had evidently no conception of what Keppler

25 had to do. ~~He~~ He had before him the latitudes and longitudes. But ~~he~~ since Tycho was a single observer at a fixed station he could make no observations of parallax that is of the third coördinate of ♂'s position, its distance from the observer. For observations of its position when on or near the horizon. Any way the smallest angle visible, — the minimum visibile is about one minute of arc and the greatest parallax of ♂ is only about 1/3 of that. Keppler it is true found an ingenious method of measuring the distance of Mars or any planet from the earth and from the sun. But it requires the theory of the motion of the planet to be complete or nearly so, first. In short, Keppler's ~~work~~ reasoning was not, and could not have been, purely mathematical. It was, on the contrary the greatest piece of Inductive reasoning ever yet conducted. Had the parallaxes, or distances of Mars from the earth been known to Keppler with the requisite degree of accuracy, it must have saved Keppler that marvellous piece of reasoning by which he ran down the truth like an indefatigable detective, with hardly a wasted day. It would have been a great loss to students of reasoning. The Conic Sections were not understood in 1610 as they are today, for Pascal was only

26 born in 1623, or thereabout, and his theorem, that if six points are taken on a conic section and straight lines are drawn through the 1st and 2nd, the 2nd and 3rd and so round to a line through the 6th and 1st, then the intersection of the first and fourth of these lines, that of the 2nd and fifth, and that of the 3rd and 6th will all lie on one straight line, no matter how the original six points are chosen (whether they are consecutively passed in going round the curve or not). This proposition, which is the foundation, one may say, of the modern theory of Conics, would have been unknown to him. But Keppler was enough of a mathematician to be himself the first discoverer of the "foci" of such curves, and he would undoubtedly have made great advances in the subject, had he been in search of the laws of the motion of Mars with knowledge which renders it a purely "deductive" problem that is ~~wholly~~ a matter of necessary reasoning. However, I have after all been rather side tracked by choosing this example; for I wished to show that mathematical problems of a new kind are generally first presented in a form which is simply a bewildering mass of facts into which the mathematician has to dive and fetch up first the question to be asked and then with great subtlety pick out the appropriate premisses; then the second difficulty is that mathematical problems are apt to be so fearfully complicated (for example there are over 80 equations in the

subject, — all the usual ones, — to this day. Namely La Place says that if a man on occasions entirely new to him sees a phenomenon equally new on every one of ~~the first~~ those occasions up to N occasions (N being any whole number) then the probability is $\frac{N+1}{N+2}$ that the same phenomenon will occur on the next such occasion. I say this is nonsense. That it is trying to conclude by mathematical reasoning that which requires a radically different kind of reasoning. And what proves that it is nonsense is that if $N=0$ the probability is $\frac{1}{2}$. That is to say that on a wholly new occasion it would be a reasonable thing to make an even bet that an unheard of event would take place. That is the nonsense that results from trying to reason mathematically on matters of fact on the basis of pure ignorance. La Place was renowned for lack of sound good sense, and his doctrine about these inverse probabilities shows it. A probability, if it is correct, is a basis for business. But there can be no such basis except experience & the idea of deducing any matter of fact from anything but knowledge is absurd.

from any point to any other and that a straight line can be "produced" (that is lengthened) at either end, I would supersede by the one postulate that, considering a line as a place, or "locus", as mathematicians have universally considered it since Descartes, "an unlimited straight line is through every pair of points, or places without parts." [Euclid's definition of a straight line is that it is a line that lies "evenly" between its extremities; by which I suppose he means, perhaps a little vaguely, that there are points from which such a line would appear as a point, or from the modern standpoint, it is a line whose shadow, if the source of light were a point on the line, would be a point. It is a question whether this is the better definition (as I decidedly think) or whether we ought to say that a straight line might be the path of a particle not acted on, during its motion, by any force. The definition that a straight line is the shortest distance between two points ought I think to be regarded as an axiom presumably only approximately true.] Now one of the great difficulties of geometry is that no proposition of the kind I should call a "theorem" can be proved without introducing subsidiary lines or surfaces, that are not mentioned either in the proposition to be proved nor in the previously proved propositions. The right to assume these subsidiary loci is derived from the postulates.

I pass over crowds of points deeply interesting to anybody who cares to explore

29 these fields, and come to another division of deductive reasonings, — that into what I call <u>necessary deductions</u> and <u>probable deductions</u>. All deductions are necessary reasonings in the sense that the conclusion must be true so long as the premisses are so. But I use the expression "<u>probable deduction</u>" as a convenient abbreviation of "deduction of a probability." Probable deductions include all the logically sound parts of the doctrine of chances, otherwise called, the calculus of probabilities. This includes so much of that doctrine as could safely be made the basis of the business of insurance. There is a lot more in the books, — particularly in LaPlace's book, which is the base of all ~~modern~~ nineteenth century works on the subject, — LaPlace being the idol of the French mathematicians, — there is I say a lot of it that is utter rot. He says a probability expresses in part knowledge and in part ignorance. This statement is a fair specimen of the loose thought of the book. LaPlace's <u>mathematics</u> is sometimes <u>clumsy</u>, but

30 it is correct as long as his premisses mean anything. But when he attempts to define anything at all difficult he writes utter nonsense. In the sense in which he means it, that which expresses ignorance is utterly worthless & is no part of true science. If two possibilities, he says, are "<u>également possibles</u>" their probabilities are equal, and if two events that are mutually exclusive have equal probabilities, the probability is double that of either, that one or other will occur. "What is the probability that the inhabitants of Saturn have red hair?" asked Mill in the first edition of his logic. That it is <u>red</u>, that it <u>is not</u> red ~~are~~ are "également possibles" since we are absolutely ignorant about it, is true. For <u>possibility</u> that a thing <u>may be</u> admits of no more or less. If it is possible, that is, if we do not know that it is not so, which is certainly the case if we are utterly ignorant, then the two are "equally possible" in the only sense the phrase can have, that we don't know anything against the truth of either. But how would an insurance company fare who should try to do business on such a basis? A basis for business has got to be knowledge and not ignorance. As a specimen of LaPlace's results I will mention something he deduces from his principle of the "également possibles" and which is copied into all the text books of the

35) the remainder after subtracting from the logarithm of the number of occasions on which the reasons apply and the given kind of result occurs, the number of occasions (in the long run, of course) to which the reasons do apply but the result in question will not occur.

I now proceed to define what I mean by the number of occasions in the long run etc. I mean that if the die were thrown over and over again without cessation and if one person A were to keep tally of the throws which brought the ⚅ side of the die uppermost, and a second person, B, were to keep tally of the throws which did not turn up the ⚅ but some other side, and a third person C were after each throw to divide the number of throws A's tally showed by the number of throws B's tally showed while a fourth person D were to divide the number A's tally showed by the sum of the numbers that A's and B's tallies showed, and a fifth person E set down the logarithm of the result of C's division. After the first throw one of the two tallies will show 0 and the other 1; so that C's result will either be 0 or ∞ (infinity), D's either 0 or 1, and E's either

32 Now you will ask me "How do you define probability?" I will define it in a concrete example. Suppose I say "I have a die and owing to its being somewhat ill made, instead of the probability of its turning up six at any one throw being 1/6, or $0.16\frac{2}{3}$, as it should be, the probability of that event is only 0.16" Now you ask what I mean by that. I mean that, (the result of any one throw not having any effect or consequence as to the result of any other throw,) the throws in which six is turned up will be 0.16 of all the throws in the long run." If you ask me what I mean by the "long run," (you will see that in defining what I mean by probability, I must not introduce that same concept,) — I reply that I mean an endless succession of throws in the order in which they are thrown." Thereupon you will

ask what I mean by 0.16 of an infinite number of throws. It is certainly itself infinite and equal to the whole. [For there are certainly as many even numbers as there are of odd and even numbers taken together, since every number has a double, which is the double of no other number

	1	2	3	4	5	6	7	8	9	10	11	12 etc
doubled give	2	4	6	8	10	12	14	16	18	20	22	24 etc

and thus there is a distinct even number for each and every whole number.] That is there is the same multitude of even numbers as there is of odd and even numbers together; and on the same principle 0.16 of an endless series is equal to the whole endless series." This I must admit; and therefore I proceed to define what I mean by 0.16 of an endless series of throws. But first let me observe that beside

"probability" there are other conceptions which express the same facts precisely. It depends upon the kind of relation which the facts one knows bear to the facts one wants to know which of the different conceptions is most convenient in a given case. The probability ~~is the proportion of occasions in which an anted a logically antecedent~~ consequent ~~state~~ that if an antecedent condition is satisfied, a consequent kind of event will take place is the quotient of the number of occasions, "in the long run," in which both the antecedent will be satisfied and the consequent kind of event will take place, divided by the total number of occasions on which the antecedent condition will be satisfied. The odds in favor of a kind of event occurring on any occasion on which an antecedent condition is fulfilled, is the quotient of the number of those occasions on which the condition is fulfilled and the kind of event in ~~other~~ question occurs divided by the number of occasions on which the condition is fulfilled and the kind of event in question does not occur. The weight of reasons for believing that if the condition is fulfilled ~~[illegible]~~ a given kind of result will occur is the three kinds of reasoning I mean are the only kinds of sound reasoning; though I can show reason to think that it ~~may~~ can be proved, and very strong probable reasons for thinking that there is no fourth kind. I call the three, Deduction, Induction, & Retroduction; though the last only is a word invented by me.

A scientific inquiry must usually, if not always, begin with retroduction. An Induction can hardly be sound or at least is to be suspected usually, unless it has been preceded by a Retroductive reasoning to the same general effect. Induction chiefly serves to render more certain ideas that have already been otherwise suggested.

I use "Induction" in a wider sense than usual. This usually regarded as a reasoning by which one passes from asserting something of a number of single things to asserting the same of the whole class to which those single things belong. I give the

+∞ or −∞ (the logarithm of 0 being −∞).

(After the first throw that one of the two tallies that shows zero may continue to do so for an indefinite number of throws. But as soon as both tallies have left the zero point, every throw must change the result of C, that of D, and that of E. For suppose C's had been $\frac{y}{n}$. Then after the next throw it will either be $\frac{y+1}{n}$ or $\frac{y}{n+1}$ and $\frac{y+1}{n} > \frac{y}{n}$ while $\frac{y}{n+1} < \frac{y}{n}$ unless $n=0$. So Ds will change from $\frac{y}{y+n}$ either to $\frac{y+1}{y+n+1}$ or to $\frac{y}{y+n+1}$; that is its reciprocal will change from $1+\frac{n}{y}$ either to $1+\frac{n}{y+1}$ or to $1+\frac{n+1}{y}$. Now as long as neither n nor y is zero $\frac{n}{y}$ can neither be equal to $\frac{n}{y+1}$ nor to $\frac{n+1}{y}$.

Accordingly as soon as the results of both A and B have left the zero, each of the results of C, D, and E must change with every throw. Now when we say that some ~~definite~~ definite fraction will result ~~will~~ will come to pass "in the long run", we mean that it will be so, whether we begin with the first occasion or the millionth or any other. And therefore, since we use this expression in regard to the results equally of C and of D (we may omit those of E since this is merely the logarithm of C,) we mean, ~~that~~ for one thing, that those values, the results of C and of D, oscillate, however irregu-

37 larly, from one side to the other, C, of $\frac{4}{21}$, or ~~0.047619~~ 0.190476, and D, of $\frac{16}{100} = \frac{4}{25}$ [For since $\frac{x}{x+y} = \frac{4}{25}$

$1 + \frac{y}{x} = \frac{25}{4}$ and $\frac{y}{x} = \frac{21}{4}$ and $\frac{x}{y} = \frac{4}{21} = 0.\dot{1}9047\dot{6}$

~~Now~~ $1 = 0.\dot{9}9999\dot{9}$

$\frac{1}{3} = 0.\dot{3}3333\dot{3}$

$\frac{1}{7}\frac{1}{3} = 0.\dot{0}4761\dot{9}$

$4 \times \frac{1}{7} \times \frac{1}{3} = 0.\dot{1}9047\dot{6}$]

One thing we mean then is that D's quotient will never cease to oscillate about the value 0.16 and C's about the value $\frac{4}{21}$. Moreover, we mean ~~that~~ 0.16 is the only value of D's quotient ~~that~~ it will not sooner or later become larger than or smaller than for the last time, although it is impossible to know when that last time will be. This sounds self-contradictory; but it is not so; and when one has attained full command of the concept of endlessly diminishing oscillations and of the endless generally, it ceases even to sound selfcontradictory in the least. This definition of the "long run," (which can of course be made easier to

38 use by being expressed in Existential Graphs,) removes absolutely all the errors and paradoxes of the calculus of probabilities, and leaves it all its utility without its pitfalls. It thus restores all its great utility, so patent in the great business of insurance, and at the same time brings out all the high moral lessons of the uncertainty of anything, or rather the fate of everything that is not anchored to eternal verity and to universality. Every insurance company as well as every other particular human creation is bound to ultimate disaster. But of course in this letter I can only set forth fragments. In my effort to avoid extreme prolixity I see that I have omitted considerations that must not be omitted in the briefest statement. Therefore here please insert the next sheet A.

Deduction, or necessary reasoning, is only one, and certainly not the highest one, of three absolutely disparate ways of reasoning. I believe I was the ~~first~~ to prove this ~~or~~ perhaps the first even to assert it. I am unable yet quite to prove that

A4

other in making up his sample, but
further that he has taken all the pre-
cautions against self-deceit that a
deep sense of his responsibility would
make him regard as a duty.

Of calculations, whether deductive nor
inductive, relating to mathematically expressed
probability, excluding those concerned with
insurance and those relating to games, and ex-
cluding (for another reason) applications of the
method of least squares (which is but fair since
a large minority at least of the men who make
these do not regard the conclusions as meaning
what they say, using such calculations in order
to escape more serious dangers,) — with these ex-
ceptions the proportion out of those one meets
with of such as are seriously fallacious is very
large; and these fallacies are largely, if not mostly
due to unskillful or unfair sampling. They
might be avoided without much additional
labour by making at the same time, control calcu-
lations of probabilities whose true values could
otherwise be ascertained. Such false calcula-
tions deceive those who have not mastered this
branch of logic, and for the most part deceive nobody else. The worst
I have seen of late years relate to "Bacon crypto-
grams in Shakespeare" etc. One deception common
in such pretended science consists (to express its
nature by an example) in making a very complicated
kind of cipher and showing that it gives sense in one
or two cases, and then calculating the probability that
it would do so by chance just as if that rule were the
one that Bacon would necessarily adopt, and as if it
would make sense of any passage in Shakespeare, in-
stead of being a very complicated one among many
tried and rejected and fitting only one or two passages in
Shakespeare's work. This is like calculating the probable
error from an equation involving many unknowns as if there
were but one.

A

More about probability

The illustration of probability
by means of throws of dice conceals
one of the most essential points in
almost all kinds of reasoning about
probability, conceals them, I may go
so far as to say, more effectually
than almost any other sort of il-
lustration does. For instance draw-
ing cards from a pack or a package
from a "grab-bag," though these six
in the same way, do not near so
much so as the dice-throwing.

For example all whist-players
have remarked the frequency of
short suits after a misdeal. The
fact is often doubted because no
reason for it is apparent. Well, I
will not waste time in calculating the
probability of such a thing. But suppose the
first two cards of a suit to be dealt to one person
what is the probability that no one of the
other eleven will be dealt to him? It is

$$\frac{39\times38\times37\times36\times35\times34\times33\times32\times31\times30\times29}{50\times49\times48\times47\times46\times45\times44\times43\times42\times41\times40}$$

which I judge to be about 1/22. Now when
one thinks that there are 77 other ways in which
the same two cards might be dealt to him and 77
other couples of cards in the same suit, one sees that

A2

it must be a good deal more likely than not that some ~~body~~ one of the 4 players will have a short suit, if the cards were thoroughly shuffled. But owing to their being picked up in tricks and very little shuffled, unless there is a misdeal, which shuffles that pack tolerably, the cards seldom get any shuffling to speak of. The calculus of probabilities is a very precious aid to anybody who is trained in using it; but it cannot be made to answer half the questions men try to make it answer, and the amount of fallacious reasoning about probabilities is ten times as great here as it is in any ~~where else~~ other field of discussion.

In order to practice or understand Probable ~~Induction~~ Deduction, it is necessary to study Induction, by which alone the fact that there is about a ~~given~~ stated value to the probability that an object taken, in succession, at random from a given class will have a specified character, ~~can be inferred~~ the first requisite is to ~~know~~ understand just what is meant by taking an object "at random." If instead of a "class," which ~~...~~ consists of would-bes and not of actually existing individuals (I mean, for example, that if we say "any man is mortal" we are not speaking merely of existing men, but of any man there may be) if, I say, we are

A3

speaking of a finite collection, then we may say that an individual instance is taken "at random" if it be ~~...~~ chosen by a method which used successively, would "in the long run" result in any one individual of the collection being taken as often as any other. Mechanical devices such as pottsing coins, throwing dice, spinning teetotems etc. to govern the selection are useful. When we are unable to devise such a method, it is necessary to make a strong effort of will not to be influenced in one's selection by any motive which in the long run would lead to one chosing objects more from one part of the class than from another, unless one has good reason to think that the part more favored is on the whole just like the part less favored in respect to the character to ascertain the probability of which is the purpose of forming the sample. A man should not undertake to do any sampling in order to base any opinion on it until he has undergone such a course of discipline of his will that he can, under a full sense of the responsibility involved in urging any opinion, or in embracing one, be reasonably sure that he not only does not lean one way in the

reached the final conclusion that the latter
was composed of 6/7 ~~of an equivalent~~ by weight of
cinnabar and 1/7 of the red oxide, which
is called "montroydite" by the mineralo-
gists.

The first step toward a comprehension
of Logical Critics must be the recognition of
the gulf which separates Deduction, — exem-
plified on the one hand in the reasoning from the recognition
of a character of a chemical species to the recog-
nition that that character must belong to
any specimen known to belong to that species,
and on the other hand the reasoning from the
fact that the character belongs to the specimen
the conclusion that the same character belongs to the whole species or
~~to~~ either to ~~its belonging~~
to the specimen the conclusion that belonging to the species. The
last reasoning is the kind of reasoning, that
in the fiction we have been imagining we
were represented as performing. The first
of these three kinds of reasoning would be
a deductive or necessary reasoning. If
instead of knowing that the specimen belonged
to the species, we only knew that it had been
taken "at random" from a set of three speci-
mens two of which were known to belong to the
species while the third was known not to have
the character in question, we could conclude
that the probability was 2/3, or the odds 2:1 that
the specimen drawn had the character in
question, or if we knew nothing about the
third specimen, we could conclude that
the probability was at least 2/3 that the
specimen drawn had the character in
question. We should be unable to say, more
exactly, what that probability was. Each
of these three reasonings as to the character
of the specimen would be a "deduction" and
as such a necessary reasoning, although
in the last two cases it would be what I call

definition a somewhat different
turn, at least, and throw much
light upon Induction by defining
it as any reasoning from a sample
to the whole sampled. For example,
if I sample a cargo of coffee, and
find throughout different samples
that about the same proportion of
the coffee-beans are "male" beans,
and thence conclude that the same
is true of the unexamined parts of
the cargo, — while it may be said,
in one sense, that this is a rea-
soning from what is true of parts
to what is true of the whole, — yet
it sheds a flood of light upon
the rationale of the inference to say
that it is an inference not so much
to the whole as to a certain fraction
of the whole. The graph

can equally be read as
"If A is true, C is true," and
"If C is false, A is false."

Fig. 17

In most cases, accordingly, to reason from having found that A is true to the conclusion that C is true is just as sound as, and no sounder than, if one had not found anything except that C is false, and thence reasoned that A is false. In probabilities, however, we are surprised to find that these two inferences may be of a radically different kind. Suppose, for instance (if so wild a fancy may be permitted) that in digging a well (that in fact I mean to dig when I put up some cottages,) I should come across a ~~vein~~ deposit of a reddish brown color, opaque, not to be scratched with my nail but with not much difficulty with a knife, almost as heavy as so much copper, almost dull looking but almost metallic, capable of being cut without breaking every time yet apt to do so; with some tendency to uneven cleavage, and finally when scratched showing a bright red streak, I should say, "This is pure cinnabar ~~and if I weigh out~~ more than 6/7 of it is Mercury. If I were to weigh out 7 ounces of it and heat it in a retort with the neck under water I should ~~get 6 ounces~~ of distill over $6\frac{1}{29}$ ounces of Mercury." I take the liberty of imagining you to say thereupon, "I wish you would try the experiment," whereupon I should reply, "I have neither the retort nor the heater nor the balance handy at the. Besides, what the use? You see, here is a list of atomic weights

Mercury is set down as 200.0 and sulphur as 32.06, and this is therefore $\frac{200.0}{232.06}$ mercury or $\frac{1}{1.1603}$, which is 86.1846 % and $\frac{6}{7}$ is 85.714

This is cinnabar, you must admit, and the atomic weights are ~~the most~~ as accurate as the figures are given." Thereupon you say, "Yes, I admit all that." I say "Well the conclusion is a necessary one, is it not?" "Yes, you say; but still I should like better a direct experiment." (I am attributing to myself faults quite contrary to my real tendencies) The direct experiment was tried and gave a little more mercury than was expected. However, metallic mercury is frequently found in minute quantities in deposits of cinnabar, and we decided to make a careful determination of the percentage of mercury in the mineral in the wet way. Three such were made [of course, it is all pure fiction designed to show the kind of reasoning under typical circumstances.] They were in excellent agreement and gave ~~for the percentage~~ in the mean, for the percentage of mercury in the mineral 86.90. In order to ascertain whether the excess of this result over the theoretical 86.185 was due to the presence of metallic mercury (as I felt almost sure was the case) or to ~~mercurous~~ mercurous sulphide, or to mercuric or mercurous fluoride or oxide (the red oxide) we took four specimens of vermillion, one Chinese, one prepared in the dry way, and two prepared in different wet ways, and ~~have~~ having brought the content of mercury in separate portions of each up to that of our mineral in the six ways mentioned, we compared the effect upon each of various solvents with the corresponding effect upon our mineral, and so

[47] logician, yet he did see that Laplace's reasoning on parts of the doctrine of chances was all wrong, and by a stroke of genius was able to put his objections to Laplace's procedure into such a form that the overwhelming truth of them must appear plain to everybody who followed the somewhat difficult, but perfectly evident, footsteps of his thought. This was the more admirable in that Boole did not himself reach a perfectly clear notion of probability, being somewhat in the Laplacian fogs. But without the suggestions which I derived from his reasonings I doubt whether I ever should have been able to attain the crystal clearness of conception in regard to probabilities that I have attained.

Mathematicians may and do employ all kinds of reasonings in getting their *suggestions*; but "mathematical reasoning," that is to say, all the reasoning with which mathematicians ~~they~~ appeal to their readers for their assent, is *deductive*.

Induction is that kind of reasoning which from what is true of a part, concludes what is true of the whole. It is evident, therefore, that it is not deductive; and as soon as it is shown that a supposedly ~~deductive~~ inductive conclusion can be proved, either certainly or probably, according to any definition of probability, from its copulative premiss, so soon is

[44] a "probable deduction," that is, a necessary reasoning about a probability. If you will pardon the repetition, by a "necessary reasoning" ~~I~~ is meant (by all ~~good~~ correct writers) one whose conclusion everybody is *forced* to admit (supposing him candid,) if he fully admits the truth of the "copulate premiss," (i.e. the premisses,) and fully comprehends the reasoning. But he would not be absolutely *forced* to admit the conclusion, if he could deny it without falling into a contradiction "*in adjecto*," that is, in denying it, though he might not *directly* deny anything stated *explicitly* in the copulate premiss (to do which would be a "contradiction *in terms*,") yet he would deny the reality of a state of things the reality of which had been affirmed, though perhaps not in one sentence, yet in the copulate premiss taken in its entirety. If you still ask for a further explanation of what I mean by saying that the reality of a "given state of things has been ~~asser~~ affirmed in a given copulate premiss, I do not ~~at present~~ just now see any way to an exacter ~~way~~ definition of my meaning than by saying that if that premiss be expressed in the syntax of existential graphs then the

4. three "permissions" attached to my account of that syntax will enable one to modify the ~~expression~~ total graph that has been scribed so that it shall assert any fact, or state of things whose reality was in any way asserted in the total graph before ~~those~~ modifications. The first permission amounts to saying that if one has ~~stated~~ asserted that some~~thing~~ fact A and some fact B are true, one may modify that ~~statement~~ assertion by saying that A is true without saying any thing about B. Thus if one has ~~it~~ asserted that James loves Eliza, one may ~~modify the~~ assert instead by saying that James loves something ~~and that~~ ~~somebody is Eliza~~, is Eliza or whom James loves without saying who. On the other hand, if one has denied that ~~A alone~~ any assertion A is true, that is, has denied that it implies no thing false, one may deny that A and B are both true. And so if one has asserted that if A is true then B is true and C is not so, one may modify that so as to say that if A and Z are both true then C and Y are not both true. ~~The second~~ ~~permission~~ The second permission amounts ~~to saying~~ that if one has asserted that A is true and that further if A and B are both true, C is true, then one may modify this so as to assert that if A is true and that if B is true C is so, or so as to assert that if B is true C and A are both so. And if one has asserted that X is true and that if Y is true then so is Z, one may modify it so as to say that if Y is true X and Z are both true etc. The third permission amounts to declaring that to say that if anything whatever is true, P is true amounts to precisely as much as to say that P is true, and vice versa.

Probable reasoning involves numbers, and I have not stated how I would deal with the logic of numbers, for the reason that that field of thought is so distasteful to many persons, and for the further reason that during the time that I have been studying logic, mathematicians have corrected most of their fallacious reasonings except in certain special departments, such as in reasoning about continuity and sometimes about infinite multitudes and infinite ordinal numbers, upon which special fields I have demonstrated some current errors. But although I have passed over the logic of number for fear it might be too distasteful to you, yet I will say that my way of dealing with it compares favorably with any part of my system that I have described whether in the rigour of its proofs, its elegance, or its surprising simplicity; and I have the pleasure of confidently counting upon what I have contributed to mathematical journals helping in the near future to enable mathematicians to correct the more fundamental of their still remaining causes of error. If you have any curiosity to know about this part of my work I should take pleasure in gratifying it. I ought to acknowledge that some fundamental points in the logic of probabilities might never have been so completely cleared up in my mind, if I had not had the advantage of standing on shoulders of George Boole. For although Boole was not himself a very strict

[51] This doctrine, as far as it goes, is quite correct. But to it Aristotle adds two assertions that are mistaken and which I will state presently, as soon as I have made some improvements upon this correct but not sufficiently definite statement. Namely, it is not from a simple syllogism such as Aristotle instances that induction results by turning its conclusion into a premiss and its major premiss into a conclusion. But the syllogism from which induction does result from such transpositions of its premisses is what I call a "Statistical Syllogism". Even so, it is not every kind of induction which so results, but only one of three classes of inductions; — the strongest of the three, however. I call it "Quantitative Induction." There is a second kind of induction, which results from in-terchanging the conclusion of a syllogism (a peculiar kind of syllogism different from that from which induction results) with the minor, instead of the major, premiss; and this I now call Qualitative Induction. Finally, we are sometimes forced to resort to a third, the weakest of all kinds of Inductions, which I call "Crude Induction" which seems to me to have no relation to a syllogism. The syllogism from which Quantitative Induction results is like this in most cases:

Premiss: Among all individual objects of a given class (~~say~~ call it the M's) that are likely to have any interest for me, a percentage, say p% will have a certain special interest, consisting, we will say in each of them having the character μ, and these will be distributed among all occurrences of M's in an entirely irregular way, so that whether or not those of the character μ have been occurring at any time more frequently or less frequently than usual is no reason for thinking that the next ones to occur ~~will~~ will be more likely to have the character μ, or not to have it, than if recent occurrences had been of any other character.

47 it proved that the reasoning was not (or need not have been) Inductive.

Yet this is a position in which I almost in a minority of one, though I am perfectly confident that the world must and will come round to it.

As far as necessary deduction goes, I think most people would assent to what I say, as long as the question was put baldly, in spite of its really conflicting, though they don't see that it does, with other opinions of theirs that they will continue to hold for a good while yet.

But when it comes to probable deductions, though these are really as completely necessary reasonings as are "necessary deductions," all the treatises on the doctrine of chances that I have ever come across are flatly against me, as well as are the great majority of those who have undertaken to explain why an Induction should be as good a reasoning as it has often seemed to be, and as they think (and I with them) that it often really is. For most of the attempts to explain Induction really are (though often not perceived by their sectators to be) attempts to reduce it to probable induction; and the astonishing feebleness of these attempts, considering the general intelligence of their authors, renders the fact all the more significant.

I will pass in as rapid a review as I possibly can all those attempts to explain the validity of Induction that are really worth examination. Of course they are not numerous. It has only been at exceptional moments that it has been possible for a man here and there, say one in a lac (100 000) or so, to have his attention at all focussed on Logical Critics. It has been a sufficiently rare historical node that permitted a proportion of grown men to engage, with any earnestness of sincerity, in any inquiry whatever; (I admit that children have a much greater spirit of science; and here and there a child in growing into manhood would preserve his interest in some particular line of thought. I am rather inclined to surmise that this accounts perhaps for the majority of cases of scientific genius.) In the immense majority of individuals, puberty occasions a supervention of stupidity, along with its other effects, good and bad.) Of the

few who do become inquirers, what a minute proportion are led to interest themselves in the question of precisely what it is, in general, that justifies an inference! And I can testify from perhaps the widest reading of anybody of my time on this subject, that the literature is scant, and very little of it goes to the point.

The Greek word for induction, of which our word is simply the latinization, — ἐπαγωγή, — was apparently introduced by Socrates. It expresses the reasoning by the metaphor of a body of soldiers led up to make an attack on a position. Socrates was the first to employ induction systematically. He offered no theory on the subject, but simply relied on common sense.

Aristotle shows that ~~an inductive~~ an induction is simply a syllogism; only that which is the conclusion of the syllogism becomes in induction one of the premisses, while the major premiss of the syllogism becomes the conclusion of the induction. Further, this syllogism of which induction is a transposition has for its minor term, i.e. the subject of the syllogistic conclusion an enumeration of instances.

Syllogism	Induction
Nothing long lived has bile.	Man, the horse, the mule are long lived
Man, the horse, the mule are long lived	Man, the horse, the mule have no bile
Ergo, Man, the horse, the mule have no bile	Ergo Nothing long lived has bile.

[55] from the ratios of the whole class is for large numbers ~~inversely~~ inversely proportional to the square root of the size of the sample so that the probable departure in the actual number of occurrences between theory and fact is ~~directly~~ directly proportional to the ~~number~~ square root of the size of the sample. [This day is the hottest I have experienced since I came to live here nearly thirty years ago; and I hardly know what I am writing.]

There are writers of such inaptitude for the the subjects on which they write, that they think that because it is probable that a large sample will be distributed into varieties nearly proportional to those of the correspond-ing varieties of the class of things sampled, ~~the~~ therefore the finding that the sample is divided into varieties in certain proportions renders it probable that the class sampled has such varieties ~~pro~~ in approximately the same proportions.

Conclusion: It follows that the next occurrences of M's up to any number specified in advance (so that we cannot by examining them as they occur and allowing the occurrences to run on until they take a particular average character and then stopping the experiment, exercise any measure of control over this average character,) will probably and approximately (i.e. with more probability the less close the specified approximation, according to a certain law made known in the doctrine of chances) resemble the whole class in ~~containing~~ having P% of its Ms of the character μ.

In this statement, owing to my habit of thinking in the syntax of existential graphs, I have lumped the major and minor premisses into one copulate premiss. In order to study Aristotle's account of induction (while taking induction in the general sense of ~~reasoning~~ argument inferring the class from the character of a sample to that of the whole class sampled) I must distinguish the major and minor premisses. The major premiss is so much of the copulate premiss as affirms or denies the character of ~~that~~ class concerning a part of which the conclusion partly affirms and partly denies the same character. The minor premiss is that which ~~states~~ affirms the character that defines the general class (i.e. the character of consisting of Ms) of that part of this class to which the conclusion asserts something (namely that probably and approximately it will agree with the whole class in that respect which the major premiss partly affirms ~~or~~ and partly denies of the class.

Now ~~in~~ any of Aristotle's syllogisms remains a necessary reasoning when the conclusion and ~~major~~ either premiss are interchanged, provided that at the same the inter-

[53] changed propositions be transmuted into their precise denials. This is of course true since the very same graph of Fig. 26 that expresses that if both premisses be true the conclusion is true, equally expresses that if one premiss be true while the conclusion is false, then the other premiss is false. Only it might be rather more natural to scribe this consequence [not consequentem, but consequentiam] in the shape of Fig. 27, though there is really no difference of interpretation between the two. (P and p are the premisses, C the conclusion)

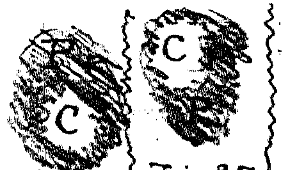


Fig 26 Fig 27

Instead of making such an indeterminate and algebraic supposition, I shall make myself more readily understood if I make the following supposition. Let there be an urn with three balls in it, one Red, one Green, and one Violet. Let a person put his hand in the urn and draw out a ball, and whatever its color may be let it be immediately be replaced by another ball of the same color, so that the urn shall still contain 3 balls, one Red, one Green and one Violet. Let the same person, who drew the first ball now draw another and let a new one of the

[54] same color be put in the urn to replace it so that once more there shall be one ball in the urn of each of the three colors. Let him go on in this way until he has drawn 6 balls. Then there are $3^6 = 729$ possibilities, and I will give at once the values of various probabilities, each multiplied by 729.

That all the balls drawn will be of 1 color . . . 3
" " " " " " " 2 colours 186
" " " " " " " 3 colours 540
" " " " " " " one or other or both of 2 given colors 64
" 5 will be ~~of the same co~~ alike in color and the 6th different — 36
" 4 will be alike and the other two alike — 90
" " " " " " " unlike — 90
" 3 " " " " " 3 alike — 60
" " " " " " " not — 60
" there will be 2 of each color — 90
" 6 will be of a given color — 1
" 5 " " " " " 12
" 4 " " " " " 60
" 3 " " " " " 160
" 2 " " " " " 240 = $\frac{8 \cdot 9}{27}$
" 1 " " " " " 192
" 0 " " " " " 64

If he had only drawn 3 balls the probability that just 1 would be of a color named in advance would have been $\frac{12}{27}$.
This illustrates the fact that the departure of a sample from the ~~ratios of the whole class is proportional~~ ~~for large numbers to the square root of the size of the sample.~~ number that is in the same

matically from the premiss. I say it obviously
does noth
Chapitre VI of his Livre Deux is
entitled; "De la probabilité des causes
et des événements futurs, tirée des
événemens observées." Now to begin
with, I object that the expression
"la probabilité d'un événement" is
utterly meaningless. Probability is
the ratio of frequency of a specific kind
of event to that of a generic kind of
event to which the specific kind be-
longs). Being of the nature of a ratio, it
is necessary to specify the two terms
between which the ratio of frequency is
desired. Furthermore, Laplace is no
authority except in mathematics; and
mathematics is the science by which one
deduces necessary consequences from
assumed premisses, generally of a
quantitative nature [Not always, since in
projective geometry there is little question
of numbers and none at all of measures.]
Consequently it is impossible to deduce
the probability of a cause from any observed
event. That requires induction, a kind
of reasoning radically unlike mathema-
tical reasoning or any other mode of de-
duction.
I have mentioned I believe Laplace's absurd
result given in all the text books on Probabi-
lities that if any event has been observed
on N successive occasions the probability
will be (N+1)/(N+2) that it will occur on the
next occasion.
One of his problems professes to calculate
from the fact that all the balls in an urn are
numbered 1, 2, 3 etc and the fact that a ball has
been drawn and found to bear a number N, what the

But, of course, it is only in fictitious
cases that we can ever know the exact
value of a probability, and we rarely know
one nearer than to the nearest per cent.
One of my colleagues of the U.S. National
Academy of Sciences says that by throw-
ing a stick on the floor and keeping account
of the number of cases in which it fell
across the line between two floor boards,
he has calculated the value of π ac-
curately to four figures! When we
consider how many times it would
so fall that one might count the
case as one of the sticks falling across a crack
or otherwise according as one was
inclined in ones heart, I think it may
be questionable whether what he has
really added to our knowledge be not
something about his scientific probity.
For to calculate a real probability to one ten-
thousandth part of itself would be a
matter of such difficulty that I do not
think a serious man would under-
take without some serious purpose,
such as is entirely wanting for that
way of calculating the value of π. The

[5]

theory of the method is all right, but the whole difficulty is that without a high power microscope there would be too many cases of doubt; and it might be a long job to resolve some of them.

I have a great mind, before leaving the subject of probable induction to run over Laplace's book & pick out the cases in which he pretends to have solved problems by mathematical reasoning when it is plain that the state of things concluded is in no form asserted in the copulate premiss, so that it cannot be reached by such reasoning. It would be too much of a job for me as well as an intolerable bore for you if I were to go through the book & extract all such cases. But I will take a few. [All the old editions of the book are made up from the sheets printed for the 1st edition (with some additions) so that if I give the page, the problem referred to can always be found.]

The first I notice is on p. 201. "Une urne étant supposée renfermer le nombre x de boules, on en tire une partie ou la totalité, et l'on demande la probabilité que le nombre des boules extraites sera pair." I should not venture to quote that absurd question without giving the page number so that you can verify it, it is so contrary to common sense. He says that the probability that the number of balls taken out will be even is

$$\frac{2^{x-1}-1}{2^x-1}$$

and this conclusion he obtains by supposing that each possible constitution of a draw is equal to every other. For instance, if there were 5 balls, ~~he says~~ A B C D E, there are five equally likely ways of taking out 1, which I admit; and that there are ten equally likely ways of taking out 2, namely,

AB AC AD AE
BC BD BE
CD CE
DE

which I equally admit. But he further assumes that each of these 10 ways of taking out 2 is equally probable with each of the 5 ways of taking out one, which I deny to be implied in any way in the supposition. He concludes that if anybody makes it a rule to bet that an odd number of balls have been taken he will be morally certain to win in the long run! I don't believe a word of it, as a matter of fact. It is not, however, a question of fact but the question is whether the conclusion follows mathe-

a reasoning by which one was led
from the knowledge of instances of
a class ~~of all~~ and from finding that
all known instances (or all those
examined) had a certain character
to conclude that this character would
be found to belong ~~apply~~ to every member
of the class. But I understand by
"induction" something so different
from this in several particulars that
I now begin to think that I had better
adopt ~~have~~ a different word to express my
conception.

The word "induction" is, of course,
taken from the Latin inductio, and
this first occurs in its logical sense, in
two passages of Cicero (unless I have
forgotten. I will consult the Lexicon. — I
have done so; and while I am confirmed
as to the two passages of Cicero, I have learn-
ed something that quite surprises me; and
from it you will see how imperfect is my
mastery of Latin.) In both those passages,
which I remembered very well, Cicero
speaks as if the Latin word were formed
in imitation of the Greek ἐπαγωγή, and
one naturally gathers that he himself
had first applied the Latin word (or
perhaps invented it) because it is formed
from in and ~~duc~~ duco "I lead", just as the
Greek from ἐπί "up against" and ἄγω "I lead".
But I had always been prevented from
believing that it was Cicero who had
invented the logical term because I
said to myself "Cicero, with his fine sense

the probable number of balls in the urn
&c. But no deductive conclusion on the
subject can be drawn from these pre-
misses correctly.

Since you want to know what I have
done, I ought to mention that my studies
of deduction have led me to various
mathematical truths that are of more
or less interest, and some of them not
without importance. I will only briefly
mention one or two.

One is that no infinite multitude is
the largest possible (as had been assumed
by several mathematicians.) They form
a series analogous to that of the whole
numbers; and the theorematic proof of
this is interesting, too.

Quite an important proposition of
which I gave a very simple proof was that
every multiple associative algebra can
be represented by a square matrix.

I also proved that there are but three
possible ~~[illegible]~~ associative algebras in
which the quotient of one quantity divided
by another that is not zero has ~~is~~ a definite
single ~~&~~ value. These three are ordinary
algebra of real quantities, ordinary alge-
bra of imaginaries, and real quaternions.

I also gave a rule for the numerical
solution of any algebraic equation, that
is the most expeditious possible; and it
has the curious property that if ~~one~~ a
numerical error in the calculation is made
one ~~[illegible]~~ only keep right on according to
the rule and one will come out all right.

I also showed that fractions can preserve all their properties and yet have only an <u>ordinal</u> signification; and I gave two simple rules one by which all ~~fra~~ numerical fractions ~~I can~~ values in their lowest terms be set down in the order of their values without reducing them to a common denominator; and the other a rule by which without any arithmetical operation whatever except counting all fractional expressions in numbers can be set down with their relations of being equal or the one larger than the other. This is of interest as showing that measurement and the equality of parts is not necessary for the employment of fractions.

There are a <u>lot more</u> of such things.

I will now proceed to <u>Induction</u>. My doctrine here is the only correct one, and will have to be received. By induction I mean any reasoning from a part to a whole. Of course, it cannot be a necessary reasoning. Nor is it a probable deduction. Though its conclusion may be more or less false, yet it is justified by the circumstance that though the conclusion may be more or less erroneous, yet one has only to persist in following the same method, and the conclusion will get corrected. It is the only way there is of assuring oneself of <u>real</u> truth beyond what direct perception ~~furnishes~~ furnishes.

I recognize <u>three</u> important Classes of Inductions, or perhaps they should rather be called Orders, since they differ chiefly in the complexity of their precautions against error. I know no way of measuring the probative force of Inductions. If there be any such way I suppose I shall have to leave the discovery of it for my successors. But the three Orders of Induction I speak of I call respectively Quantitative, Qualitative, and Crude Induction.

I cannot find, though my reading in logic has been pretty near exhaustive, I imagine, (miserable trash it is too, on the average, though here & there a Leibniz or a Boole rears his head & though very liable to error does advance the science,) that anybody before me had any <u>distinct</u> conception of the nature of Induction & I will now run through history and make remarks on the different theories of this kind of reasoning, — <u>all</u> of which, except Gratry's (one of the least credible) I can absolutely disprove.

~~Before~~ Before doing that I will remark upon the name "induction". Before me, this ~~term~~ was always taken by logicians to mean

[6] that under certain circumstances
one would act in a definite way, and
would be content to do so. And so
real a thing is imagination that not
only will a habit be created by
acting over and over again in a
particular way, but it is almost
as effectually created by repeatedly imagining
the same performance in an intense
way, this intensity consisting in a
certain stress that one puts upon a
mental self-command or self-im-
perative at the same time calling
up all the essential, and all the characteristic circumstances
that belong to the kind of perform-
ance that one wishes to inculcate
in oneself. I don't mean matters of
morals, because in *that* field every-
body knows the truth of what I am
saying, but I have rather in mind
things that hard to do. For instance,
if you cannot with one foot describe
horizontal circles in one direction
while simultaneously describing
with the fist of the same side (right
or left) circles in the opposite direc-
tion and then at a word of command
given by another person, reverse
the direction of motion of hand or
foot (whichever be commanded) or both

for the meanings of words would
never have turned ἀπαγωγή,
which means, outside of logic, the
leading of a body of individuals
up against a military position
especially a fortification, "Cicero,"
I said to myself, "would have seen
that, not inducere, but adducere
was the appropriate equivalent of
ἀπάγειν, and would have adopted
for the logical word, not inductio,
but adductio." But now in reward
for my energy in jumping up and
fishing out the Lexicon, when there
seemed no great need of it, I learn
that adductio is not in the Lexicon
at all, so that it cannot have occur-
red in ~~the~~ ancient Latin. It may
have been avoided because of its
suggesting the "procuring" of a woman.
In Medieval Latin, in which I am more
at home, it is common enough; and in
English, since logical remarks have
been common, we have spoken of the
"adduction" of passages or examples
in support of any statement exactly

as the word ἐπαγωγή is used in Greek. If one spoke of the "induction" of such passages, one would hardly be understood. Yet the bringing forward of instances is just the characteristic of the kind of reasoning or argumentation called "induction". In view of this, I intend to take seriously into consideration, in view of my conception of the essential nature of such reasoning being as different from that of all who preceded me as it is, whether I ought not to have a different word for what I mean and call it <u>adduction</u>. If it were a mere scientific term, I should not hesitate. In science the only proper way is to invent new terms for new significations. But the wider logical words are or ought to be like the ~~great~~ names of other great classes, in being fit for the use of the generality of those who read and write; and what would do well enough for mere terms of science may not suit the taste of those who know much about matters

of taste but little about matters of science. It was about 1870 — I don't think it could have been as late as 1872 — that I invented the word "pragmatism" to mean that way of thinking that regards ~~conceptions as~~ thinking as consisting not necessarily in talking to oneself because an algebraist, like Boole, plainly thought in algebraic symbols; and so did I, until, at great pains, I learned to think in diagrams, which is a much superior method. I am convinced there is a far better one, capable of wonders; but the great cost of the apparatus forbids my learning it. It consists in thinking in stereoscopic moving pictures. Of course one might substitute the real objects moving in solid space; and that might not be so very unreasonably costly. — Well I was saying that by pragmatism I meant a philosophy which should regard ~~concepts~~ thinking as manipulating signs so as to consider questions. But attention, whether voluntary or not, is always an <u>act</u>; and a general conception is a <u>habit</u>, and <u>believing</u>, <u>real</u> genuine belief, consists in a habit ~~of~~ with which one is contented and which one usually recognizes (though not always) this habit consisting in the general fact

71 the least indication that he called the method ἀπαγωγή, though we know he was a good soldier. Though I haven't Ast's Lexicon and cannot be absolutely sure, yet if that word had occurred in any of the writings attributed to Plato it could not possibly, I think, have failed to have attracted my attention. Xenophon does use the word, but not in the *Memorabilia*, I feel pretty sure [I am sorry to say I haven't a copy of that book as I ought to have. But there is nothing for me to do but regret it.]

The Adductions of Socrates are all of the "crude" order, and one may say the crudest of the crude; but all classical Greek is entirely anterior to all science, naturally. When men get so advanced as seriously to have reached the scientific way of looking at matters, that delicious naïveté is necessarily gone. Here is one of Socrates's genuine Adductions given by

[68] if commanded, and that in a perfectly smooth and facile manner; I say if you cannot do this, then you can acquire the habit of doing it, meaning that you can acquire the power of doing it easily at will, without ~~[illegible]~~ any actual practice at all; although you may need to put hand or foot or both into ~~a~~ certain positions and note how it feels to move them one way or the other, just enough to acquaint yourself with the kinds of efforts you have to learn to make, but not practising the motions at all. I will venture to guess that you will be surprised to learn by such an experience how true it is that a habit can be acquired by imaginary practice. Out of such considerations which turn, as if upon a pivot, about the idea that a thought is nothing but a habit connected with a sign, one

-can build up quite a little philosophy which is what I meant by "pragmatism". I think the idea was suggested to me by Berkeley's two little books about vision; and while the idea was a fresh one for me in the early seventies, I used to talk my friends to death about "pragmatism." I never, however, saw fit to use the word in print, and even in 1889, when I had entire charge of the philosophical, logical, metrological, and various other departments of the Century Dictionary, I decided against inserting the word pragmatism.

William James who had heard me talk about it so much, but who had no head for logic at all took it up and made the man in the street get some notions of what pragmatism was. I still think I was right in not inserting it in the Century Dictionary. It is a grave responsibility to introduce a new word even if one has the power to do so. Therefore I will not rashly decide to call what in my logic corresponds to induction by the name Adduction. But in order to try it on, as it were, I will so-call it in this letter.

The first man ~~who made~~ to make a systematic use of this great method of reasoning, Adduction, the only way in which can get anything like certainty beyond what the senses afford us of real things, — since Deduction only traces the consequences of premisses that ~~are~~ may be, for aught Deduction can teach us, mere arbitrary suppositions, — was as living a man as ever did live. He naturally would be so. It was Socrates. It is curious that there is not

[71] 39 to 44 inclusive will be 0.4025
that it will be from 38 to 45 inclusive will be 0.5521
" " " " " 32 to 51 " " " 0.9422

There are two conditions of this reasoning
being legitimate that are apt to be over-
looked; and they need mention here
because ~~they are~~ corresponding condi-
tions apply to Quantitative Adduction.

The first is that the fact that it was the
probability of the ~~the~~ balls carrying a
letter E and not any other distinguishing
mark ought to be decided upon before the
drawings. ~~The~~ This is the reason it was men-
tioned in the premisses that there were many
other letters on balls. ~~For~~ Among so many,
it is very likely that some will occur oft-
er on the 125 drawn than the rules of pro-
bability indicate and others less often. If then
it were not decided what the conclusion
was to relate to, one might be tempted either
to use it for some character which had been
remarked to be frequent on the balls drawn or
the reverse, according as one either trusted
or distrusted calculations of probability
more than one ought to do.

The other condition is that in drawing
the balls they shall really be drawn at random
That is why I supposed that special machinery
should have been acting incessantly for a year
in mixing up the balls; and instead of this being an
exaggerated precaution, it would on the contrary,
be altogether inadequate if one proposed to state
the probabilities, as I have done, to 4 figures of decimals.

[72] Xenophon himself [for though I have
not the book I pick up scraps of it here
& there in footnotes to German books that
I have etc. They only serve to make me feel
that I ought not to publish anything until I
can get books and refresh my memory
about the books I read in my early years.]

— Well I am unable to find the passage
I wanted. But examples of Socrates's
way of reasoning are given in Plato's earliest
dialogues, Euthyphron (written, say, 400 B.C.)
Crito (398 B.C.) Charmides (398 B.C.) Hippias Minor (397 B.C.)
Lysis (395 BC) Laches (395 BC) Euthydemus (394 B.C.)
[The dates are the results of an investigation of my own
and cannot be far wrong.]

It has suddenly occurred to me where the little
example of Socrates's style of Adduction was. It
is in Cicero's De Inventione Rhetorica, Book I cap 31.
But as long as it is not from Xenophon himself,
though Xenophon and his wife are in it, it loses its
interest.

Aristotle was the first, so far as I know,
to use the Greek word ἐπαγωγή in its
logical sense, and there is a passage in
the last chapter of the first book of his Topics
which seems to me convincing proof that the
proper English translation of the noun (sup-
posing our dear Lady Welby's principle, that
what was originally unwisely done ought to be
corrected now at once no matter whether a few
millennia of usage may seem to some to have
consecrated it) ought to be "adduction" [My
diagrammatic syntax has utterly spoiled my
English.] However I shall call it Induction

as long as it is not just what I call Adduction. Aristotle's theory of Induction is that it is the inference of the major premiss of an ordinary syllogism (he does not say this must be in the first figure, but it would be odd if it were in the third) from the minor premiss and conclusion. He gives this example

The Syllogism

No long lived things have gall
Man, the horse, the mule are long lived
∴ Man, the horse, the mule must be long lived

The Induction

Man, the horse, the mule are [a sample of] things long lived

Man, the horse, the mule have no bile
So Whatever is longlived is without bile

This is Adduction, though only Crude Adduction. There is also the germ of a penetrating truth in the relation which Aristotle finds between the Syllogism and the Induction, although in the bald way in which he states it, there seems to be little or no reason for asserting any particular relation between the two. If however instead of its being an ordinary Deductive Syllogism with which he starts, it had been a Probable Deductive Syllogism, it would be seen that there really was just that relation which he asserts between the Syllogism and the Adduction which would result, He follows an illustrative example of this

Probable Deduction

~~Major Premiss~~ Here is a large chest full of little balls the size of pease. Just one third of them are marked each with the letter E; and for a year, well considered machinery, moving with great rapidity day and night has been mixing the balls up. It will be seen that most of the balls carry over twenty characters, letters of the alphabets, Roman, Greek, AngloSaxon, English, Russian, numeral characters, astronomical signs, etc. in great variety. It is, however, only the letter E that is to form the subject of this experiment.

Now I propose to draw out 125 balls at random. That is, the chest being nearly cubical, I shall endeavour, as nearly as I conveniently can, to imagine each dimension of the chest to be equally divided into five parts and I shall endeavour to draw one pea from each of the 125 cubes (or rectangular parallelopipeda) so imagined. And the probability will be (calculating not according to the exact rule, which would be ridiculous, since almost all probabilities are somewhat doubtful, but as is usual, by means of the Θ function of probabilities, which must not be confounded with those of the same name in elliptic functions. Tables of Θ are given in all works on probability) 0.1505 that the number of balls marked E among the 125 drawn will be either 41 or ~~be between be exactly~~ 42; ~~that these~~ That the number will be ~~44~~, 43, 42, 41, or 40 is 0.2957. That it will be from

Nights' Entertainments is not more purely fictional than mathematics in its exactitude. If anything is stated to be exactly so and so, and it is veraciously so stated, then what one is talking about is pure, — the most exquisitely pure of fictions. One and one make EXACTLY two. That is if one ever _should_ put down just one thing & nothing else & then another and nothing more one _would_ have put down just two. But then one never _did_ do that. And even that is probably not _exact_ For one may once or twice have done so. But when one speaks of doing something twice there is no exact idea there. I once carried a platinum Kilo, in a

These reasonings about the balls that have so far been considered have all been deductive. That is to say, nothing has been asserted in the conclusions that had not been asserted in the premisses; — at least, such was my aim. I don't know as I quite succeeded. In saying that nothing is asserted in the conclusions that had not been asserted in the premisses, I mean that when I conclude that a _probability has a certain value_, I mean that in the long run the event would happen so often. But when I say that the drawings are made at random (and independently of each other) from a collection of balls as described, I _mean precisely_ that they are so made that in the long run they will turn out in the same way. I see that in one respect my state-

[76] could not have confirmed them.

But what I mean is that inferences as to mathematical probabilities, if they are correctly drawn, — for which purpose it must be known in advance that events will occur just so often in the long run, from which supposing further that the different events are absolutely independent of one another, so that one's turning out one way has not the slightest influence on the frequency of the other, then the reasoning being confined strictly to concluding the probabilities of combinations of those events is strictly deductive.

Of course, it is purely mathematical; and the Arabian

[77] ment was faulty. For the method of drawing out the balls amounts nearly to drawing them from 125 distinct parts of the chest instead of drawing each one at random from the whole chest. What ought to have been done was to have subjected the whole lot to a new mixing operation, & didn't because I thought you might get tired waiting 125 years for the experiment to be concluded. But I might have had 125 chests prepared beforehand. My method was too favorable to the doctrine of chances. It made the drawings conform to theory more closely than they were entitled to do. If I had calculated the probabilities of my probabilities, experiment

and utterly different from a deduc-
tion. Here is the

Quantitative Adduction.

These 125 balls have been carefully drawn strictly at random from this chest of balls, for the purpose stated in advance of trying how many of them are marked with an E).

On examination, it is found that that 42 of them are so marked.

Hence ~~the~~ presumably and provisionally until further evidence is obtained, we ought to hold that ~~about~~ $\frac{42}{125}$, or ~~very~~ nearly $\frac{1}{3}$, of all the balls in the chest are so marked.

All scientific reasoning, outside of mathematics and the ~~Arabian~~ Nights
is provisional. Every scientific man knows
it. It was only the other day that the second
law of motion was exploded. The same force
that would accelerate a slowly moving body
very much, will have hardly any effect if
the body affected is moving nearly as fast
as light.
But many reflexions force themselves upon

velvet-lined étui into which it fitted as accurately as anything can fit into velvet, & in my hand-bag, from the Office of Weights & Measures in Washington to Dr. B (his name escapes me,) at the International Office of W. & M. in the Pavillon de Breteuil in the Park of St Cloud. He remarked that the weight would probably have suffered loss from rubbing in the transport. "True," I said, "but so it was sent to us; and at the time it was made they did not know how to cast platinum, nor did they know that platinum occludes hydrogen in great quantities."

But that I am forbid
To speak the secrets of my prison house
I could a tale unfold,

and not one only, but one or more from my personal experience of each of the most renowned of the national prototype standards that would cause you to admit that the porcupine would not evince any great fretfulness if he should let fly a quill or two at the kind of keeping those standards have had. But if such be the exactitude of weights & measures, what shall we say to expressing probabilities to several places of decimals, which is done every day?

Not only are the above reasonings deductive, but the following would be equally so.

The probability would be just one ~~sixth~~ third that if a sample of 125 balls has been taken at random out of a certain chest of balls, and that if one ball were taken from that sample at random it would prove to be marked with an E

∴ Therefore the probability is just one third that a ball taken at random from the whole chest would be marked with an E and exactly one third of all the balls in that chest were marked with an E,

This is deductive as long as the premisses do not speak of what has happened merely but of what would happen in the long run.

But now I will turn the leaf and write down a reasoning related to the first just as Aristotle says that Induction is related to ordinary necessary deduction; and you will see that this will be an Adduction,

57 to be no other explanation of his having
discovered the true nature of the relation
of Induction (or Quantitative Adduction)
to Deduction, I am all but ready to embrace
the opinion that he did ~~not~~ know something
of the doctrine of chances. I will mention one
other circumstance that squints, as it seems
to me, most singularly toward Aristotle's
knowing something of the doctrine of chances.

Namely, if Aristotle knew nothing of the
doctrine of chances, how can he have been
so stupid, so triply dull, as not to say to
himself, "If so fine and precious a method
of reasoning results from transposing
a syllogistic conclusion of the first figure
with the major premiss, could we not
gain another ~~by~~ way of reasoning by
transposing a syllogistic conclusion
and its minor premiss?" Must he ~~not~~ not
have asked himself that question? Yet if
he had thought of trying the experiment,
he would at once have found that there
are such reasonings in great number.
For instance, I see an animal somewhat
like a dog, yet entirely unlike any breed
I ever saw. I watch him & note that
he is decided by a home with human
beings. I say the syllogism is

The dog is fond of human beings
Suppose This animal is a dog
Then he should be fond of humans,
~~But that~~ is just what he is. I note that

our attention at this stage of our
studies. In the first place what a gulf
there is between the logical character
of the last reasoning and that of the
antepenultimate one, — the Adduction
and the Deduction. The deduction adds
nothing to our knowledge, whatever:
it only calls our attention to our
having already admitted something
that we may have noticed. The adduc-
tion does teach us something new. It
furnishes us with information, albeit only
approximate and provisional concerning
the whole lot sampled, on the evidence of
an insignificant sample. For it shows
that about 1/3 of ~~the~~ all the balls carry ~~an~~ Es
and that the other 2/3 do not.

In the next place, we remark that the
connexion between the probable deduction
and the adduction is evident, undeniable.
~~It~~ depends on the same two conditions
as the other. One may say that we accept
the conclusion of the adduction as showing
us how it comes about that 1/3 of our sam-
ple carry Es and the rest do not. The
conclusion is accepted as accounting
for this.

Next, we note that this is just Aris-
totle's ~~curious~~ theory of Crude Induction;
and we are disposed to admit his theory
because in the case of Quantitative Ad-
duction it is evidently correct. In fact,
nobody could miss that theory in the

Quantitative case. Aristotle's own instance by no means allows the close relationship to be perceived so easily. For my part, it seems to me so surprising that Aristotle should have discovered the connexion, or that having discovered it, he should see that that was the secret of the whole matter, that the more I think of it, the more astounding it seems. There is some mystery about this. A mystery? Greece was a great place for mysteries, — especially Thessaly and probably Macedonia. And Aristotle, the boss of Athens, enormously wealthy, is believed to have spent money in prying into mysteries. He seems to have had no head for mathematics. Trade secrets are the secrets that are kept the best. I know something of one or two, and of their histories. Gambling is the kind of trade that might have had secrets undisclosed for many centuries. I think the doctrine of chances is an instance in point. For in Todhunter's history of the subject, which is the standard work, it is told how the doctrine took its rise in a correspondence between Pascal and a certain Chevalier de Méré. This would be in or about 1654. You can find some account of it in Bayle's Dictionary under the article Zenon. As far as I remember (but this history of Probability is

not the only work of Todhunter's that I have to lament that I have have forgotten parts that I sadly need — fit to tear my hair, which I would ill spare!) as far as I remember Todhunter mentions nothing earlier on the subject. At any rate, most works date the beginning of the Doctrine of Chances from de Méré. But in so well known and entertaining a book as Libri's *Histoire des Mathématiques en Italie*, Vol. II. p. 188 footnote there is copied from a MS. a passage from a commentary on Dante, which Libri says was *printed* in 1477 so that it is earlier than that, and this passage undertakes to derive the frequencies of the different totals of the throws of 3 dice from the number of ways in which they can come about. It only gives specimen throws, and one of these is wrong, for he says that 4 can be thrown in only one way. But I think that may have been in order not to let the reader know too much. Any way, as soon as the idea was hit upon any clear headed gambler, — and gamblers are made clear headed by the survival of the fittest, — could easily see for himself that 4 can be thrown in 3 ways: 112, 121, 211. Now the same game with three dice is so frequently referred to in Greek tragedies, comedies, and philosophical works that it is pretty evident that the Greeks were a good deal addicted to it. And who can believe that none of the gamblers of Athens had found out how to calculate the chances of the game? Of course they would keep it secret, but Aristotle would know it, probably under an obligation of secrecy, which he was not the man to be unfaithful to. I really think this is antecedently more likely than not. And when I consider that there seems

[91]

all there are and call an argument which concludes some thing to be true of all of a class by making the premisses enumerate every instance of the class, this I say they call a "perfect induction." It is not an adduction at all. It is a necessary inference & therefore deductive, i.e. supposing the premiss to state, as Sir W. Hamilton for example requires that the list of instances are a complete ~~enumeration~~ the members of the class,

But understanding Aristotle to mean as I say all the instances of which the reasoner has sufficient knowledge to use them at all, or all he remembers, then the reasoning is what I should call "crude adduction."

I say

The dog takes several naps during the day and sleeps lightly at night

Supposing then this animal is a dog He ought to take several naps in the day time and sleep lightly at night, I watch him and find he does, and the syllogism would _explain_ his doing so, if he is a dog. But I should say

When a dog lies down he goes round and round several times before finally laying down

Supposing then this animal to be a dog He will turn round three or more times before he lies down.

I watch him, and find he does that. All these syllogisms _explain_ his behavior if he is a dog and are inexplicable if he is not a dog.

Surely Aristotle would have made a chapter on this kind of reasoning without fail, ~~if he~~ But he does n't! He never mentions it. Explain that most singular omission if you can!

Well, it needs no explanation if we suppose that Aristotle was looking at Induction from the point of view of the doctrine of chances. Because then he would find ~~this sort of~~ that he must keep the minor premiss ~~as~~ a premiss, since otherwise he could not ~~be~~ make any application of the doctrine of chances not having two premisses ~~both~~ speaking of a countable number of objects

In that way the omission is explicable and I do not see how else it is. This is the kind of reasoning that I call Qualitative Adduction. It enumerates qualities and circumstances though they are things ~~are~~ not capable of being counted, or rather ~~they~~ they have no sharp unmistakable boundaries so that there can be no doubt how they ought to be counted. Indeed we don't want to count them but we need to weigh them. But there is no simple unmistakable way of measuring them.

As it is Aristotle says in the chapter I have mentioned and in several other places ~~that there~~ are but two ways of reasoning By Syllogism and By Induction;

And now I will give the second layer that conclusively shows that

96 either Aristotle never dreamed of the doctrine of chances or if he did was determined not to let all ~~his~~ his students who ~~were allowed to refer to the MSS of his works~~ [we know from the numerous cross-references thereunto] [i.e. One treatise referring the reader to a second, and then this second referring the reader to the first. And many such references plainly show that the persons who made them did not understand what they read.] And that is that referring to his example of an induction where he speaks of "~~the~~ man, the horse, the mule" and he notes that ~~in place of~~ though he names merely the three, yet it is to be understood that all are meant and then adds "ἡ γὰρ ἐπαγωγὴ διὰ πάντων," — "for Induction draws its conclusion from all instances." He doubtless means from all of which one has ~~certain~~ sufficient knowledge although lots of logicians, I suppose the majority, understand him to mean

The original stoics were of a very different order of minds from those philistine moralists that the word "stoic" is apt to bring up to our fancy now. They were strict logicians. I do not say they were sound logicians, for, on the contrary, they were nominalists; and nominalism is deadly poison to any living reasoning. As to anything like adduction, they, very consistently with their nominalism, altogether condemned. They thought its *rôle* in the development of science could be filled by the *reductio ad absurdum*, a futile idea. They were already decidedly philistine, — by which I mean that they stuck to certain opinions as if they were known to be revealed to them by God; and would not criticize them as they did every suggestion opposed to them.

It is a striking illustration of how slight and fragmentary our knowledge of the world of ideas of the Greeks is, that were it not for a single roll of papyrus found in Herculaneum of which (according to my recollection) no one single sentence remains intact and complete, and most of it is mere despair-bringing words showing that a deeply interesting and most intelligent logical discussion had originally been written there, — I say, if it were not for this single fragmentary roll, we should be unable to deny, what many historians of philosophy writing since the discovery of this roll have continued to assert, namely, that the Epicureans had no system of logic. But fragmentary as this roll is, it enables us to assert with certainty that they did have a logic, and that that logic was as highly developed as that of John Stuart Mill, which it greatly resembles, while curiously contrasting with that. I note this as showing that the fact that we hear nothing of a knowledge of probabilities in Ancient Greece has hardly any force as an argument against such knowledge having existed, and in my opinion it does not affect the antecedent likelihood that there would be such knowledge. As to the special nature of the Epicurean doctrine of induction, it will be time enough to consider that when I come to Mill's theory.

[98] 3rd Some characters belong merely to individuals, others are uniform through any variety, others are specific, others generic, etc.

4th An object, especially a particular man, may be known usually to possess or to want the whole of some groups of characters if he possesses or wants any of them.

The knowledge of any such *uniformity* enables us to make certain quantitative or qualitative inductions with great confidence from only a few instances.

People have generalized all these so as to declare that "Nature is uniform; under the same circumstances you meet the same phenomenon." But this is only a vague expression of the fact that there are a great many special uniformities. Some will say "Oh it is more than that, I know that under precisely the same circumstances one finds *precisely* the same state of things *every time*." I ask him, How many times have you ever been in precisely the same circumstances? One of the circumstances that

As for Francis Bacon, (for there was nothing worth mention in the way of studies of the theory of Induction between Cicero's youth and the Novum Organum) I am pretty well satisfied that he never had any definite theory on the subject. He simply thought that some system of registry of observations and of ~~ex making~~ planning and making experiments might be devised by him that would render it possible to discover what he called the real "forms" of natural phenomena. But he never did and it was quite visionary. However, it is most remarkable that he should have lit upon the true nature of heat and should have rendered the true doctrine decidedly plausible and even likely, although he did not succeed in impressing the minds of physicists, whom even Count Rumford could not convince by experiments that ought to have convinced them. So that it was only when I was old enough to be keenly interested in physical science & its reasonings that the physicists ultimately came round to the "mechanical theory of heat" and to the doctrine of "energy" which they now treat as if it were much more Logically established than it really is. For in this world of uncertainties nothing whatever is more certain than that absolute certainty is unattainable, especially concerning real things; and if my system of Logic is not greatly at fault, ~~at~~ about the next ~~nearest~~ to certainty is that the quantitative exactitude of any statement about real things can never receive so much

as the smallest presumption for sound Logic.

~~When~~ Whately, whose Logic was first published in 1826, and the great majority of English logicians who have written since, base the trustworthiness of inductions on uniformities. There is an endless variety of uniformities or partial uniformities; but the simplest kinds are these four.

1st The members of a class may be known to be alike in certain respects. Thus all specimens of a substance of any definite chemical constitution will agree in a great many respects well known to chemists, such as specific gravity, solubility, etc., so that a chemist will need only to examine a single specimen in order to pronounce with confidence on all specimens as far as those characters are concerned.

2nd Certain sets of characters are intimately connected. Zoology is full of such connections so that if two or three of them are found in the same fossil a paleontologist will be pretty sure the animal possessed all the others they lead us to conform to; For instance, our instinct that a straight line or the nearest to a straight line that can be on a ~~curved~~ rounded surface is the shortest distance between two points is to be trusted because it has been developed in us under the influence of the fact that it really is so; ~~or~~ or under the influence of facts which cause our instincts to conform to them. At the same time, there is danger in following what appear to be our natural instincts too closely; and partly because we can not distinguish between true instincts and mere prejudices; as we see when in the book of Genesis, the creator is represented as having become so disgusted with the creatures that he had created with entire understanding of how they would act, that he resolves to exterminate them, until reflexion shows him, that he is acting like a dam fool, and he decides to save one family. There is a striking example of how we may be grossly misled by trusting too much to what seem to us "natural" beliefs & which, in a sense, really are

must have been the same was the year, the day, the hour, and the minute. I dare say that that single circumstance being the same everything else was so. No it means nothing. If you modify it so that it will mean anything it is easy to prove that it is false. The character of nature, its true characteristic is not uniformity but variety. The uniformities are to the varieties as one in a million. But owing to the fact that uniformities are useful to us & that varieties are not, we take note of the former & dismiss the latter from our minds.

The uniformities do help inductions. It is absurd to say they are the sole basis of Adductions.

But how were the uniformities one and all found out? Only by Induction or Adduction. Now it is strange doctrine that uniformities are the sole ground of inductions while inductions are the sole ground of uniformities.

No uniformities *help* some Inductions & render them more certain. But they have this power only because they are supported by Inductions. The *application* of a uniformity to an induction takes place by Deductive reasoning. But the only reason we have for believing in Adduction is that in the long run it must lead to the truth.

Skipping a great deal, I now take up the third great class of Reasonings, which I call Retroductions. By *Deduction* one infers that if certain premisses are exactly true, then a certain conclusion must be true, either always or once in ~~so often~~ a certain proportion of cases in the long run. By Adduction one infers that a certain state of things *is* true, at least approximately. By the third class of reasonings one only infers that a certain state of things *may be* true and that there are sufficient indications of its being so to warrant further examination. We must accept the Deductive conclusion because we have already assented to it, and consistency requires it. We accept the Adductive conclusion because though it may not be accurate, yet persistence in the same method of reasoning will diminish the error, if there be one, until it indefinitely approaches exactitude. The reason for accepting the Retroductive conclusion, is that man must trust to his power of getting at the truth simply because it is all he has to guide him; and moreover when we look at the instincts of various animals, we are struck with wonder at how they lead those creatures toward rational behaviour. For instance how wasps having no experience of the destiny of an egg will lay its eggs so that the young will find a kind of food that would not be food at all to the mother, even if she knew that any young were to come forth. Now man has equally powerful instincts though we do not recognize them any more than the wasp does hers, and for the same reason, because in following our instincts we, like her, seem to ourselves to be acting according to manifest good sense. So it is to us, though it would not be so but for our instincts. The reason we go right in the main in following our instincts, is the same as the reason that the wasp does so, Namely, it is because those instincts have been formed under the influence of those very laws of nature that undeceived. Retroduction on the other hand is the most impulsive of reasonings. There is really no reason to accept the conclusion except that we cannot help it or in its least impulsive form feel that it is the natural, the reasonable, the human way of thinking. If the child after his first fright begins to look closer and compare the looks of the object before him with the way a bear would look, he is no longer merely conjecturing; he is adducing new qualities.

But I am confident that not all Retroductions take on the form of Qualitative Adductions. Walking along one of those interminable French roads bordered by two interminable rows of poplars

so. Therefore all conclusions of Retroduction ought to be submitted to Adductive criticism, when there is an opportunity to do so. At the same time, one may often be misled by a modest willingness to surrender one's own Retroduction to the presumed wisdom of one who without any such divine light, brings apparent Adductive objections against one's Retroductive conclusions.

I do not, at present, feel quite convinced that any logical form can be assigned that will cover all "Retroductions." For what I mean by a Retroduction is simply a conjecture which arises in the mind. Now does not this often happen before we can formulate any judgment about the state of things that we are experiencing and which leads us to the conjecture? In my youth, however, I was accustomed to think that that very state of being unable to characterize what we experience may be regarded as predicating a confused jumble of characters of it. Thus, a child out of doors alone on a dark night and far from home has a feeling of timidity and looking at a dark object & trying to make out what it is, does not definitely make any description to ~~itself~~ himself of what he sees, but sees a dark object which frightens it and says to himself

That thing has a peculiar kind of dreadfulness.

~~A bear would have that very kind of dreadfulness~~

& guess it must be a bear.

This would be the same form of reasoning as a Qualitative Adduction. Thus, the question arises whether there is any difference in form between a Retroduction and a Qualitative Adduction. But the distinction that is undoubtedly of highest importance between Reasonings (and I call anything a "Reasoning" where one belief or tendency to believe causes another) is that which consists in the nature of the assurance being different. Now Adduction is the very coolest of all classes of Reasonings, for its conclusion is accepted provisionally because if it deceives us we have only to persist in the same kind of reasoning in order to be

I have not thoroughly considered the question. The first scientific paper I ever published was an attack on the atomic theory, and Sterry Hunt said I was right & continued to do so after I had quite given up my contention. But all this is aside from the point; which is that the existence of atoms no more accounts for the simple ratios of multiple proportions than the fact that a bag of coffee consists of separate "beans" goes toward proving that if two kinds of coffee are mixed in a bag it must be in some simple proportion. It was no reason at all; but what it was perhaps, was one of those cases in which men have made guesses apparently utterly unfounded and yet correct. No doubt many of these are cases

I notice that on one side each tree has a white-washed stone of about ten kilos at the foot of it. I am seized with an impulse to lift one of the stones and see what is under it. I yield to the impulse and dam'd if I dont find a louis d'or under it! I say to myself "There certainly can't be a gold piece under every one; and yet it's very strange that I should have been impelled to lift the very stone that had that strange deposit. That is a manifest case of Retroduction. But should I go on and lift two or three successively and I should find a gold piece under each it would be getting up to be a Quantitative Adduction. Here seems to be a case where Retroduction takes the form of Quantitative

Adduction. Though it might be said to be a case of reasoning from Consequent to Antecedent.

"If every stone covers a gold-piece" being the antecedent, a "This stone covers a gold piece" being the consequent.

However one of the most marvellous and unmistakable cases of what I call (quite unsuitably according to my present views) Retroduction seems to me clearly to have had neither the Quantitative nor the Qualitative form of Adduction. Namely, when Dalton discovered (as he supposed for the first time) that the chemical elements combine in the simplest of multiple proportions, he was seized with such an intense conviction that they were composed of atoms that he never thereafter for a moment doubted it; and ~~the strangest part of it~~ stranger still, to my mind, ~~is that~~ other chemists when they heard of Dalton's theory at once accepted it with hardly more of doubt than Dalton himself entertained. And the most marvellous circumstance of all is that there is now other evidence, absolutely proving (in the sense in which such things can be "absolutely proved") that bodies are composed of chemical molecules and single electrons or corpuscles have also been isolated; so that there seems to be no reason to doubt the reality of atoms though I doubt if one ought to think it anything approaching a certainty; though

111– of my life has been devoted to,
though I base it upon Critic,
Of course in order to study me-
thodeutic it is necessary to make
researches in as great a variety
of sciences as possible,—real re-
searches, not the twopenny halfpenny
"research work" that students of col-
leges do. Now I have always been
poor; and my father when I was
a boy was always on the fringe
of penury. In France as distinguished
a man of science as he was who
rescued his country from contempt
in regard to mathematics & astrono-
my would not have been so
poor. But even today the United
States is governed & led by men
actually below the average. Witness
the miserable tyranny that is ex-
ercised over the great business
men, the barbarous "Sherman act,"

of instinct. Twice in my life I have
had extraordinary experiences of that
sort.
I consider Retroduction (a poor
name) to be the most important kind
of reasoning, notwithstanding its
~~very~~ unreliable nature, because
it is the only kind of reasoning
that opens up new ground. De-
duction doesn't teach anything
but only draws attention to know-
ledge we may have overlooked.
Adduction only increases our
knowledge in various respects
without producing any new
knowledge. At least, it is not
at all likely to teach us anything
quite ~~new~~ novel and also
important unless where Retro-
duction furnishes a hint. But
Retroduction is ~~not~~ gives hints
that come straight from our
dear and adorable Creator;

We ought to labour to cultivate this <u>Divine</u> privilege. It is the side of human intellect that is exposed to influence from on high. With this investigation starts. Having once formed a conjecture, the first thing to be done is to draw Deductions from it and compare them with observation. So we correct the errors of our Retroductions by processes of Adduction.

So Retroduction comes first and is the least certain and least complex kind of Reasoning.

Deduction follows. It is as certain as are its premisses, no more so.

Finally Adduction ranges in complexity from a simple Crude Adduction up to elaborate

Quantitative Adductions which afford material for new Retroductions

I have now sketched my doctrine of Logical Critic, skipping a good deal. I recognize two other parts of Logic. One which may be called <u>Analytic</u> examines the nature of thought, not psychologically but simply to define what it is to doubt, to believe, to learn, etc., and then to base critic on these definitions is my real method, though in this letter I have taken The third branch of logic is <u>Methodeutic</u> which shows how to conduct an inquiry. This is what the greater part

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Now when you reflect that it takes usually 100 times as much labour of all kinds to reduce an error to $\frac{1}{10}$ of its previous amount. That is, if they had in one month succeeded in measuring anything to a millimetre, then it must be expected that a hundred months labor mental & physical would be required to measure the same thing accurately to a tenth of a millimetre; when you remember this you will see how my statement that their error was a hundred times as great as they supposed sounded! Well, the idea was evidently completely new to them & they didn't venture to say much about it. However, the bugbear of monopolies. Monopolies, such as can be created in our days are most beneficent for the public.

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Well, as soon as I had taken my academic degree, I was appointed an aid in the Coast Survey at $ 15 a month. That was an opportunity to learn my lessons in one science. I managed three years later to take a degree in chemistry, and I was the first in Harvard to take a degree in that science summa cum laude. In the Coast Survey I particularly made myself master of the subject of weights & measures. Later I was appointed to the charge of all the investigations of the Survey into gravity. I got leave

[116] to go abroad to study European methods of investigating gravity. While I was in Paris, there happened to be a conference of all the European Surveys. It was held in the <u>Palais des affaires étrangères</u>; and I received an invitation to attend the meetings. At the first I attended, the subject of gravity was discussed; and I was taken completely by surprise when the president, Gen. Ibañez, called upon me for my opinion of the work they had been doing. Of course, I was obliged to express my real opinion. They thought they were measuring gravity with errors not exceeding 1 or at most 2 millionths of itself. But the pendulum was swung from a

[117] brass tripod and I expressed the opinion very decidedly from an examination I had made of that tripod in Geneva that it swayed under the pendulum to an extent which though not directly observable, I had been able to get a notion of the amount of it by measuring how much the ~~would there~~ the part where the pendulum rested would be moved by a horizontal pull of 1 kilo's weight. Hence I concluded that all the values of gravity which they had been publishing during the past ten years were too small by about $\frac{1}{10000}$ of themselves, or a <u>hundred</u> times the error they thought they were excluding.

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to be at the time over at the Brooklyn Navy Yard or somewhere (I am sure I forget where but near at hand) another Coast Survey fellow who thought he was under some obligation to me, and who was a perfect master of Newspaper English. I sent to him to meet me the next morning at the Century Club (the old 15th St. house). And there he and I concocted half a dozen paragraphs — which my friend copied in his exquisite handwriting, — after we had put our heads together in the composition of them. Then that night I took a coupé so as to be down in Park Row as near midnight, just a bit later — as I would wish

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the next year they had another meeting in Brussels when three of the members who were supposed to be the most competent reported certain experiments (of the most ridiculous nature in my opinion. For instance, one of them had put a delicate spirit level on the stand of a half second pendulum & it didn't budge. This really seemed too much like an ostrich sticking his head under the sand. These delicate levels require nearly a minute to oscillate!) Any way they reported that "our American colleague" — in brief had found a mare's nest. I did not receive the report

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of that meeting until nearly a year later within about a fortnight to three weeks of a third meeting. Meantime with the apparatus I had procured abroad a duplicate of their most approved pattern made for me with the utmost care by the first mechanician of Europe I had ~~done~~ a lot of work and was by that time perfectly sure that the amount of the error that I had stated in Paris was as nearly as I stated it exactly right. I had had a good still stand made too and had proved the pendulum swing quicker or that by the calculated amount

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So I instantly applied for permission of absence. Well, the people in Washington were awed by the fame of the gentlemen who had reported against me in Brussels. Besides that, there was that stomach-turning jealousy — faugh! — to a scientific man who looks upon scientific work as an humble act of worship of his God and his Creator, — it is unspeakable! So the leave of absence was refused me. I was in New York, stationed there by official order. There happened

[126] graph sheets with me, and the whole demonstration was complete; and when I sat down each of my three antagonists at Brussels got up one after another & very handsomely admitted that I was entirely right. And from that time I was acknowledged as the head of that small branch or twig of science.

Another time when I was in a minority of about 2 with the entire brunt of the argument to sustain was when I saved the country from having the metric system made compulsory. Of course the only unit that is of any particular importance is the inch. That certainly was so at that time & probably is so now. I won't tell the story in writing because it was in a secret session. There were very near a hundred men red-hot against me, but I carried the point.

into each of the half dozen leading morning papers — & tell the fellows at the desk with an air of great authority "Send that up to the Night Editor, and tell him to put it right in, without fail!" & then I would leave in equal haste. Well the next morning, sure enough, three of the best papers had our paragraph, and among them the Tribune. When I saw that, for the paragraph had almost the tone of an imperative order to be executed at once, I laughed heartily & felt the thing was done. And early in the day I think it was about noon — I had not a leave of absence but an order to go & represent the

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Survey. You may believe that I got on board the first steamer. I was landed at Plymouth & travelled right through night & day to Stuttgart where was the meeting I got ~~the~~ to the hotel in the evening during dinner. I knew there were 2 men who believed in me, or rather $1\frac{1}{3}$. The one was Genl. Baeyer the leader of European geodesy. The $\frac{1}{3}$ was a fraction of Mon Emile Plantamour, who had seen me at work in Geneva. I met Genl. Baeyer and his daughter in the corridor of the hotel as I was being shown to my room & the old General who had been fighting for me all day but really did not know much about the subject was so delighted to see me that he threw his arms round me and kissed me on both cheeks! The next

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morning I went into the meeting which was a particularly distinguished gathering, several men who were not regular geodesists being among them, as Henri Ste. Claire Deville, M. Faye etc. I began with the mathematical theory which I had, in coming across, succeeded in putting into a form in which every man of them could see the correctness of it. Then I described the instrument by which I had automatically registered the instants of the passage of the pendulum over the vertical, while it was swinging on the brass tripod & when it was on a properly stiff support. I had the chrono-

order to have that character must
pick the reasoning to pieces into as
many steps as possible, and therefore
They are intentionally made to reach
The conclusion as slowly as possible.
I know by keeping tab on myself and others
that I am one of the most accurate deduc-
tive reasoners widely known; and I can
testify that this Syntax has helped me
times and again to get my reasoning right.
Here is an instance of the kind that nobody
would quite relish making public, but I
put no embargo on it, thinking it my duty to
be frank in such matters. Nor is there any
merit in not laying any such embargo, since
to ~~be~~ have one's acquirements recognized, if
they are unusual, is more inconvenient than
otherwise. I was the other day rather idly
thinking of what I might say in something I
was writing, and it occurred to me that a
complete statement of the interrelations
of whole numbers not negative would be to
say 1st that there is ~~a lowest such number, 2nd~~
one such number that is higher than none; 2nd
that for every such number there is one and only one [illegible] num-
ber higher than it, but not higher than one itself
higher than it (i.e. next higher) ~~and 3rd that~~
~~whatever is true of any such number if [illegible]~~
~~[illegible]~~ and 3rd that of any two such
numbers one is higher than the other. If I had really
been at work, I should not have made such a mistake;
but as it was for the moment I thought this would
suffice (or at any rate would if I added that no
said number is next higher than more than a single such
number) to prove that if any property belongs to the

draft of [illegible]

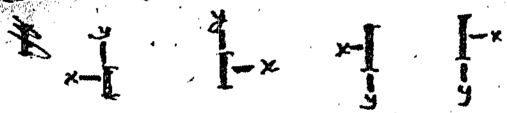
take the aim of the invention, but as
it were to suppose it to be the direct
contrary of what it is. Especially would
a person at all familiar with other
logical inventions made during the
past seventy years ~~&~~ naturally sup-
pose that my invention had the same
purpose as those others, — namely, that
of enabling a person to draw the ne-
cessary conclusion from any given
premisses as simply and speedily as
possible. ~~Now~~ that is, as nearly as pos-
sible, the CONTRARY of the purpose of
my Syntax. For after much experience
of those inventions, including two of my
own invention (of which the less valuable has
been expounded, by expansion of the 17
duodecimo pages in which I had sufficiently
described it, to an octavo of 649 pages
devoted exclusively to that, by a German of
course) I say my experience of those sys-
tems is that while they afford wholesome
exercise in reasoning about exceptionally
intricate subjects, they are not at all needed
by a trained mind; and untrained minds
when they meet, say twice or thrice in a life-
time, with one of those exceptional cases, can
as easily invent upon the spot a diagram to
steer them through the special case as carry
in mind the rules of a system so seldom called
into use. But on the other hand, my expe-
rience has been very many times to find

exceptionally powerful intellects, — mathematicians, logicians, economists, famed for their great reasoning powers, — who having found some curious way of reasoning to work all right in many instances have been led to apply it to others and so to reach very false conclusions; and the reason has been that their familiarity with cases in which the reasoning has worked well has seduced them into thinking that a conclusion so drawn <u>evidently must</u> be true, when the truth is that in all the cases where it worked something that their attention never had been drawn to had been true and this unnoticed truth had rendered the conclusion a necessary result, not of what they had considered the premisses, but of these together with the unnoticed circumstance. Euclid is full of such fallacies. (The 16th proposition of the first book is one) The greatest modern mathematicians have fallen into them. I remember hearing myself one of the giants of modern algebra ~~go~~ make a statement, in a lecture, with considerable exultation, which I happened to know was false. After the lecture I went to him and gave an example where it was manifestly false. "Oh yes," he said, "but it is true in the <u>general case</u>." Now what algebraists mean by the "general case" means what is assumed to be true in speaking of quantities denoted by letters, but which may happen to be false for certain special numbers. For example if $\frac{x}{y} = \frac{z}{y}$, it is true in the general case that $x = z$. But if y is zero or infinite it need not be true. Now the discovery of my lecturer looked true in the general case, but I had already proved that, as a matter of fact, there are only 3 cases out of ~~an~~ an absolutely innumerable class of cases in which it is <u>true</u>.

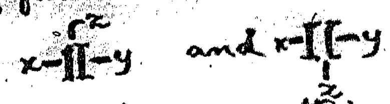
Now these cases need a check upon them; and besides, it is very unsatisfactory to be able only to say that one proposition <u>appears to be</u> a necessary consequence of others without one can show just <u>how</u> it "<u>must be</u>" true. I have stated that a consequence follows necessarily only in case it asserts no ~~other~~ state of facts that has not been already asserted in the premisses from which it is supposed necessarily to follow. Kant had a glimmer of this truth when he said that syllogistic reasoning is "explicatory reasoning." My three permissions state the truth more explicitly. Whatever cannot be produced by manipulating the premisses by those three permissions does not follow logically from the premisses. These ~~premisses~~ permissions, in who tries to show what are the different kinds and degrees of trustworthiness of reasonings, ~~and~~ how to distinguish them both in kind and in degree, and how best to conduct the different kinds of inquiries.

I will now proceed to fulfill my promise to give some examples of the application of the Syntax of Existential Graphs to Deductive, i.e., necessary reasoning.

For this purpose there are certain individual graphs which I will recommend for the sake of facilitating the interpretation of graphs scribed by one person when the interpreter is to be another person. In the first place, I recommend the three first Roman numerals to be so used that, whatever graphs x, y, and z may be,



shall all assert precisely the same thing, namely, that some x possesses the some character that is y; further that



shall assert the same thing, namely, that some x stands in some relation that is z to some y; and finally that

take the purpose of the invention ~~and~~ even to assume that the effort of the inventor has been directed towards a result that he has really been trying with all his might and main to shun! A person who has only studied logic a short time, and even then with small interest and distracted attention will be quite apt to suppose that the logician and the mathematician have substantially similar purposes; since they seem to him to be alike straining out gnats, and fatiguing him with their bare A's, and B's and C's, X's, Y's, and Z's, et cetera ad infinitum. I have noticed in particular that those who have written at great length about logical algebra have quite failed to see the opposition of the two aims, their own being that of the mathematician first and foremost, and that is the reason that while I found 17 pages of duodecimo sufficient substantially to exhaust the logical interest of the algebra of dyadic relations that I invented, a German author has found 649 pages of octavo necessary; and after his death left materials for another volume on this subject. Yet that invention did nothing for logic that the simple syntax of existential graphs does not do better. The distinction between the two conflicting aims results from this, that the mathematical demonstrator seeks nothing but the solution of his problem; and, of course, desires to reach that goal in the smallest possible number of steps; while what the logician wishes to ascertain is what are the distinctly different elementary steps into which every necessary reasoning can be broken up. The mathematician will thus seek to state a proof in the smallest possible number of inferential steps that are perfectly evident; while the logician, on the contrary, will endeavour to analyze the proof into the greatest possible number of elementary ~~steps~~ indivisible steps that are unlike one another and that are really indispensible to a proof that is to be broken up into indivisible steps. In short, the mathematician wants a pair of seven-league boots, so as to get over the ground as expeditiously as possible. The logician has no purpose of getting over the ground; he regards an offered demonstration as a bridge over a cañon, and himself as the inspector who must narrowly examine every element of the truss because the whole is in danger unless every tie and every strut is ~~and~~ not only correct in theory but also flawless in execution; that is why he has no occasion for swift progression but does need minute care of every element of the structure. But hold! Where am I going? Metaphors are treacherous—far more so than bridges; for they tempt us to wander from our destination, —a treachery of which nothing is more innocent than a bridge. The logician is not an inspector of reasonings; he is only a person cal. But to a clear mind, or one trained in Deductive Reasoning there is nothing paradoxical about it. It is a matter of course.

Only it might be called a miracle if it were true that in the long run the frequency of the die turning up ⚅ would be to that of its turning up other sides, ~~if the~~ — it might, I say well be reckoned a miracle if this ratio should happen to be precisely equal to any ratio of one whole number to another, however large the two numbers might be. For to say that the ratio would be like $\sqrt{2}$, or $\sqrt[3]{2}$, or π, the ratio of the circumference to the diameter, which we call "irrational" values, by a stupid attempt at translation the Greek ἄλογος, which means, not "irrational", but "inexpressible," i.e. inexpressible as the exact ratio of one whole number to another. If I can squeeze it into this letter, I will show you that multitude of all rational numbers is but a beggarly infinity, while the number of irrational ones is 2^{∞}. To call this infinitely greater is a ridiculous understatement. To say that 2^{10} is ten times as great as 10 is a bad understatement, since $2^{10} = 1024$; but to call 2^{∞} infinitely greater than ∞ is so much worse an understatement that I shall have to give it up.

Of course if the probability has really an irrational value, as I suppose it nearly always is there is no ratio that would not ultimately prove either too small or too large

$+\infty$ or $-\infty$ (the logarithm of zero being negative infinity, since a logarithmic infinity is of a lower order than the ordinary infinity and its negative is not the equal of itself, while an ordinary infinity being equal to $\frac{1}{0}$ and $+0 = -0$, it follows that it equals its own negative. Whether these formulae have any meaning or not, we have to admit them.) But there are indisputable differences in the magnitude of different infinite collections. The magnitude of a collection I call the "multitude" of that collection; and perhaps I was the first to prove strictly that there is an endless series of different infinite multitudes. If I get room I will insert this proof stated in the syntax of existential graphs.)

To return to our muttons, after the first throw of the die, that one of the two tallies (that of A and that of B) that shows zero may chance to continue to show 0 for an indefinite number of throws of the die. But it will not do so forever; and as soon as both tallies have left the value 0, every throw must change the result of C and consequently that of D and of E (for if we write c for C's result when that of D is d we have $d = \frac{c}{1+c}$ and $c = \frac{d}{1-d}$, so that when one changes, so does the other; and when very high numbers are reached any change of d divided by $1-d$ is equal to the simultaneous change of c divided by $1+c$.)

Since it is an essential feature of the kind of situation one refers to when one properly talks of probability, that it makes no difference whether the tallies begin with the very first throw of the die after it is made or at the hundredth, or the hundred millionth, or where you will, it necessarily follows that the fluctuations of the values of c and d will be, about the those

sandth throw substantially on the
average, one ~~tenth~~ as great as about the
hundredth throw, that on the whole for
the most part, the fluctuations will get
smaller and smaller. For though in a very
long series of throws exceptionally large
fluctuations are more likely than in short
series, yet these are as likely to occur
at the beginning as at any other point.
Taking all things into account, it is gene-
rally stated, and no doubt correctly
than the departure from the mean of
the sum of any number N of throws
is likely to be $\sqrt{N}$ times as great as
that of any one throw, and consequently
the departures from the mean, and the
size of the fluctuations of the values
of c and d will be inversely as the
square roots of whole tally of A, or of B, or of their sum.
I have not recently critically reexamined this
point, but I believe it is correct; and at
any rate these fluctuations must get
smaller and smaller, and consequently
as time goes on and the tallies increase
extreme values of c and of d are undoubt-
edly ceasing ever to reappear, although
we never can know for certain that any
value never will appear again (barring
the initial 0 or ∞). But if it should be true
that "in the long run the side $\boxed{::}$ of the die
should turn up some number M
of times to every N (some other number) of

times that some other side would turn up" to say that would mean
that as the tallies increased C's result in its fluctuations would
from time to time ~~cease for ever after to take values as large~~
as Take one value and another absolutely for the last time
(though nobody would know when this happened) and forevermore would take
only smaller values than that, while from time to time it would take one
and another value equally for absolutely the last time and would never
again take so small a value; and one would further mean that
there would be no value whatever of C's result that would not ulti-
mately cease forever to appear excepting only the value M/N which
would never cease to be traversed, the quotient of the number of A's tally
divided by B's, never ceasing to cross from being smaller than M/N to being larger
and then again to being smaller; and when one said that M/N is the precise
value "in the long run" one would mean that this was the only value that
would not sooner or later disappear for ever from being traversed in the
changes of C's result, that alone never would cease from being traversed
although every time it was traversed an infinite multitude of other values
would get traversed. To a person who reasons by the jingle of words (as many
and many do, if their procedure be allowed the name of "reasoning") this sounds paradoxi-

draft Sheet A CSP-J4K p. 31A

In continuation of the consideration of Probable Induction.

It is true, of course, that a finite proportion of an infinite collection is infinite. But then, even if there be any collection of single objects that is infinite, which is open to question, it would seem presumable that man can never have experience of such a multitude. For no single man can have distinct recognition of such a multitude of objects; and all the men there ever will be ~~must~~ be finite in number. An infinite ~~multitude~~ collection must, therefore, it would seem, if we are to know it be either a collection of can-bes or else of would-bes.

5 4

$\frac{5}{2} \cdot 4 \cdot 3$

$\frac{5 \cdot 4 \cdot 3}{2}$

Sheet A CSP-… p39A

In continuation of the consideration of Probabl^2 Induction.

It must never be forgotten that an infinite ~~multitude~~ collection contains an infinite multitude of collections that are parts of it, yet each is of a multitude equal to the whole. Indeed it not only contains an infinite multitude of parts each equal to itself, but this multitude is infinitely greater than the multitude of individuals it contains.

Consequently, it means nothing to say of an infinite collection that a part of it bears any particular finite proportion to the whole (further than that it means that that part of it is likewise infinite.)

It is true that physicians, for example, not reflecting upon this (any more than any other considerable part of the community does) will use such an expression as "About 2 per cent of all persons wounded in the liver recover." What they ought to say is "About 2 per cent of persons wounded in the liver, taken in the order of their coming to medical notice

In my opinion the presumption is that a more or less similar explanation applies to bodies not containing carbon but having right-handed and left handed crystals, and that in any case HgS cannot for an instant be considered as the structural formula of cinnabar. But as long as such an opinion is not absolutely disproved it cannot be said that merely showing that a specimen is "cinnabar" renders its chemical nature certain, or that there is any sound necessary inference as to the chemical species, since a chemical species is a class of substances entirely of the very same chemical characters. But it is as good as certain, in my opinion, that there are three kinds of red sulphide of mercury.

draft of CSP–JHK 6/28/'12 [44]

a probable deduction". Though I so call it, it would be a necessary reasoning, as the word "deduction" implies. It would be so because it concludes nothing that is not asserted in the premisses. For to say that the probability is $2/3$ is to assert neither more nor less than that in the long run it would so happen in $2/3$ of the times; or, to be still more explicit, that if tallies were kept as supposed, the value of the quotient of one divided by the other would never cease to traverse the value $\frac{2}{1}$ while there would be no other value that would not sooner or later be traversed for the last time.

All this is fairly easy to convince oneself of. But it requires close logical thinking to assure oneself that the reasoning from the characters of the specimen to its belonging to the species (unless, indeed, the species is defined by those very characters; but merely I am supposing that cinnabar is known to be a red, or crystalline, variety of mercuric sulphide, HgS, where Hg is 200, and S 32, or thereabouts, in mass) That conclusion, I perfectly admit, is a sound one. But I utterly deny that it is a necessary one, since we can perfectly well imagine the premiss to be true and yet this conclusion to be false. It is not even a probable deduction.

I ought to state that cinnabar belongs, like quartz to what is called "trapezohedral class of the rhombohedral system," and like quartz it has right-handed and left-handed crystals, as well as twinned crystals formed by the union of a right handed and a left-handed crystal. Now, you probably know that Pasteur discovered that Racemic acid is a mixture of right handed and left handed tartaric acid which are recognized as chemically different although their chemical reactions with all ordinary chemical bodies, i.e. those which do not rotate a ray of polarized light are identical. For with many bodies that are so "optically active" they behave very differently, though the one acts toward a body that rotates the beam of light to the left just the same as the other acts toward the variety of the same substance that turns the beam to the right, and vice versa. In the organic bodies that are optically active, it has been shown by the labours of Van't Hoff and LeBel (working independently,) that ~~all organic bodies that have~~ this is always due to their containing an "unsymmetrical carbon atoms." A carbon atom appears to have four poles or pegs to one of which any other atom that is to combine with the carbon atom must be attached. Now in case each of these four pegs is joined to an atom of any other substance and the four ~~and~~ atoms so combined with the one carbon atom are all of different elements there seems to be invariably a right handed and a left handed variety of the compound. The theory which accounts for this is probably true in the main. It is that a carbon atom has the form of a regular tetrahedron or triangular pyramid having all its ~~six~~ edges equal. Now if Fig. 18 represents such an atom resting on its face ~~ABD~~ ABC with its vertex D nearer to the eye, it can be turned over into any of

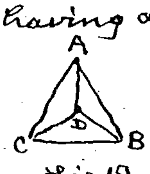


Fig 18

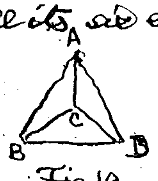


Fig 19

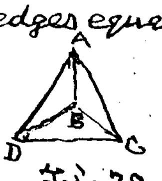


Fig 20

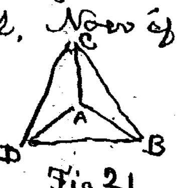


Fig 21

the three positions shown in Figs 19, 20, 21; but it can never be put into any of the positions ~~represented~~ in Figs 22, 23, 24, 25 ~~although these~~ which represent possible positions of one pyramid.

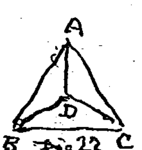


Fig 22

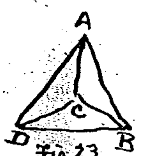


Fig 23

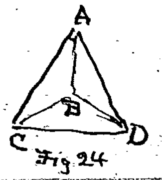


Fig 24

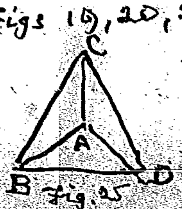


Fig 25

6/22/11?

In my own feeling, whatever I did in any other science than logic was only an exercize in methodeutic and as soon as I had the <u>method</u> of investigation thoroughly shown my interest dropped off.

F.cu II α

draft of CSP-JHK 6/22/11?

sider the suggestions of your letter, I want for my own sake and for the sake of your good opinion of me, to tell you a little more of the events which have interfered with my work.