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Title: The Logic of Chance, 3rd edition

An Essay on the Foundations and Provice of the Theory of Probability

Author: John Venn

Release Date: June 19, 2018 [EBook #57359]

Language: English

Character set encoding: UTF-8

\*\*\* START OF THIS PROJECT GUTENBERG EBOOK THE LOGIC OF CHANCE, 3RD EDITION \*\*\*

Produced by Juliet Sutherland, Andrew D. Hwang, and the

Online Distributed Proofreading Team at http://www.pgdp.net.

Transcriber's Note

Minor typographical errors and irregularities have been corrected.

Footnotes have been numbered sequentially within chapters and moved

to the end of each chapter.

THE

LOGIC OF CHANCE

AN ESSAY

ON THE FOUNDATIONS AND PROVINCE OF

THE THEORY OF PROBABILITY,

WITH ESPECIAL REFERENCE TO ITS LOGICAL BEARINGS

AND ITS APPLICATION TO

MORAL AND SOCIAL SCIENCE AND TO STATISTICS,

BY

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CAMBRIDGE.

LATE EXAMINER IN LOGIC AND MORAL PHILOSOPHY IN THE

UNIVERSITY OF LONDON.

"So careful of the type she seems

So careless of the single life."

\_THIRD EDITION, RE-WRITTEN AND ENLARGED.\_

London:

MACMILLAN AND CO.

AND NEW YORK

1888

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\_First Edition printed\_ 1866.

\_Second Edition\_ 1876.

\_Third Edition\_ 1888.

PREFACE TO FIRST EDITION

Any work on Probability by a Cambridge man will be so

likely to have its scope and its general treatment of the

subject prejudged, that it may be well to state at the outset

that the following Essay is in no sense mathematical. Not

only, to quote a common but often delusive assurance, will

'no knowledge of mathematics beyond the simple rules of

Arithmetic' be required to understand these pages, but it is

not intended that any such knowledge should be acquired by

the process of reading them. Of the two or three occasions

on which algebraical formulæ occur they will not be found to

form any essential part of the text.

The science of Probability occupies at present a somewhat

anomalous position. It is impossible, I think, not to

observe in it some of the marks and consequent disadvantages

of a \_sectional\_ study. By a small body of ardent students it

has been cultivated with great assiduity, and the results they

have obtained will always be reckoned among the most extraordinary

products of mathematical genius. But by the

general body of thinking men its principles seem to be

regarded with indifference or suspicion. Such persons may

admire the ingenuity displayed, and be struck with the profundity

of many of the calculations, but there seems to

them, if I may so express it, an \_unreality\_ about the whole

treatment of the subject. To many persons the mention of

Probability suggests little else than the notion of a set of

rules, very ingenious and profound rules no doubt, with which

mathematicians amuse themselves by setting and solving

puzzles.

It must be admitted that some ground has been given

for such an opinion. The examples commonly selected by

writers on the subject, though very well adapted to illustrate

its rules, are for the most part of a special and peculiar

character, such as those relating to dice and cards. When

they have searched for illustrations drawn from the practical

business of life, they have very generally, but unfortunately,

hit upon just the sort of instances which, as I shall endeavour

to show hereafter, are among the very worst that could

be chosen for the purpose. It is scarcely possible for any

unprejudiced person to read what has been written about the

credibility of witnesses by eminent writers, without his experiencing

an invincible distrust of the principles which they

adopt. To say that the rules of evidence sometimes given

by such writers are broken in practice, would scarcely be

correct; for the rules are of such a kind as generally to defy

any attempt to appeal to them in practice.

This supposed want of harmony between Probability and

other branches of Philosophy is perfectly erroneous. It

arises from the belief that Probability is a branch of mathematics

trying to intrude itself on to ground which does not

altogether belong to it. I shall endeavour to show that this

belief is unfounded. To answer correctly the sort of questions

to which the science introduces us does generally demand

some knowledge of mathematics, often a great knowledge,

but the discussion of the fundamental principles on which

the rules are based does not necessarily require any such

qualification. Questions might arise in other sciences, in

Geology, for example, which could only be answered by the

aid of arithmetical calculations. In such a case any one

would admit that the arithmetic was extraneous and accidental.

However many questions of this kind there might

be here, those persons who do not care to work out special

results for themselves might still have an accurate knowledge

of the principles of the science, and even considerable

acquaintance with the details of it. The same holds true in

Probability; its connection with mathematics, though certainly

far closer than that of most other sciences, is still of

much the same kind. It is principally when we wish to

work out results for ourselves that mathematical knowledge

is required; without such knowledge the student may still

have a firm grasp of the principles and even see his way to

many of the derivative results.

The opinion that Probability, instead of being a branch of

the general science of evidence which happens to make much

use of mathematics, \_is\_ a portion of mathematics, erroneous as

it is, has yet been very disadvantageous to the science in

several ways. Students of Philosophy in general have thence

conceived a prejudice against Probability, which has for the

most part deterred them from examining it. As soon as a

subject comes to be considered 'mathematical' its claims

seem generally, by the mass of readers, to be either on the

one hand scouted or at least courteously rejected, or on the

other to be blindly accepted with all their assumed consequences.

Of impartial and liberal criticism it obtains little

or nothing.

The consequences of this state of things have been, I

think, disastrous to the students themselves of Probability.

No science can safely be abandoned entirely to its own devotees.

Its details of course can only be studied by those who

make it their special occupation, but its general principles

are sure to be cramped if it is not exposed occasionally to

the free criticism of those whose main culture has been of

a more general character. Probability has been very much

abandoned to mathematicians, who as mathematicians have

generally been unwilling to treat it thoroughly. They have

worked out its results, it is true, with wonderful acuteness,

and the greatest ingenuity has been shown in solving various

problems that arose, and deducing subordinate rules. And

this was all that they could in fairness be expected to do.

Any subject which has been discussed by such men as

Laplace and Poisson, and on which they have exhausted all

their powers of analysis, could not fail to be profoundly

treated, so far as it fell within their province. But from this

province the real principles of the science have generally

been excluded, or so meagrely discussed that they had better

have been omitted altogether. Treating the subject as mathematicians

such writers have naturally taken it up at the

point where their mathematics would best come into play,

and that of course has not been at the foundations. In the

works of most writers upon the subject we should search in

vain for anything like a critical discussion of the fundamental

principles upon which its rules rest, the class of

enquiries to which it is most properly applicable, or the

relation it bears to Logic and the general rules of inductive

evidence.

This want of precision as to ultimate principles is perfectly

compatible here, as it is in the departments of Morals

and Politics, with a general agreement on processes and

results. But it is, to say the least, unphilosophical, and

denotes a state of things in which positive error is always

liable to arise whenever the process of controversy forces us

to appeal to the foundations of the science.

With regard to the remarks in the last few paragraphs,

prominent exceptions must be made in the case of two recent

works at least.[1] The first of these is Professor de Morgan's

\_Formal Logic\_. He has there given an investigation into the

foundations of Probability as conceived by him, and nothing

can be more complete and precise than his statement of

principles, and his deductions from them. If I could at all

agree with these principles there would have been no necessity

for the following essay, as I could not hope to add

anything to their foundation, and should be far indeed from

rivalling his lucid statement of them. But in his scheme

Probability is regarded very much from the Conceptualist

point of view; as stated in the preface, he considers that

Probability is concerned with formal inferences in which the

premises are entertained with a conviction short of absolute

certainty. With this view I cannot agree. As I have entered

into criticism of some points of his scheme in one of the

following chapters, and shall have occasion frequently to refer

to his work, I need say no more about it here. The other

work to which I refer is the profound \_Laws of Thought\_ of

the late Professor Boole, to which somewhat similar remarks

may in part be applied. Owing however to his peculiar

treatment of the subject, I have scarcely anywhere come

into contact with any of his expressed opinions.

The view of the province of Probability adopted in this

Essay differs so radically from that of most other writers on

the subject, and especially from that of those just referred to,

that I have thought it better, as regards details, to avoid all

criticism of the opinions of others, except where conflict was

unavoidable. With regard to that radical difference itself

Bacon's remark applies, behind which I must shelter myself

from any change of presumption.--"Quod ad universalem

istam reprehensionem attinet, certissimum vere est rem reputanti,

eam et magis probabilem esse et magis modestam,

quam si facta fuisset ex parte."

Almost the only writer who seems to me to have expressed

a just view of the nature and foundation of the rules

of Probability is Mr Mill, in his \_System of Logic\_.[2] His

treatment of the subject is however very brief, and a considerable

portion of the space which he has devoted to it is

occupied by the discussion of one or two special examples.

There are moreover some errors, as it seems to me, in what

he has written, which will be referred to in some of the

following chapters.

The reference to the work just mentioned will serve to

convey a general idea of the view of Probability adopted in

this Essay. With what may be called the Material view of

Logic as opposed to the Formal or Conceptualist,--with that

which regards it as taking cognisance of laws of things and

not of the laws of our own minds in thinking about things,--I

am in entire accordance. Of the province of Logic, regarded

from this point of view, and under its widest aspect, Probability

may, in my opinion, be considered to be a portion. The

principal objects of this Essay are to ascertain how great a

portion it comprises, where we are to draw the boundary between

it and the contiguous branches of the general science

of evidence, what are the ultimate foundations upon which its

rules rest, what the nature of the evidence they are capable

of affording, and to what class of subjects they may most fitly

be applied. That the science of Probability, on this view of

it, contains something more important than the results of a

system of mathematical assumptions, is obvious. I am convinced

moreover that it can and ought to be rendered both

interesting and intelligible to ordinary readers who have any

taste for philosophy. In other words, if the large and growing

body of readers who can find pleasure in the study of

books like Mill's \_Logic\_ and Whewell's \_Inductive Sciences\_,

turn with aversion from a work on Probability, the cause in

the latter case must lie either in the view of the subject or

in the manner and style of the book.

I take this opportunity of thanking several friends,

amongst whom I must especially mention Mr Todhunter, of

St John's College, and Mr H. Sidgwick, of Trinity College,

for the trouble they have kindly taken in looking over the

proof-sheets, whilst this work was passing through the Press.

To the former in particular my thanks are due for thus

adding to the obligations which I, as an old pupil, already

owed him, by taking an amount of trouble, in making suggestions

and corrections for the benefit of another, which few

would care to take for anything but a work of their own.

His extensive knowledge of the subject, and his extremely

accurate judgment, render the service he has thus afforded

me of the greatest possible value.

GONVILLE AND CAIUS COLLEGE

\_September\_ 1866.

1. I am here speaking, of course, of

those only who have expressly treated

of the foundations of the science. Mr

Todhunter's admirable work on the

\_History of the Theory of Probability\_

being, as the name denotes, mainly

historical, such enquiries have not

directly fallen within his province.

2. This remark, and that at the

commencement of the last paragraph,

having been misunderstood, I ought

to say that the only sense in which

originality is claimed for this Essay

is in the thorough working out of the

Material view of Logic as applied to

Probability. I have given a pretty

full discussion of the general principles

of this view in the tenth

chapter, and have there pointed out

some of the peculiarities to which it

leads.

PREFACE TO SECOND EDITION

The principal reason for designating this volume a second

edition consists in the fact that the greater portion of what

may be termed the first edition is incorporated into it. Besides

various omissions (principally where the former treatment

has since seemed to me needlessly prolix), I have added

new matter, not much inferior in amount to the whole of

the original work. In addition, moreover, to these alterations

in the matter, the general arrangement of the subject

as regards the successive chapters has been completely

changed; the former arrangement having been (as it now

seems to me) justly objected to as deficient and awkward in

method.

After saying this, it ought to be explained whether any

change of general view or results will be found in the present

treatment.

The general view of Probability adopted is quite unchanged,

further reading and reflection having only confirmed

me in the conviction that this is the soundest and most

fruitful way of regarding the subject. It is the more necessary

to say this, as to a cursory reader it might seem

otherwise; owing to my having endeavoured to avoid the

needlessly polemical tone which, as is often the case with

those who are making their first essay in writing upon any

subject, was doubtless too prominent in the former edition.

I have not thought it necessary, of course, except in one or

two cases, to indicate points of detail which it has seemed

necessary to correct.

A number of new discussions have been introduced upon

topics which were but little or not at all treated before. The

principal of these refer to the nature and physical origin of

Laws of Error (Ch. II.); the general view of Logic, and consequently

of Probability, termed the Material view, adopted

here (Ch. X.); a brief history and criticism of the various

opinions held on the subject of Modality (Ch. XII.); the

logical principles underlying the method of Least Squares

(Ch. XIII.); and the practices of Insurance and Gambling,

so far as the principles involved in them are concerned

(Ch. XV.). The Chapter on the Credibility of Extraordinary

Stories is also mainly new; this was the portion of the former

work which has since seemed to me the least satisfactory,

but owing to the extreme intricacy of the subject I am far

from feeling thoroughly satisfied with it even now.

I have again to thank several friends for the assistance

they have so kindly afforded. Amongst these I must prominently

mention Mr C. J. Monro, late fellow of Trinity. It is

only the truth to say that I have derived more assistance from

his suggestions and criticisms than has been consciously obtained

from all other external sources together. Much of this

criticism has been given privately in letters, and notes on

the proof-sheets; but one of the most elaborate of his discussions

of the subject was communicated to the Cambridge

Philosophical Society some years ago; as it was not published,

however, I am unfortunately unable to refer the reader to it.

I ought to add that he is not in any way committed to any

of my opinions upon the subject, from some of which in fact

he more or less dissents. I am also much indebted to

Mr J. W. L. Glaisher, also of Trinity College, for many hints

and references to various publications upon the subject of

Least Squares, and for careful criticism (given in the midst

of much other labour) of the chapter in which that subject is

treated.

I need not add that, like every one else who has had to

discuss the subject of Probability during the last ten years,

I have made constant use of Mr Todhunter's History.

I may take this opportunity of adding that a considerable

portion of the tenth chapter has recently appeared in the

January number of \_Mind\_, and that the substance of several

chapters, especially in the more logical parts, has formed part

of my ordinary lectures in Cambridge; the foundation and

logical treatment of Probability being now expressly included

in the Schedule of Subjects for the Moral Sciences Tripos.

\_March\_ 1876.

PREFACE TO THIRD EDITION

The present edition has been revised throughout, and in fact

rewritten. Three chapters are new, viz. the fifth (On the

conception of Randomness) and the eighteenth and nineteenth

(On the nature, and on the employment, of Averages).

The eighth, tenth, eleventh, and fifteenth chapters have been

recast, and much new matter added, and numerous alterations

made in the remaining portions.[1] On the other hand

three chapters of the last edition have been nearly or

entirely omitted.

These alterations do not imply any appreciable change of

view on my part as to the foundations and province of

Probability. Some of them are of course due to the necessary

changes involved in the attempt to write up to date upon a

subject which has not been stationary during the last eleven

years. For instance the greatly increased interest now

taken in what may be called the Theory of Statistics has

rendered it desirable to go much more fully into the Nature

and treatment of Laws of Error. The omissions are mainly

due to a wish to avoid increasing the bulk of this volume

more than is actually necessary, and to a feeling that the

portions treating specially of Inductive Logic (which occupied

some space in the last edition) would be more

suitable to a regular work on that subject. I am at present

engaged on such a work.

The publications which I have had occasion to notice

have mostly appeared in various scientific journals. The

principal authors of these have been Mr F. Galton and

Mr F. Y. Edgeworth: to the latter of whom I am also

personally much obliged for many discussions, oral and

written, and for his kindness in looking through the proof-sheets.

His published articles are too numerous for separate

mention here, but I may say generally, in addition to the

obligations specially noticed, that I have been considerably

indebted to them in writing the last two chapters. Two

authors of works of a somewhat more substantial character,

viz. Prof. Lexis and Von Kries, only came under my notice

unfortunately after this work was already in the printer's

hands. With the latter of these authors I find myself in

closer agreement than with most others, in respect of his

general conception and treatment of Probability.

\_December\_ 1887.

1. I have indicated the new chapters and sections by printing them in

italics in the Table of Contents.

TABLE OF CONTENTS.[1]

1. Chapters and sections which are nearly or entirely new are printed

in \_italics\_.

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22, 23. \_The actual calculations are the same as before,\_

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THE LOGIC OF CHANCE.

CHAPTER I.

\_ON CERTAIN KINDS OF GROUPS OR SERIES AS THE FOUNDATION OF PROBABILITY.\_

1. It is sometimes not easy to give a clear definition of a

science at the outset, so as to set its scope and province before

the reader in a few words. In the case of those sciences

which are more immediately and directly concerned with

what are termed objects, rather than with what are termed

processes, this difficulty is not indeed so serious. If the

reader is already familiar with the objects, a simple reference

to them will give him a tolerably accurate idea of the

direction and nature of his studies. Even if he be not

familiar with them, they will still be often to some extent

connected and associated in his mind by a name, and

the mere utterance of the name may thus convey a fair

amount of preliminary information. This is more or less

the case with many of the natural sciences; we can often

tell the reader beforehand exactly what he is going to study.

But when a science is concerned, not so much with objects

directly, as with processes and laws, or when it takes for the

subject of its enquiry some comparatively obscure feature

drawn from phenomena which have little or nothing else in

common, the difficulty of giving preliminary information

becomes greater. Recognized classes of objects have then

to be disregarded and even broken up, and an entirely novel

arrangement of the objects to be made. In such cases it is

the study of the science that first gives the science its unity,

for till it is studied the objects with which it is concerned

were probably never thought of together. Here a definition

cannot be given at the outset, and the process of obtaining it

may become by comparison somewhat laborious.

The science of Probability, at least on the view taken of

it in the following pages, is of this latter description. The

reader who is at present unacquainted with the science

cannot be at once informed of its scope by a reference to

objects with which he is already familiar. He will have

to be taken in hand, as it were, and some little time

and trouble will have to be expended in directing his

attention to our subject-matter before he can be expected to

know it. To do this will be our first task.

2. In studying Nature, in any form, we are continually

coming into possession of information which we sum up in

general propositions. Now in very many cases these general

propositions are neither more nor less certain and accurate

than the details which they embrace and of which they are

composed. We are assuming at present that the truth of

these generalizations is not disputed; as a matter of fact

they may rest on weak evidence, or they may be uncertain

from their being widely extended by induction; what is

meant is, that when we resolve them into their component

parts we have precisely the same assurance of the truth of

the details as we have of that of the whole. When I know,

for instance, that all cows ruminate, I feel just as certain

that any particular cow or cows ruminate as that the whole

class does. I may be right or wrong in my original statement,

and I may have obtained it by any conceivable mode

in which truths can be obtained; but whatever the value of

the general proposition may be, that of the particulars is

neither greater nor less. The process of inferring the particular

from the general is not accompanied by the slightest

diminution of certainty. If one of these 'immediate inferences'

is justified at all, it will be equally right in every

case.

But it is by no means necessary that this characteristic

should exist in all cases. There is a class of immediate inferences,

almost unrecognized indeed in logic, but constantly

drawn in practice, of which the characteristic is, that as they

increase in particularity they diminish in certainty. Let me

assume that I am told that \_some\_ cows ruminate; I cannot

infer logically from this that any particular cow does so,

though I should feel some way removed from absolute disbelief,

or even indifference to assent, upon the subject; but

if I saw a herd of cows I should feel more sure that some of

them were ruminant than I did of the single cow, and my

assurance would increase with the numbers of the herd about

which I had to form an opinion. Here then we have a class

of things as to the individuals of which we feel quite in

uncertainty, whilst as we embrace larger numbers in our

assertions we attach greater weight to our inferences. It is

with such classes of things and such inferences that the

science of Probability is concerned.

3. In the foregoing remarks, which are intended to

be purely preliminary, we have not been able altogether to

avoid some reference to a subjective element, viz. the degree

of our certainty or belief about the things which we are

supposed to contemplate. The reader may be aware that

by some writers this element is regarded as the subject-matter

of the science. Hence it will have to be discussed

in a future chapter. As however I do not agree with the

opinion of the writers just mentioned, at least as regards

treating this element as one of primary importance, no

further allusion will be made to it here, but we will pass on

at once to a more minute investigation of that distinctive

characteristic of certain classes of things which was introduced

to notice in the last section.

In these classes of things, which are those with which

Probability is concerned, the fundamental conception which

the reader has to fix in his mind as clearly as possible, is, I

take it, that of a series. But it is a series of a peculiar kind,

one of which no better compendious description can be given

than that which is contained in the statement that it combines

individual irregularity with aggregate regularity. This

is a statement which will probably need some explanation.

Let us recur to an example of the kind already alluded to,

selecting one which shall be in accordance with experience.

Some children will not live to thirty. Now if this proposition

is to be regarded as a purely indefinite or, as it would

be termed in logic, 'particular' proposition, no doubt the

notion of a series does not obviously present itself in connection

with it. It contains a statement about a certain

unknown proportion of the whole, and that is all. But it is

not with these purely indefinite propositions that we shall

be concerned. Let us suppose the statement, on the contrary,

to be of a numerical character, and to refer to a given

proportion of the whole, and we shall then find it difficult to

exclude the notion of a series. We shall find it, I think,

impossible to do so as soon as we set before us the aim of

obtaining accurate, or even moderately correct inferences.

What, for instance, is the meaning of the statement that

two new-born children in three fail to attain the age of

sixty-three? It certainly does not declare that in any given

batch of, say, thirty, we shall find just twenty that fail:

whatever might be the strict meaning of the words, this

is not the import of the statement. It rather contemplates

our examination of a large number, of a long succession

of instances, and states that in such a succession we shall

find a numerical proportion, not indeed fixed and accurate at

first, but which tends in the long run to become so. In

every kind of example with which we shall be concerned we

shall find this reference to a large number or succession

of objects, or, as we shall term it, \_series\_ of them.

A few additional examples may serve to make this plain.

Let us suppose that we toss up a penny a great many

times; the results of the successive throws may be conceived

to form a series. The separate throws of this series seem to

occur in utter disorder; it is this disorder which causes our

uncertainty about them. Sometimes head comes, sometimes

tail comes; sometimes there is a repetition of the same face,

sometimes not. So long as we confine our observation to a

few throws at a time, the series seems to be simply chaotic.

But when we consider the result of a long succession we find

a marked distinction; a kind of order begins gradually to

emerge, and at last assumes a distinct and striking aspect.

We find in this case that the heads and tails occur in about

equal numbers, that similar repetitions of different faces do

so also, and so on. In a word, notwithstanding the individual

disorder, an aggregate order begins to prevail. So again if

we are examining the length of human life, the different lives

which fall under our notice compose a series presenting the

same features. The length of a single life is familiarly uncertain,

but the average duration of a batch of lives is becoming

in an almost equal degree familiarly certain. The

larger the number we take out of any mixed crowd, the

clearer become the symptoms of order, the more nearly will

the average length of each selected class be the same.

These few cases will serve as simple examples of a property

of things which can be traced almost everywhere, to a

greater or less extent, throughout the whole field of our experience.

Fires, shipwrecks, yields of harvest, births, marriages,

suicides; it scarcely seems to matter what feature we

single out for observation.[1] The irregularity of the single

instances diminishes when we take a large number, and at

last seems for all practical purposes to disappear.

In speaking of the effect of the average in thus diminishing

the irregularities which present themselves in the details,

the attention of the student must be prominently directed to

the point, that it is not the \_absolute\_ but the \_relative\_ irregularities

which thus tend to diminish without limit. This

idea will be familiar enough to the mathematician, but to

others it may require some reflection in order to grasp it

clearly. The absolute divergences and irregularities, so far

from diminishing, show a disposition to increase, and this (it

may be) without limit, though their relative importance shows

a corresponding disposition to diminish without limit. Thus

in the case of tossing a penny, if we take a few throws, say

ten, it is decidedly unlikely that there should be a difference

of six between the numbers of heads and tails; that is, that

there should be as many as eight heads and therefore as few

as two tails, or \_vice versâ\_. But take a thousand throws, and

it becomes in turn exceedingly likely that there should be

as much as, or more than, a difference of six between the

respective numbers. On the other hand the \_proportion\_ of

heads to tails in the case of the thousand throws will be very

much nearer to unity, in most cases, than when we only took

ten. In other words, the longer a game of chance continues

the larger are the spells and runs of luck in themselves, but

the less their relative proportions to the whole amounts

involved.

4. In speaking as above of events or things as to the

details of which we know little or nothing, it is not of course

implied that our ignorance about them is complete and universal,

or, what comes to the same thing, that irregularity

may be observed in all their qualities. All that is meant is

that there are \_some\_ qualities or marks in them, the existence

of which we are not able to predicate with certainty in the

individuals. With regard to all their other qualities there

may be the utmost uniformity, and consequently the most

complete certainty. The irregularity in the length of human

life is notorious, but no one doubts the existence of such

organs as a heart and brains in any person whom he happens

to meet. And even in the qualities in which the irregularity

is observed, there are often, indeed generally, positive limits

within which it will be found to be confined. No person,

for instance, can calculate what may be the length of any

particular life, but we feel perfectly certain that it will not

stretch out to 150 years. The irregularity of the individual

instances is only shown in certain respects, as e.g. the length

of the life, and even in these respects it has its limits. The

same remark will apply to most of the other examples with

which we shall be concerned. The disorder in fact is not

universal and unlimited, it only prevails in certain directions

and up to certain points.

5. In speaking as above of a series, it will hardly be

necessary to point out that we do not imply that the objects

themselves which compose the series must occur successively

in time; the series may be formed simply by their coming

in succession under our notice, which as a matter of fact

they may do in any order whatever. A register of mortality,

for instance, may be made up of deaths which took place

simultaneously or successively; or, we might if we pleased

arrange the deaths in an order quite distinct from either of

these. This is entirely a matter of indifference; in all these

cases the series, for any purposes which we need take into

account, may be regarded as being of precisely the same description.

The objects, be it remembered, are given to us in

nature; the order under which we view them is our own private

arrangement. This is mentioned here simply by way of

caution, the meaning of this assertion will become more plain

in the sequel.

I am aware that the word 'series' in the application with

which it is used here is liable to some misconstruction, but I

cannot find any better word, or indeed any as suitable in all

respects. As remarked above, the events need not necessarily

have occurred in a regular sequence of time, though

they often will have done so. In many cases (for instance,

the throws of a penny or a die) they really do occur in succession;

in other cases (for instance, the heights of men, or

the duration of their lives), whatever may have been the

order of their actual occurrence, they are commonly brought

under our notice in succession by being arranged in statistical

tables. In all cases alike our processes of inference involve

the necessity of examining one after another of the members

which compose the group, or at least of being prepared to do

this, if we are to be in a position to justify our inferences.

The force of these considerations will come out in the course

of the investigation in Chapter VI.

The late Leslie Ellis[2] has expressed what seems to me

a substantially similar view in terms of genus and species,

instead of speaking of a series. He says, "When individual

cases are considered, we have no conviction that the ratios of

frequency of occurrence depend on the circumstances common

to all the trials. On the contrary, we recognize in the determining

circumstances of their occurrence an extraneous

element, an element, that is, extraneous to the idea of the

genus and species. Contingency and limitation come in (so

to speak) together; and both alike disappear when we consider

the genus in its entirety, or (which is the same thing)

in what may be called an ideal and practically impossible

realization of all which it potentially contains. If this be

granted, it seems to follow that the fundamental principle

of the Theory of Probabilities may be regarded as included

in the following statement,--The conception of a genus

implies that of numerical relations among the species subordinated

to it." As remarked above, this appears a substantially

similar doctrine to that explained in this chapter,

but I do not think that the terms genus and species are by

any means so well fitted to bring out the conception of a

tendency or limit as when we speak of a series, and I therefore

much prefer the latter expression.

6. The reader will now have in his mind the conception

of a series or group of things or events, about the individuals

of which we know but little, at least in certain respects,

whilst we find a continually increasing uniformity as we

take larger numbers under our notice. This is definite

enough to point out tolerably clearly the kind of things

with which we have to deal, but it is not sufficiently definite

for purposes of accurate thought. We must therefore attempt

a somewhat closer analysis.

There are certain phrases so commonly adopted as to

have become part of the technical vocabulary of the subject,

such as an 'event' and the 'way in which it can

happen.' Thus the act of throwing a penny would be called

an event, and the fact of its giving head or tail would be

called the way in which the event happened. If we were

discussing tables of mortality, the former term would denote

the mere fact of death, the latter the age at which

it occurred, or the way in which it was brought about,

or whatever else in it might be the particular circumstance

under discussion. This phraseology is very convenient, and

will often be made use of in this work, but without explanation

it may lead to confusion. For in many cases the

way in which the event happens is of such great relative

importance, that according as it happens in one way or

another the event would have a different name; in other

words, it would not in the two cases be nominally the same

event. The phrase therefore will have to be considerably

stretched before it will conveniently cover all the cases to

which we may have to apply it. If for instance we were

contemplating a series of human beings, male and female,

it would sound odd to call their humanity an event, and

their sex the way in which the event happened.

If we recur however to any of the classes of objects

already referred to, we may see our path towards obtaining

a more accurate conception of what we want. It will easily

be seen that in every one of them there is a mixture of

similarity and dissimilarity; there is a series of events

which have a certain number of features or attributes in

common,--without this they would not be classed together.

But there is also a distinction existing amongst them; a

certain number of other attributes are to be found in some

and are not to be found in others. In other words, the

individuals which form the series are compound, each being

made up of a collection of things or attributes; some of

these things exist in all the members of the series, others

are found in some only. So far there is nothing peculiar

to the science of Probability; that in which the distinctive

characteristic consists is this;--that the occasional attributes,

as distinguished from the permanent, are found on

an extended examination to tend to exist \_in a certain definite

proportion of the whole number of cases\_. We cannot tell in

any given instance whether they will be found or not, but

as we go on examining more cases we find a growing uniformity.

We find that the proportion of instances in which

they are found to instances in which they are wanting, is

gradually subject to less and less comparative variation, and

approaches continually towards some apparently fixed value.

The above is the most comprehensive form of description;

as a matter of fact the groups will in many cases take a

far simpler form; they may appear, e.g. simply as a succession

of things of the same kind, say human beings, with

or without an occasional attribute, say that of being left-handed.

We are using the word attribute, of course, in its

widest sense, intending it to include every distinctive feature

that can be observed in a thing, from essential qualities

down to the merest accidents of time and place.

7. On examining our series, therefore, we shall find

that it may best be conceived, not necessarily as a succession

of events happening in different ways, but as a succession

of groups of things. These groups, on being analysed, are

found in every case to be resolvable into collections of substances

and attributes. That which gives its unity to the

succession of groups is the fact of some of these substances or

attributes being common to the whole succession; that which

gives their distinction to the groups in the succession is the

fact of some of them containing only a portion of these substances

and attributes, the other portion or portions being

occasionally absent. So understood, our phraseology may

be made to embrace every class of things of which Probability

can take account.

8. It will be easily seen that the ordinary expression

(viz. the 'event,' and the 'way in which it happens') may be

included in the above. When the occasional attributes are

unimportant the permanent ones are sufficient to fix and

appropriate the name, the presence or absence of the others

being simply denoted by some modification of the name or

the addition of some predicate. We may therefore in all such

cases speak of the collection of attributes as 'the event,'--the

same event essentially, that is--only saying that \_it\_ (so as

to preserve its nominal identity) happens in different ways

in the different cases. When the occasional attributes however

are important, or compose the majority, this way of

speaking becomes less appropriate; language is somewhat

strained by our implying that two extremely different assemblages

are in reality the same event, with a difference only

in its mode of happening. The phrase is however a very

convenient one, and with this caution against its being misunderstood,

it will frequently be made use of here.

9. A series of the above-mentioned kind is, I apprehend,

the ultimate basis upon which all the rules of

Probability must be based. It is essential to a clear comprehension

of the subject to have carried our analysis up

to this point, but any attempt at further analysis into the

intimate nature of the events composing the series, is not

required. It is altogether unnecessary, for instance, to form

any opinion upon the questions discussed in metaphysics as

to the independent existence of substances. We have discovered,

on examination, a series composed of groups of

substances and attributes, or of attributes alone. At such

a series we stop, and thence investigate our rules of inference;

into what these substances or attributes would themselves

be ultimately analysed, if taken in hand by the

psychologist or metaphysician, it is no business of ours to

enquire here.

10. The stage then which we have now reached is

that of having discovered a quantity of things (they prove

on analysis to be groups of things) which are capable of

being classified together, and are best regarded as constituting

a series. The distinctive peculiarity of this series is

our finding in it an order, gradually emerging out of disorder,

and showing in time a marked and unmistakeable uniformity.

The impression which may possibly be derived from the

description of such a series, and which the reader will probably

already entertain if he have studied Probability before,

is that the gradual evolution of this order is indefinite, and

its approach therefore to perfection unlimited. And many of

the examples commonly selected certainly tend to confirm

such an impression. But in reference to the theory of the

subject it is, I am convinced, an error, and one liable to lead

to much confusion.

The lines which have been prefixed as a motto to this

work, "So careful of the type she seems, so careless of the

single life," are soon after corrected by the assertion that

the type itself, if we regard it for a long time, changes,

and then vanishes and is succeeded by others. So in Probability;

that uniformity which is found in the long run,

and which presents so great a contrast to the individual

disorder, though durable is not everlasting. Keep on watching

it long enough, and it will be found almost invariably to

fluctuate, and in time may prove as utterly irreducible to

rule, and therefore as incapable of prediction, as the individual

cases themselves. The full bearing of this fact upon

the theory of the subject, and upon certain common modes

of calculation connected with it, will appear more fully in

some of the following chapters; at present we will confine

ourselves to very briefly establishing and illustrating it.

Let us take, for example, the average duration of life.

This, provided our data are sufficiently extensive, is known to

be tolerably regular and uniform. This fact has been already

indicated in the preceding sections, and is a truth indeed

of which the popular mind has a tolerably clear grasp at the

present day. But a very little consideration will show that

there may be a superior as well as an inferior limit to

the extent within which this uniformity can be observed;

in other words whilst we may fall into error by taking too

few instances we may also fail in our aim, though in a very

different way and from quite different reasons, by taking too

many. At the present time the average duration of life in

England may be, say, forty years; but a century ago it was

decidedly less; several centuries ago it was presumably very

much less; whilst if we possessed statistics referring to a still

earlier population of the country we should probably find that

there has been since that time a still more marked improvement.

What may be the future tendency no man can say for

certain. It may be, and we hope that it will be the case,

that owing to sanitary and other improvements, the duration

of life will go on increasing steadily; it is at least conceivable,

though doubtless incredible, that it should do so without

limit. On the other hand, and with much more likelihood,

this duration might gradually tend towards some fixed

length. Or, again, it is perfectly possible that future generations

might prefer a short and a merry life, and therefore

reduce their average longevity. The duration of life cannot

but depend to some extent upon the general tastes, habits

and employments of the people, that is upon the ideal which

they consciously or unconsciously set before them, and he would

be a rash man who should undertake to predict what this ideal

will be some centuries hence. All that it is here necessary

however to indicate is, that this particular uniformity (as we

have hitherto called it, in order to mark its relative character)

has varied, and, under the influence of future eddies

in opinion and practice, may vary still; and this to any

extent, and with any degree of irregularity. To borrow a

term from Astronomy, we find our uniformity subject to what

might be called an irregular \_secular\_ variation.

11. The above is a fair typical instance. If we had

taken a less simple feature than the length of life, or one

less closely connected with what may be called by comparison

the great permanent uniformities of nature, we should

have found the peculiarity under notice exhibited in a far

more striking degree. The deaths from small-pox, for example,

or the instances of duelling or accusations of witchcraft,

if examined during a few successive decades, might

have shown a very tolerable degree of uniformity. But these

uniformities have risen possibly from zero; after various and

very great fluctuations seem tending towards zero again, at

least in this century; and may, for anything we know, undergo

still more rapid fluctuations in future. Now these

examples must be regarded as being only extreme ones, and

not such very extreme ones, of what is the almost universal

rule in nature. I shall endeavour to show that even the few

apparent exceptions, such as the proportions between male

and female births, &c., may not be, and probably in reality

are not, strictly speaking, exceptions. A type, that is, which

shall be in the fullest sense of the words, persistent and

invariable is scarcely to be found in nature. The full import

of this conclusion will be seen in future chapters. Attention

is only directed here to the important inference that, although

statistics are notoriously of no value unless they are in sufficient

numbers, yet it does not follow but that in certain cases

we may have too many of them. If they are made too extensive,

they may again fall short, at least for any particular

time or place, of their greatest attainable accuracy.

12. These natural uniformities then are found at

length to be subject to fluctuation. Now contrast with them

any of the uniformities afforded by games of chance; these

latter seem to show no trace of secular fluctuation, however

long we may continue our examination of them. Criticisms

will be offered, in the course of the following chapters, upon

some of the common attempts to prove \_à priori\_ that there

must be this fixity in the uniformity in question, but of its

existence there can scarcely be much doubt. Pence give

heads and tails about equally often now, as they did when

they were first tossed, and as we believe they will continue

to do, so long as the present order of things continues. The

fixity of these uniformities may not be as absolute as is

commonly supposed, but no amount of experience which we

need take into account is likely in any appreciable degree to

interfere with them. Hence the obvious contrast, that,

whereas natural uniformities at length fluctuate, those afforded

by games of chance seem fixed for ever.

13. Here then are series apparently of two different

kinds. They are alike in their initial irregularity, alike in

their subsequent regularity; it is in what we may term

their ultimate form that they begin to diverge from each

other. The one tends without any irregular variation

towards a fixed numerical proportion in its uniformity; in

the other the uniformity is found at last to fluctuate, and to

fluctuate, it may be, in a manner utterly irreducible to rule.

As this chapter is intended to be little more than explanatory

and illustrative of the foundations of the science,

the remark may be made here (for which subsequent justification

will be offered) that it is in the case of series of the

former kind only that we are able to make anything which

can be interpreted into strict scientific inferences. We shall

be able however in a general way to see the kind and extent

of error that would be committed if, in any example, we were

to substitute an imaginary series of the former kind for any

actual series of the latter kind which experience may present

to us. The two series are of course to be as alike as possible

in all respects, except that the variable uniformity has been

replaced by a fixed one. The difference then between them

would not appear in the initial stage, for in that stage the

distinctive characteristics of the series of Probability are not

apparent; all is there irregularity, and it would be as impossible

to show that they were alike as that they were

different; we can only say generally that each shows the

same kind of irregularity. Nor would it appear in the next

subsequent stage, for the real variability of the uniformity

has not for some time scope to make itself perceived. It

would only be in what we have called the ultimate stage,

when we suppose the series to extend for a very long time,

that the difference would begin to make itself felt.[3] The

proportion of persons, for example, who die each year at the

age of six months is, when the numbers examined are on a

small scale, utterly irregular; it becomes however regular

when the numbers examined are on a larger scale; but if we

continued our observation for a very great length of time, or

over a very great extent of country, we should find this

regularity itself changing in an irregular way. The substitution

just mentioned is really equivalent to saying, Let

us assume that the regularity is fixed and permanent. It is

making a hypothesis which may not be altogether consistent

with fact, but which is forced upon us for the purpose of

securing precision of statement and definition.

14. The full meaning and bearing of such a substitution

will only become apparent in some of the subsequent

chapters, but it may be pointed out at once that it is in this

way only that we can with perfect strictness introduce the

notion of a 'limit' into our account of the matter, at any

rate in reference to many of the applications of the subject

to purely statistical enquiries. We say that a certain proportion

begins to prevail among the events in the long run;

but then on looking closer at the facts we find that we have

to express ourselves hypothetically, and to say that if present

circumstances remain as they are, the long run will show its

characteristics without disturbance. When, as is often the

case, we know nothing accurately of the circumstances by

which the succession of events is brought about, but have

strong reasons to suspect that these circumstances are likely

to undergo some change, there is really nothing else to be

done. We can only introduce the conception of a limit, towards

which the numbers are tending, by assuming that

these circumstances do not change; in other words, by substituting

a series with a fixed uniformity for the actual one

with the varying uniformity.[4]

15. If the reader will study the following example,

one well known to mathematicians under the name of the

Petersburg[5] problem, he will find that it serves to illustrate

several of the considerations mentioned in this chapter. It

serves especially to bring out the facts that the series with

which we are concerned must be regarded as indefinitely

extensive in point of number or duration; and that when so

regarded certain series, but certain series only (the one in

question being a case in point), take advantage of the indefinite

range to keep on producing individuals in it whose

deviation from the previous average has no finite limit

whatever. When rightly viewed it is a very simple problem,

but it has given rise, at one time or another, to a good

deal of confusion and perplexity.

The problem may be stated thus:--a penny is tossed up;

if it gives head I receive one pound; if heads twice running

two pounds; if heads three times running four pounds, and

so on; the amount to be received doubling every time that

a fresh head succeeds. That is, I am to go on as long as it

continues to give a succession of heads, to regard this succession

as a 'turn' or set, and then take another turn, and so

on; and for each such turn I am to receive a payment; the

occurrence of tail being understood to yield nothing, in fact

being omitted from our consideration. However many times

head may be given in succession, the number of pounds I

may claim is found by raising two to a power one less

than that number of times. Here then is a series formed by

a succession of throws. We will assume,--what many persons

will consider to admit of demonstration, and what

certainly experience confirms within considerable limits,--that

the rarity of these 'runs' of the same face is in direct

proportion to the amount I receive for them when they do

occur. In other words, if we regard only the occasions on

which I receive payments, we shall find that every other

time I get one pound, once in four times I get two pounds,

once in eight times four pounds, and so on without any end.

The question is then asked, what ought I to pay for this

privilege? At the risk of a slight anticipation of the results

of a subsequent chapter, we may assume that this is equivalent

to asking, what amount paid each time would on the

average leave me neither winner nor loser? In other words,

what is the average amount I should receive on the above

terms? Theory pronounces that I ought to give an \_infinite\_

sum: that is, no finite sum, however great, would be an

adequate equivalent. And this is really quite intelligible.

There is a series of indefinite length before me, and the

longer I continue to work it the richer are my returns, and

this without any limit whatever. It is true that the very

rich hauls are extremely rare, but still they do come, and

when they come they make it up by their greater richness.

On every occasion on which people have devoted themselves

to the pursuit in question, they made acquaintance, of course,

with but a limited portion of this series; but the series on

which we base our calculation is unlimited; and the inferences

usually drawn as to the sum which ought in the long

run to be paid for the privilege in question are in perfect

accordance with this supposition.

The common form of objection is given in the reply, that

so far from paying an infinite sum, no sensible man would

give anything approaching to £50 for such a chance. Probably

not, because no man would see enough of the series to

make it worth his while. What most persons form their

practical opinion upon, is such small portions of the series

as they have actually seen or can reasonably expect. Now

in any such portion, say one which embraces 100 turns, the

longest succession of heads would not amount on the average

to more than seven or eight. This is observed, but it is forgotten

that the formula which produced these, would, if it

had greater scope, keep on producing better and better ones

without any limit. Hence it arises that some persons are

perplexed, because the conduct they would adopt, in reference

to the curtailed portion of the series which they are practically

likely to meet with, does not find its justification in inferences

which are necessarily based upon the series in the completeness

of its infinitude.

16. This will be more clearly seen by considering the

various possibilities, and the scope required in order to exhaust

them, when we confine ourselves to a \_limited\_ number of

throws. Begin with three. This yields eight equally likely

possibilities. In four of these cases the thrower starts with

tail and therefore loses: in two he gains a single point

(i.e. £1); in one he gains two points, and in one he gains four

points. Hence his total gain being eight pounds achieved in

four different contingencies, his average gain would be two

pounds.

Now suppose he be allowed to go as far as n throws, so

that we have to contemplate 2^{n} possibilities. All of these

have to be taken into account if we wish to consider what

happens on the average. It will readily be seen that, when

all the possible cases have been reckoned once, his total gain

will be (reckoned in pounds),

2^{n-2} + 2^{n-3}·2 + 2^{n-4}·2^{2} + ... + 2·2^{n-3} + 2^{n-2} + 2^{n-1},

viz.

(n + 1) 2^{n-2}.

This being spread over 2^{n-1} different occasions of gain his

average gain will be 1/2 (n + 1).

Now when we are referring to averages it must be remembered

that the minimum number of different occurrences

necessary in order to justify the average is that which enables

each of them to present itself once. A man proposes to stop

short at a succession of ten heads. Well and good. We tell

him that his average gain will be £5. 10\_s\_. 0\_d\_.: but we also

impress upon him that in order to justify this statement he

must commence to toss at least 1024 times, for in no less

number can all the contingencies of gain and loss be exhibited

and balanced. If he proposes to reach an average gain of £20,

he will require to be prepared to go up to 39 throws,

To \_justify\_ this payment he must commence to throw 2^{39} times,

i.e. about a million million times. Not before he has

accomplished this will he be in a position to prove to any

sceptic that this is the true average value of a 'turn' extending

to 39 successive tosses.

Of course if he elects to toss to all eternity we must

adopt the line of explanation which alone is possible where

questions of infinity in respect of number and magnitude are

involved. We cannot tell him to pay down 'an infinite sum,'

for this has no strict meaning. But we tell him that, however

much he may consent to pay each time runs of heads occur,

he will attain at last a stage in which he will have won

back his total payments by his total receipts. However

large n may be, if he perseveres in trying 2^{n} times he \_may\_

have a true average receipt of 1/2 (n + 1) pounds, and if he

continues long enough onwards he \_will\_ have it.

The problem will recur for consideration in a future

chapter.

1. The following statistics will give a fair idea of the wide range of

experience over which such regularity is found to exist: "As

illustrations of equal amounts of fluctuation from totally

dissimilar causes, take the deaths in the West district of London in

seven years (fluctuation 13.66), and offences against the person

(fluctuation 13.61); or deaths from apoplexy (fluctuation 5.54), and

offences against property, without violence (fluctuation 5.48); or

students registered at the College of Surgeons (fluctuation 1.85),

and the number of pounds of manufactured tobacco taken for home

consumption (fluctuation 1.89); or out-door paupers (fluctuation

3.45) and tonnage of British vessels entered in ballast (fluctuation

3.43), &c." [Extracted from a paper in the Journal of the

Statistical Society, by Mr Guy, March, 1858; the 'fluctuation' here

given is a measure of the amount of irregularity, that is of

departure from the average, estimated in a way which will be

described hereafter.]

2. Transactions of the Cambridge Philosophical Society, Vol. IX. p. 605.

Reprinted in the collected edition of his writings, p. 50.

3. We might express it thus:--a few instances are not sufficient to

display a law at all; a considerable number will suffice to display

it; but it takes a very great number to establish that a \_change\_ is

taking place in the law.

4. The mathematician may illustrate the nature of this substitution by

the analogies of the 'circle of curvature' in geometry, and the

'instantaneous ellipse' in astronomy. In the cases in which these

conceptions are made use of we have a phenomenon which is

continuously varying and also changing its rate of variation. We

take it at some given moment, suppose its rate at that moment to be

fixed, and then complete its career on that supposition.

5. So called from its first mathematical treatment appearing in the

\_Commentarii\_ of the Petersburg Academy; a variety of notices upon

it will be found in Mr Todhunter's History of the Theory of

Probability.

CHAPTER II.

\_FURTHER DISCUSSION UPON THE NATURE OF THE SERIES MENTIONED IN THE LAST

CHAPTER.\_

1. In the course of the last chapter the nature of a particular

kind of series, that namely, which must be considered

to constitute the basis of the science of Probability, has received

a sufficiently general explanation for the preliminary

purpose of introduction. One might indeed say more than

this; for the characteristics which were there pointed out are

really sufficient in themselves to give a fair general idea of

the nature of Probability, and of the sort of problems with

which it deals. But in the concluding paragraphs an indication

was given that the series of this kind, as they actually

occur in nature or as the results of more or less artificial

production, are seldom or never found to occur in such a

simple form as might possibly be expected from what had

previously been said; but that they are almost always seen

to be associated together in groups after a somewhat complicated

fashion. A fuller discussion of this topic must now

be undertaken.

We will take for examination an instance of a kind with

which the investigations of Quetelet will have served to

familiarize some readers. Suppose that we measure the

heights of a great many adult men in any town or country.

These heights will of course lie between certain extremes in

each direction, and if we continue to accumulate our measures

it will be found that they tend to lie continuously

between these extremes; that is to say, that under those

circumstances no intermediate height will be found to be

permanently unrepresented in such a collection of measurements.

Now suppose these heights to be marshalled in the

order of their magnitude. What we always find is something

of the following kind;--about the middle point between

the extremes, a large number of the results will be

found crowded together: a little on each side of this point

there will still be an excess, but not to so great an extent;

and so on, in some diminishing scale of proportion, until as

we get towards the extreme results the numbers thin off and

become relatively exceedingly small.

The point to which attention is here directed is not the

mere fact that the numbers thus tend to diminish from the

middle in each direction, but, as will be more fully explained

directly, the \_law\_ according to which this progressive diminution

takes place. The word 'law' is here used in its mathematical

sense, to express the formula connecting together the

two elements in question, namely, the height itself, and the

relative number that are found of that height. We shall

have to enquire whether one of these elements is a function

of the other, and, if so, what function.

2. After what was said in the last chapter, it need

hardly be insisted upon that the interest and significance of

such investigations as these are almost entirely dependent

upon the statistics being very extensive. In one or other of

Quetelet's works on Social Physics[1] will be found a selection

of measurements of almost every element which the physical

frame of man can furnish:--his height, his weight, the muscular

power of various limbs, the dimensions of almost every

part and organ, and so on. Some of the most extensive of

these express the heights of 25,000 Federal soldiers from the

Army of the Potomac, and the circumferences of the chests

of 5738 Scotch militia men taken many years ago. Those

who wish to consult a large repertory of such statistics cannot

be referred to any better sources than to these and other

works by the same author.[2]

Interesting and valuable, however, as are Quetelet's statistical

investigations (and much of the importance now

deservedly attached to such enquiries is, perhaps, owing

more to his efforts than to those of any other person), I cannot

but feel convinced that there is much in what he has

written upon the subject which is erroneous and confusing as

regards the foundations of the science of Probability, and the

philosophical questions which it involves. These errors are

not by any means confined to him, but for various reasons

they will be better discussed in the form of a criticism of his

explicit or implicit expression of them, than in any more independent

way.

3. In the first place then, he always, or almost always,

assumes that there can be but one and the same law of arrangement

for the results of our observations, measurements,

and so on, in these statistical enquiries. That is, he assumes

that whenever we get a group of such magnitudes

clustering about a mean, and growing less frequent as

they depart from that mean, we shall find that this diminution

of frequency takes place according to one invariable

law, whatever may be the nature of these magnitudes,

and whatever the process by which they may have

been obtained.

That such a uniformity as this should prevail amongst

many and various classes of phenomena would probably seem

surprising in any case. But the full significance of such a

fact as this (if indeed it were a fact) only becomes apparent

when attention is directed to the profound distinctions in the

nature and origin of the phenomena which are thus supposed

to be harmonized by being brought under one comprehensive

principle. This will be better appreciated if we take a brief

glance at some of the principal classes into which the things

with which Probability is chiefly concerned may be divided.

These are of a three-fold kind.

4. In the first place there are the various combinations,

and runs of luck, afforded by games of chance. Suppose

a handful, consisting of ten coins, were tossed up a

great many times in succession, and the results were tabulated.

What we should obtain would be something of the

following kind. In a certain proportion of cases, and these

the most numerous of all, we should find that we got five

heads and five tails; in a somewhat less proportion of cases

we should have, as equally frequent results, four heads six

tails, and four tails six heads; and so on in a continually

diminishing proportion until at length we came down, in a

very small relative number of cases, to nine heads one tail,

and nine tails one head; whilst the least frequent results

possible would be those which gave all heads or all tails.[3]

Here the statistical elements under consideration are, as

regards their origin at any rate, optional or brought about

by human choice. They would, therefore, be commonly

described as being mainly artificial, but their results ultimately

altogether a matter of chance.

Again, in the second place, we might take the accurate

measurements--i.e. the actual magnitudes themselves,--of

a great many natural objects, belonging to the same genus

or class; such as the cases, already referred to, of the heights,

or other characteristics of the inhabitants of any district.

Here human volition or intervention of any kind seem to

have little or nothing to do with the matter. It is optional

with us to \_collect\_ the measures, but the things measured are

quite outside our control. They would therefore be commonly

described as being altogether the production of nature,

and it would not be supposed that in strictness chance had

anything whatever to do with the matter.

In the third place, the result at which we are aiming

may be some fixed magnitude, one and the same in each

of our successive attempts, so that if our measurements

were rigidly accurate we should merely obtain the same

result repeated over and over again. But since all our

methods of attaining our aims are practically subject to

innumerable imperfections, the results actually obtained

will depart more or less, in almost every case, from the

real and fixed value which we are trying to secure. They

will be sometimes more wide of the mark, sometimes less

so, the worse attempts being of course the less frequent.

If a man aims at a target he will seldom or never hit it

precisely in the centre, but his good shots will be more[4]

numerous than his bad ones. Here again, then, we have

a series of magnitudes (i.e. the deflections of the shots from

the point aimed at) clustering about a mean, but produced

in a very different way from those of the last two cases.

In this instance the elements would be commonly regarded

as only partially the results of human volition, and chance

therefore as being only a co-agent in the effects produced.

With these must be classed what may be called \_estimates\_,

as distinguished from measurements. By the latter are

generally understood the results of a certain amount of

mechanism or manipulation; by the former we may understand

those cases in which the magnitude in question is

determined by direct observation or introspection. The

interest and importance of this class, so far as scientific

principles are concerned, dates mainly from the investigations

of Fechner. Its chief field is naturally to be found amongst

psychological data.

Other classes of things, besides those alluded to above,

might readily be given. These however are the classes about

which the most extensive statistics are obtainable, or to

which the most practical importance and interest are attached.

The profound distinctions which separate their

origin and character are obvious. If they all really did

display precisely the same law of variation it would be a

most remarkable fact, pointing doubtless to some deep-seated

identity underlying the various ways, apparently

so widely distinct, in which they had been brought about.

The questions now to be discussed are: Is it the case,

with any considerable degree of rigour, that only one law

of distribution does really prevail? and, in so far as this

is so, how does it come to pass?

5. In support of an affirmative answer to the former

of these two questions, several different kinds of proof are,

or might be, offered.

(I.) For one plan we may make a direct appeal to

experience, by collecting sets of statistics and observing

what is their law of distribution. As remarked above, this

has been done in a great variety of cases, and in some

instances to a very considerable extent, by Quetelet and

others. His researches have made it abundantly convincing

that many classes of things and processes, differing widely

in their nature and origin, do nevertheless appear to conform

with a considerable degree of accuracy to one and the

same[5] law. At least this is made plain for the more

central values, for those that is which are situated most

nearly about the mean. With regard to the extreme values

there is, on the other hand, some difficulty. For instance

in the arrangements of the heights of a number of men,

these extremes are rather a stumbling-block; indeed it has

been proposed to reject them from both ends of the scale

on the plea that they are monstrosities, the fact being that

their relative numbers do not seem to be by any means

those which theory would assign.[6] Such a plan of rejection

is however quite unauthorized, for these dwarfs and giants

are born into the world like their more normally sized

brethren, and have precisely as much right as any others

to be included in the formulæ we draw up.

Besides the instance of the heights of men, other classes

of observations of a somewhat similar character have been

already referred to as collected and arranged by Quetelet.

From the nature of the case, however, there are not many

appropriate ones at hand; for when our object is, not to

illustrate a law which can be otherwise proved, but to

obtain actual direct proof of it, the collection of observations

and measurements ought to be made upon such a large

scale as to deter any but the most persevering computers

from undergoing the requisite labour. Some of the remarks

made in the course of the note on the opposite page will

serve to illustrate the difficulties which would lie in the way

of such a mode of proof.

We are speaking here, it must be understood, only of

\_symmetrical\_ curves: if there is asymmetry, i.e. if the Law of

Error is different on different sides of the mean,--a comparatively

very small number of observations would suffice

to detect the fact. But, granted symmetry and rapid

decrease of frequency on each side of the mean, we could

generally select some one species of the exponential curve

which should pretty closely represent our statistics in the

neighbourhood of the mean. That is, where the statistics are

numerous we could secure agreement; and where we could

not secure agreement the statistics would be comparatively

so scarce that we should have to continue the observations

for a very long time in order to prove the disagreement.

6. Allowing the various statistics such credit as they

deserve, for their extent, appropriateness, accuracy and so

on, the general conclusion which will on the whole be drawn

by almost every one who takes the trouble to consult them,

is that they do, in large part, conform approximately to one

type or law, at any rate for all except the extreme values.

So much as this must be fully admitted. But that they do

not, indeed we may say that they cannot, always do so in

the case of the extreme values, will become obvious on

a little consideration. In some of the classes of things to

which the law is supposed to apply, for example, the successions

of heads and tails in the throws of a penny, there is

no limit to the magnitude of the fluctuations which may and

will occur. Postulate as long a succession of heads or of tails

as we please, and if we could only live and toss long enough

for it we should succeed in getting it at length. In other

cases, including many of the applications of Probability

to natural phenomena, there can hardly fail to be such

limits. Deviations exceeding a certain range may not be

merely improbable, that is of very rare occurrence, but they

may often from the nature of the case be actually impossible.

And even when they are not actually impossible it

may frequently appear on examination that they are only

rendered possible by the occasional introduction of agencies

which are not supposed to be available in the production

of the more ordinary or intermediate values. When, for

instance, we are making observations with any kind of

instrument, the nature of its construction may put an

absolute limit upon the possible amount of error. And even

if there be not an absolute limit under all kinds of usage

it may nevertheless be the case that there is one under

fair and proper usage; it being the case that only when

the instrument is designedly or carelessly tampered with will

any new causes of divergence be introduced which were not

confined within the old limits.

Suppose, for instance, that a man is firing at a mark.

His worst shots must be supposed to be brought about by

a combination of such causes as were acting, or prepared

to act, in every other case; the extreme instance of what

we may thus term 'fair usage' being when a number of

distinct causes have happened to conspire together so as

to tend in the same direction, instead of, as in the other

cases, more or less neutralizing one another's work. But

the aggregate effect of such causes may well be supposed

to be limited. The man will not discharge his shot nearly

at right angles to the true line of fire unless some entirely

new cause comes in, as by some unusual circumstance

having distracted his attention, or by his having had some

spasmodic seizure. But influences of this kind were not

supposed to have been available before; and even if they

were we are taking a bold step in assuming that these

occasional great disturbances are subject to the same kind

of laws as are the aggregates of innumerable little ones.

We cannot indeed lay much stress upon an example

of this last kind, as compared with those in which we

can see for certain that there is a fixed limit to the range

of error. It is therefore offered rather for illustration than

for proof. The enormous, in fact inconceivable magnitude

of the numbers expressive of the chance of very rare combinations,

such as those in question, has such a bewildering

effect upon the mind that one may be sometimes apt to confound

the impossible with the higher degrees of the merely

mathematically improbable.

7. At the time the first edition of this essay was composed

writers on Statistics were, I think, still for the most

part under the influence of Quetelet, and inclined to overvalue

his authority on this particular subject: of late however

attention has been repeatedly drawn to the necessity of

taking account of other laws of arrangement than the binomial

or exponential.

Mr Galton, for instance,--to whom every branch of the

theory of statistics owes so much,--has insisted[7] that the

"assumption which lies at the basis of the well-known law of

'Frequency of Error'... is incorrect in many groups of vital

and social phenomena.... For example, suppose we endeavour

to match a tint; Fechner's law, in its approximative and

simplest form of sensation = log stimulus, tells us that a

series of tints, in which the quantities of white scattered on a

black ground are as 1, 2, 4, 8, 16, 32, &c., will appear to the

eye to be separated by equal intervals of tint. Therefore, in

matching a grey that contains 8 portions of white, we are

just as likely to err by selecting one that has 16 portions as

one that has 4 portions. In the first case there would be an

error in excess, of 8; in the second there would be an error,

in deficiency, of 4. Therefore, an error of the same magnitude

in excess or in deficiency is not equally probable." The consequences

of this assumption are worked out in a remarkable

paper by Dr D. McAlister, to which allusion will have to be

made again hereafter. All that concerns us here to point out

is that when the results of statistics of this character are

arranged graphically we do \_not\_ get a curve which is symmetrical

on both sides of a central axis.

8. More recently, Mr F. Y. Edgeworth (in a report of

a Committee of the British Association appointed to enquire

into the variation of the monetary standard) has urged the

same considerations in respect of prices of commodities. He

gives a number of statistics "drawn from the prices of twelve

commodities during the two periods 1782-1820, 1820-1865.

The maximum and minimum entry for each series having

been noted, it is found that the number of entries above the

'middle point,' half-way between the maximum and minimum,[8]

is in every instance less than half the total number of entries

in the series. In the twenty-four trials there is not a single

exception to the rule, and in very few cases even an approach

to an exception. We may presume then that the curves are

of the lop-sided character indicated by the accompanying

diagram." The same facts are also ascertained in respect to

\_place\_ variations as distinguished from time variations. To

these may be added some statistics of my own, referring to

the heights of the barometer taken at the same hour on more

than 4000 successive days (v. \_Nature\_, Sept. 2, 1887). So far

as these go they show a marked asymmetry of arrangement.

In fact it appears to me that this want of symmetry

ought to be looked for in all cases in which the phenomena

under measurement are of a 'one-sided' character; in the

sense that they are measured on one side only of a certain

fixed point from which their possibility is supposed to start.

For not only is it impossible for them to fall below this point:

long before they reach it the influence of its proximity is felt

in enhancing the difficulty and importance of the same

amount of absolute difference.

Look at a table of statures, for instance, with a mean

value of 69 inches. A diminution of three feet (were this

possible) is much more influential,--counts for much more,

in every sense of the term,--than an addition of the same

amount; for the former does not double the mean, while the

latter more than halves it. Revert to an illustration. If

a vast number of petty influencing circumstances of the kind

already described were to act upon a swinging \_pendulum\_ we

should expect the deflections in each direction to display

symmetry; but if they were to act upon a \_spring\_ we should

not expect such a result. Any phenomena of which the

latter is the more appropriate illustration can hardly be

expected to range themselves with symmetry about a mean.[9]

9. (II.) The last remarks will suggest another kind of

proof which might be offered to establish the invariable nature

of the law of error. It is of a direct deductive kind, not

appealing immediately to statistics, but involving an enquiry

into the actual or assumed nature of the causes by which the

events are brought about. Imagine that the event under

consideration is brought to pass, in the first place, by some

fixed cause, or group of fixed causes. If this comprised all

the influencing circumstances the event would invariably

happen in precisely the same way: there would be no errors

or deflections whatever to be taken account of. But now

suppose that there were also an enormous number of very

small causes which tended to produce deflections; that these

causes acted in entire independence of one another; and that

each of the lot told as often, in the long run, in one direction

as in the opposite. It is easy[10] to see, in a general way, what

would follow from these assumptions. In a very few cases

nearly all the causes would tell in the same direction; in

other words, in a very few cases the deflection would be

extreme. In a greater number of cases, however, it would

only be the most part of them that would tell in one direction,

whilst a few did what they could to counteract the rest;

the result being a comparatively larger number of somewhat

smaller deflections. So on, in increasing numbers, till we

approach the middle point. Here we shall have a very large

number of very small deflections: the cases in which the

opposed influences just succeed in balancing one another,

so that no error whatever is produced, being, though actually

infrequent, relatively the most frequent of all.

Now if all deflections from a mean were brought about in

the way just indicated (an indication which must suffice for

the present) we should always have one and the same law of

arrangement of frequency for these deflections or errors, viz.

the exponential[11] law mentioned in §5.

10. It may be readily admitted from what we know

about the production of events that something resembling

these assumptions, and therefore something resembling the

consequences which follow from them, is really secured in a

very great number of cases. But although this may prevail

approximately, it is in the highest degree improbable that it

could ever be secured, even artificially, with anything approaching

to rigid accuracy. For one thing, the causes of

deflection will seldom or never be really independent of one

another. Some of them will generally be of a kind such that

the supposition that several are swaying in one direction,

may affect the capacity of each to produce that full effect

which it would have been capable of if it had been left to do

its work alone. In the common example, for instance, of

firing at a mark, so long as we consider the case of the tolerably

good shots the effect of the wind (one of the causes of

error) will be approximately the same whatever may be the

precise direction of the bullet. But when a shot is considerably

wide of the mark the wind can no longer be regarded as

acting at right angles to the line of flight, and its effect in

consequence will not be precisely the same as before. In

other words, the causes here are not strictly independent, as

they were assumed to be; and consequently the results to be

attributed to each are not absolutely uninfluenced by those

of the others. Doubtless the effect is trifling here, but I

apprehend that if we were carefully to scrutinize the modes

in which the several elements of the total cause conspire

together, we should find that the assumption of absolute

independence was hazardous, not to say unwarrantable, in a

very great number of cases. These brief remarks upon the

process by which the deflections are brought about must

suffice for the present purpose, as the subject will receive a

fuller investigation in the course of the next chapter.

According, therefore, to the best consideration which

can at the present stage be afforded to this subject, we may

draw a similar conclusion from this deductive line of argument

as from the direct appeal to statistics. The same

general result seems to be established; namely, that approximately,

with sufficient accuracy for all practical purposes, we

may say that an examination of the causes by which the

deflections are generally brought about shows that they are

mostly of such a character as would result in giving us the

commonly accepted 'Law of Error,' as it is termed.[12] The

two lines of enquiry, therefore, within the limits assigned,

afford each other a decided mutual confirmation.

11. (III.) There still remains a third, indirect and

mathematical line of proof, which might be offered to establish

the conclusion that the Law of Error is always one and

the same. It may be maintained that the recognized and

universal employment of one and the same method, that

known to mathematicians and astronomers as the Method of

Least Squares, in all manner of different cases with very

satisfactory results, is compatible only with the supposition

that the errors to which that method is applied must be

grouped according to one invariable law. If all 'laws of

error' were not of one and the same type, that is, if the

relative frequency of large and small divergences (such as we

have been speaking of) were not arranged according to one

pattern, how could one method or rule equally suit them all?

In order to preserve a continuity of treatment, some

notice must be taken of this enquiry here, though, as in the

case of the last argument, any thorough discussion of the

subject is impossible at the present stage. For one thing, it

would involve too much employment of mathematics, or at

any rate of mathematical conceptions, to be suitable for the

general plan of this treatise: I have accordingly devoted a

special chapter to the consideration of it.

The main reason, however, against discussing this argument

here, is, that to do so would involve the anticipation of

a totally different side of the science of Probability from that

hitherto treated of. This must be especially insisted upon, as

the neglect of it involves much confusion and some error.

During these earlier chapters we have been entirely occupied

with laying what may be called the physical foundations of

Probability. We have done nothing else than establish, in one

way or another, the existence of certain groups or arrangements

of things which are found to present themselves in

nature; we have endeavoured to explain how they come to

pass, and we have illustrated their principal characteristics.

But these are merely the foundations of Inference, we have

not yet said a word upon the logical processes which are to

be erected upon these foundations. We have not therefore

entered yet upon the \_logic\_ of chance.

12. Now the way in which the Method of Least Squares

is sometimes spoken of tends to conceal the magnitude of

this distinction. Writers have regarded it as synonymous

with the Law of Error, whereas the fact is that the two are

not only totally distinct things but that they have scarcely

even any necessary connection with each other. The Law of

Error is the statement of a physical fact; it simply assigns,

with more or less of accuracy, the relative frequency with

which errors or deviations of any kind are found in practice

to present themselves. It belongs therefore to what may be

termed the physical foundations of the science. The Method

of Least Squares, on the other hand, is not a law at all in the

scientific sense of the term. It is simply a rule or direction

informing us how we may best proceed to treat any group of

these errors which may be set before us, so as to extract the

true result at which they have been aiming. Clearly therefore

it belongs to the inferential or logical part of the subject.

It cannot indeed be denied that the methods we employ

must have some connection with the arrangement of the facts

to which they are applied; but the two things are none the

less distinct in their nature, and in this case the connection

does not seem at all a necessary one, but at most one of propriety

and convenience. The Method of Least Squares is

usually applied, no doubt, to the most familiar and common

form of the Law of Error, namely the exponential form with

which we have been recently occupied. But other forms of

laws of error may exist, and, if they did, the method in

question might equally well be applied to them. I am not

asserting that it would necessarily be the best method in

every case, but it would be a possible one; indeed we may

go further and say, as will be shown in a future chapter,

that it would be a good method in almost every case. But

its particular merits or demerits do not interfere with its

possible employment in every case in which we may choose

to resort to it. It will be seen therefore, even from the few

remarks that can be made upon the subject here, that the

fact that one and the same method is very commonly employed

with satisfactory results affords little or no proof that

the errors to which it is applied must be arranged according

to one fixed law.

13. So much then for the attempt to prove the prevalence,

in all cases, of this particular law of divergence. The

next point in Quetelet's treatment of the subject which deserves

attention as erroneous or confusing, is the doctrine maintained

by him and others as to the existence of what he terms a \_type\_

in the groups of things in question. This is a not unnatural

consequence from some of the data and conclusions of the

last few paragraphs. Refer back to two of the three classes

of things already mentioned in §4. If it really were the case

that in arranging in order a series of incorrect observations

or attempts of our own, and a collection of natural objects

belonging to some one and the same species or class, we found

that the law of their divergence was in each case identical in

the long run, we should be naturally disposed to apply the

same expression 'Law of Error' to both instances alike,

though in strictness it could only be appropriate to the

former. When we perform an operation ourselves with a

clear consciousness of what we are aiming at, we may quite

correctly speak of every deviation from this as being an

error; but when Nature presents us with a group of objects

of any kind, it is using a rather bold metaphor to speak in

this case also of a law of error, as if she had been aiming at

something all the time, and had like the rest of us missed

her mark more or less in almost every instance.[13]

Suppose we make a long succession of attempts to measure

accurately the precise height of a man, we should from one

cause or another seldom or never succeed in doing so with

absolute accuracy. But we have no right to assume that these

imperfect measurements of ours would be found so to deviate

according to one particular law of error as to present the

precise counterpart of a series of actual heights of \_different\_

men, supposing that these latter were assigned with absolute

precision. What might be the actual law of error in a series

of direct measurements of any given magnitude could hardly

be asserted beforehand, and probably the attempt to determine

it by experience has not been made sufficiently often to

enable us to ascertain it; but upon general grounds it seems

by no means certain that it would follow the so-called exponential

law. Be this however as it may, it is rather a

licence of language to talk as if nature had been at work in

the same way as one of us; aiming (ineffectually for the most

part) at a given result, that is at producing a man endowed

with a certain stature, proportions, and so on, who might

therefore be regarded as the typical man.

14. Stated as above, namely, that there is a fixed

invariable human type to which all individual specimens of

humanity may be regarded as having been meant to attain,

but from which they have deviated in one direction or

another; according to a law of deviation capable of \_à priori\_

determination, the doctrine is little else than absurd. But

if we look somewhat closer at the facts of the case, and the

probable explanation of these facts, we may see our way to

an important truth. The facts, on the authority of Quetelet's

statistics (the great interest and value of which must be

frankly admitted), are very briefly as follows: if we take any

element of our physical frame which admits of accurate

measurement, say the height, and determine this measure in

a great number of different individuals belonging to any

tolerably homogeneous class of people, we shall find that

these heights do admit of an orderly arrangement about a

mean, after the fashion which has been already repeatedly

mentioned. What is meant by a homogeneous class? is a

pertinent and significant enquiry, but applying this condition

to any simple cases its meaning is readily stated. It implies

that the mean in question will be different according to the

nationality of the persons under measurement. According to

Quetelet,[14] in the case of Englishmen the mean is about

5 ft. 9 in.; for Belgians about 5 ft. 7 in.; for the French about

5 ft. 4 in. It need hardly be added that these measures are

those of adult males.

15. It may fairly be asked here what would have

been the consequence, had we, instead of keeping the English

and the French apart, mixed the results of our measurements

of them all together? The question is an important one, as it

will oblige us to understand more clearly what we mean by

homogeneous classes. The answer that would usually be

given to it, though substantially correct, is somewhat too decisive

and summary. It would be said that we are here

mixing distinctly heterogeneous elements, and that in consequence

the resultant law of error will be by no means of

the simple character previously exhibited. So far as such an

answer is to be admitted its grounds are easy to appreciate.

In accordance with the usual law of error the divergences

from the mean grow continuously less numerous as they

increase in amount. Now, if we mix up the French and

English heights, what will follow? Beginning from the

English mean of 5 feet 9 inches, the heights will at first follow

almost entirely the law determined by these English

conditions, for at this point the English data are very numerous,

and the French by comparison very few. But, as we

begin to approach the French mean, the numbers will cease

to show that continual diminution which they should show,

according to the English scale of arrangement, for here the

French data are in turn very numerous, and the English by

comparison few. The result of such a combination of heterogeneous

elements is illustrated by the figure annexed, of

course in a very exaggerated form.

[Figure: Superposition of two Gaussians.]

16. In the above case the nature of the heterogeneity,

and the reasons why the statistics should be so collected and

arranged as to avoid it, seemed tolerably obvious. It will be

seen still more plainly if we take a parallel case drawn from

artificial proceedings. Suppose that after a man had fired a

few thousand shots at a certain spot, say a wafer fixed somewhere

on a wall, the position of the spot at which he aims

were shifted, and he fired a few thousand more shots at the

wafer in its new position. Now let us collect and arrange all

the shots of both series in the order of their departure from

either of the centres, say the new one. Here we should

really be mingling together two discordant sets of elements,

either of which, if kept apart from the other, would have

been of a simple and homogeneous character. We should

find, in consequence, that the resultant law of error betrayed

its composite or heterogeneous origin by a glaring departure

from the customary form, somewhat after the fashion indicated

in the above diagram.

The instance of the English and French heights resembles

the one just given, but falls far short of it in the stringency

with which the requisite conditions are secured. The

fact is we have not here got the most suitable requirements,

viz. a group consisting of a few fixed causes supplemented by

innumerable little disturbing influences. What we call a

nation is really a highly artificial body, the members of

which are subject to a considerable number of local or occasional

disturbing causes. Amongst Frenchmen were included,

presumably, Bretons, Provençals, Alsatians, and so on,

thus commingling distinctions which, though less than those

between French and English, regarded as wholes, are very

far from being insignificant. And to these differences of

race must be added other disturbances, also highly important,

dependent upon varying climate, food and occupation.

It is plain, therefore, that whatever objections exist against

confusing together French and English statistics, exist also,

though of course in a less degree, against confusing together

those of the various provincial and other components which

make up the French people.

17. Out of the great variety of important causes

which influence the height of men, it is probable that those

which most nearly fulfil the main conditions required by the

'Law of Error' are those about which we know the least.

Upon the effects of food and employment, observation has

something to say, but upon the purely physiological causes

by which the height of the parents influences the height of

the offspring, we have probably nothing which deserves to

be called knowledge. Perhaps the best supposition we can

make is one which, in accordance with the saying that 'like

breeds like', would assume that the purely physiological

causes represent the constant element; that is, given a homogeneous

race of people to begin with, who freely intermarry,

and are subject to like circumstances of climate, food,

and occupation, the standard would remain on the whole

constant.[15]

In such a case the man who possessed the mean height,

mean weight, mean strength, and so on, might then be

called, in a sort of way, a 'type'. The deviations from this

type would then be produced by innumerable small influences,

partly physiological, partly physical and social, acting

for the most part independently of one another, and resulting

in a Law of Error of the usual description. Under such

restrictions and explanations as these, there seems to be no

reasonable objection to speaking of a French or English type

or mean. But it must always be remembered that under

the present circumstances of every political nation, these

somewhat heterogeneous bodies might be subdivided into

various smaller groups, each of which would frequently exhibit

the characteristics of such a type in an even more

marked degree.

18. On this point the reports of the Anthropometrical

Committee, already referred to, are most instructive. They

illustrate the extent to which this subdivision could be

carried out, and prove,--if any proof were necessary,--that

the discovery of Quetelet's \_homme moyen\_ would lead us a

long chase. So far as their results go the mean 'English'

stature (in inches) is 67.66. But this is composed of Scotch,

Irish, English and Welsh constituents, the separate means of

these being, respectively; 68.71, 67.90, 67.36, and 66.66.

But these again may be subdivided; for careful observation

shows that the mean English stature is distinctly greater in

certain districts (e.g. the North-Eastern counties) than in

others. Then again the mean of the professional classes is

considerably greater than that of the labourers; and that of

the honest and intelligent is very much greater than that of

the criminal and lunatic constituents of the population.

And, so far as the observations are extensive enough for the

purpose, it appears that every characteristic in respect of the

grouping about a mean which can be detected in the more

extensive of these classes can be detected also in the narrower.

Nor is there any reason to suppose that the same

process of subdivision could not be carried out as much

farther as we chose to prolong it.

19. It need hardly be added to the above remarks

that no one who gives the slightest adhesion to the Doctrine

of Evolution could regard the type, in the above qualified

sense of the term, as possessing any real permanence and

fixity. If the constant causes, whatever they may be, remain

unchanged, and if the variable ones continue in the

long run to balance one another, the results will continue to

cluster about the same mean. But if the constant ones

undergo a gradual change, or if the variable ones, instead of

balancing each other suffer one or more of their number to

begin to acquire a preponderating influence, so as to put a

sort of bias upon their aggregate effect, the mean will at

once begin, so to say, to shift its ground. And having once

begun to shift, it may continue to do so, to whatever extent

we recognize that Species are variable and Development is a

fact. It is as if the point on the target at which we aim, instead

of being fixed, were slowly changing its position as we

continue to fire at it; changing almost certainly to some extent

and temporarily, and not improbably to a considerable

extent and permanently.

20. Our examples throughout this chapter have been

almost exclusively drawn from physical characteristics,

whether of man or of inanimate things; but it need not be

supposed that we are necessarily confined to such instances.

Mr Galton, for instance, has proposed to extend the same

principles of calculation to mental phenomena, with a view

to their more accurate determination. The objects to be

gained by so doing belong rather to the inferential part of

our subject, and will be better indicated further on; but

they do not involve any distinct principle. Like other attempts

to apply the methods of science in the region of the

mind, this proposal has met with some opposition; with very

slight reason, as it seems to me. That our mental qualities,

if they could be submitted to accurate measurement, would

be found to follow the usual Law of Error, may be assumed

without much hesitation. The known extent of the correlation

of mental and bodily characteristics gives high probability

to the supposition that what is proved to prevail, at

any rate approximately, amongst most bodily elements which

have been submitted to measurement, will prevail also

amongst the mental elements.

To what extent such measurements could be carried

out practically, is another matter. It does not seem to

me that it could be done with much success; partly because

our mental qualities are so closely connected with,

indeed so run into one another, that it is impossible to

isolate them for purposes of comparison.[16] This is to some

extent indeed a difficulty in bodily measurements, but it is

far more so in those of the mind, where we can hardly get

beyond what can be called a good guess. The doctrine,

therefore, that mental qualities follow the now familiar law

of arrangement can scarcely be grounded upon anything

more than a strong analogy. Still this analogy is quite

strong enough to justify us in accepting the doctrine and

all the conclusions which follow from it, in so far as our

estimates and measurements can be regarded as trustworthy.

There seems therefore nothing unreasonable in the attempt

to establish a system of natural classification of mankind

by arranging them into a certain number of groups above

and below the average, each group being intended to correspond

to certain limits of excellency or deficiency.[17] All

that is necessary for such a purpose is that the rate of

departure from the mean should be tolerably constant under

widely different circumstances: in this case throughout all

the races of man. Of course if the law of divergence is

the same as that which prevails in inanimate nature we

have a still wider and more natural system of classification

at hand, and one which ought to be familiar, more or less, to

every one who has thus to estimate qualities.

21. Perhaps one of the best illustrations of the legitimate

application of such principles is to be found in Mr

Galton's work on \_Hereditary Genius\_. Indeed the full force

and purport of some of his reasonings there can hardly be

appreciated except by those who are familiar with the conceptions

which we have been discussing in this chapter. We

can only afford space to notice one or two points, but the

student will find in the perusal, of at any rate the more

argumentive parts, of that volume[18] an interesting illustration

of the doctrines now under discussion. For one thing it

may be safely asserted, that no one unfamiliar with the Law

of Error would ever in the least appreciate the excessive

rapidity with which the superior degrees of excellence tend

to become scarce. Every one, of course, can see at once, in

a numerical way at least, what is involved in being 'one of a

million'; but they would not at all understand, how very

little extra superiority is to be looked for in the man who is

'one of two million'. They would confound the mere numerical

distinction, which seems in some way to imply

double excellence, with the intrinsic superiority, which

would mostly be represented by a very small fractional advantage.

To be 'one of ten million' sounds very grand, but

if the qualities under consideration could be estimated in

themselves without the knowledge of the vastly wider area

from which the selection had been made, and in freedom

therefore from any consequent numerical bias, people would

be surprised to find what a very slight comparative superiority

was, as a rule, thus obtained.

22. The point just mentioned is an important one in

arguments from statistics. If, for instance, we find a small

group of persons, connected together by blood-relationship,

and all possessing some mental characteristic in marked

superiority, much depends upon the comparative rarity of

such excellence when we are endeavouring to decide whether

or not the common possession of these qualities was accidental.

Such a decision can never be more than a rough

one, but if it is to be made at all this consideration must

enter as a factor. Again, when we are comparing one nation

with another,[19] say the Athenian with any modern European

people, does the popular mind at all appreciate what sort of

evidence of general superiority is implied by the production,

out of one nation, of such a group as can be composed of

Socrates, Plato, and a few of their contemporaries? In this

latter case we are also, it should be remarked, employing the

'Law of Error' in a second way; for we are assuming that

where the extremes are great so will also the means be, in

other words we are assuming that every amount of departure

from the mean occurs with a (roughly) calculable degree

of relative frequency. However generally this truth may

be accepted in a vague way, its evidence can only be appreciated

by those who know the reasons which can be given

in its favour.

But the same principles will also supply a caution in

the case of the last example. They remind us that, for

the mere purpose of comparison, the \_average\_ man of any

group or class is a much better object for selection than

the eminent one. There may be greater difficulties in the

way of detecting him, but when we have done so we have

got possession of a securer and more stable basis of comparison.

He is selected, by the nature of the case, from

the most numerous stratum of his society; the eminent

man from a thinly occupied stratum. In accordance therefore

with the now familiar laws of averages and of large

numbers the fluctuations amongst the former will generally

be very few and small in comparison with those amongst the

latter.

1. \_Essai de Physique Sociale\_, 1869. \_Anthropométrie\_, 1870.

2. As regards later statistics on the same subject the reader can

refer to the Reports of the Anthropometrical Committee of the

British Association (1879, 1880, 1881, 1883;--especially this

last). These reports seem to me to represent a great advance on the

results obtained by Quetelet, and fully to justify the claim of the

Secretary (Mr C. Roberts) that their statistics are "unique in range

and numbers". They embrace not merely military recruits--like most

of the previous tables--but almost every class and age, and both

sexes. Moreover they refer not only to stature but to a number of

other physical characteristics.

3. As every mathematician knows, the relative numbers of each of these

possible throws are given by the successive terms of the expansion

of (1 + 1)^{10}, viz. 1, 10, 45, 120, 210, 252, 210, 120, 45, 10, 1.

4. That is they will be more densely aggregated. If a space \_the size

of the bull's-eye\_ be examined in each successive circle, the number

of shot marks which it contains will be successively less. The

\_actual\_ number of shots which strike the bull's-eye will not be the

greatest, since it covers so much less surface than any of the other

circles.

5. Commonly called the exponential law; its equation being of the form

y = Ae^{-hx^{2}}. The curve corresponding to it cuts the axis of y

at right angles (expressing the fact that near the mean there are a

large number of values approximately equal); after a time it begins

to slope away rapidly towards the axis of x (expressing the fact

that the results soon begin to grow less common as we recede from

the mean); and the axis of x is an asymptote in both directions

(expressing the fact that no magnitude, however remote from the

mean, is strictly impossible; that is, every deviation, however

excessive, will have to be encountered at length within the range of

a sufficiently long experience). The curve is obviously symmetrical,

expressing the fact that equal deviations from the mean, in excess

and in defect, tend to occur equally often in the long run.

[Figure: Gaussian normal distribution.]

A rough graphic representation of the curve is given above. For the

benefit of those unfamiliar with mathematics one or two brief

remarks may be here appended concerning some of its properties.

(1) It must not be supposed that all specimens of the curve are

similar to one another. The dotted lines are equally specimens of

it. In fact, by varying the essentially arbitrary units in which x

and y are respectively estimated, we may make the portion towards

the vertex of the curve as obtuse or as acute as we please. This

consideration is of importance; for it reminds us that, by varying

one of these arbitrary units, we could get an 'exponential curve'

which should tolerably closely resemble any symmetrical curve of

error, provided that this latter recognized and was founded upon the

assumption that extreme divergences were excessively rare. Hence it

would be difficult, by mere observation, to prove that the law of

error in any given case was not exponential; unless the statistics

were very extensive, or the actual results departed considerably

from the exponential form. (2) It is quite impossible by any graphic

representation to give an adequate idea of the excessive rapidity

with which the curve after a time approaches the axis of x. At the

point R, on our scale, the curve would approach within the

fifteen-thousandth part of an inch from the axis of x, a distance

which only a very good microscope could detect. Whereas in the

hyperbola, e.g. the rate of approach of the curve to its asymptote

is continually decreasing, it is here just the reverse; this rate is

continually increasing. Hence the two, viz. the curve and the axis

of x, appear to the eye, after a very short time, to merge into one

another.

6. As by Quetelet: noted, amongst others, by Herschel, \_Essays\_, page

409.

7. \_Proc. R. Soc.\_ Oct. 21, 1879.

8. We are here considering, remember, the case of a \_finite\_ amount of

statistics; so that there are actual limits at each end.

9. It must be admitted that experience has not yet (I believe) shown

this asymmetry in respect of heights.

10. The above reasoning will probably be accepted as valid at this

stage of enquiry. But in strictness, assumptions are made here,

which however justifiable they may be in themselves, involve

somewhat of an anticipation. They demand, and in a future chapter

will receive, closer scrutiny and criticism.

11. A definite numerical example of this kind of concentration of

frequency about the mean was given in the note to §4. It was of a

binomial form, consisting of the successive terms of the expansion

of (1 + 1)^{m}. Now it may be shown (Quetelet, \_Letters\_, p. 263;

Liagre, \_Calcul des Probabilités\_, §34) that the expansion of such

a binomial, as m becomes indefinitely great, approaches as its limit

the exponential form; that is, if we take a number of equidistant

ordinates proportional respectively to 1, m, m(m - 1)/(1·2) &c., and

connect their vertices, the figure we obtain approximately

represents some form of the curve y = Ae^{-hx^{2}}, and tends to

become identical with it, as m is increased without limit. In other

words, if we suppose the errors to be produced by a limited number

of finite, equal and independent causes, we have an approximation to

the exponential Law of Error, which merges into identity as the

causes are increased in number and diminished in magnitude without

limit. Jevons has given (\_Principles of Science\_, p. 381) a diagram

drawn to scale, to show how rapid this approximation is. One point

must be carefully remembered here, as it is frequently overlooked

(by Quetelet, for instance). The coefficients of a binomial of two

equal terms--as (1 + 1)^{m}, in the preceding paragraph--are

symmetrical in their arrangement from the first, and very speedily

become indistinguishable in (graphical) outline from the final

exponential form. But if, on the other hand, we were to consider the

successive terms of such a binomial as (1 + 4)^{m} (which are

proportional to the relative chances of 0, 1, 2, 3, ... failures in

m ventures, of an event which has one chance in its favour to four

against it) we should have an unsymmetrical succession. If however

we suppose m to increase without limit, as in the former

supposition, the unsymmetry gradually disappears and we tend towards

precisely the same exponential form as if we had begun with two

equal terms. The only difference is that the position of the vertex

of the curve is no longer in the centre: in other words, the

likeliest term or event is not an equal number of successes and

failures but successes and failures in the ratio of 1 to 4.

12. 'Law of Error' is the usual technical term for what has been

elsewhere spoken of above as a Law of Divergence from a mean. It is

in strictness only appropriate in the case of one, namely the third,

of the three classes of phenomena mentioned in §4, but by a

convenient generalization it is equally applied to the other two; so

that we term the amount of the divergence from the mean an 'error'

in every case, however it may have been brought about.

13. This however seems to be the purport, either by direct assertion

or by implication, of two elaborate works by Quetelet, viz. his

\_Physique Sociale\_ and his \_Anthropométrie\_.

14. He scarcely, however, professes to give these as an accurate

measure of the mean height, nor does he always give precisely the

same measure. Practically, none but soldiers being measured in any

great numbers, the English stature did not afford accurate data on

any large scale. The statistics given a few pages further on are

probably far more trustworthy.

15. This statement will receive some explanation and correction in the

next chapter.

16. I am not speaking here of the now familiar results of

Psychophysics, which are mainly occupied with the measurement of

perceptions and other simple states of consciousness.

17. Perhaps the best brief account of Mr Galton's method is to be

found in a paper in \_Mind\_ (July, 1880) on the statistics of Mental

Imagery. The subject under comparison here--viz. the relative

power, possessed by different persons, of raising clear visual

images of objects no longer present to us--is one which it seems

impossible to 'measure', in the ordinary sense of the term. But by

arranging all the answers in the order in which the faculty in

question seems to be possessed we can, with some approach to

accuracy, select the middlemost person in the row and use him as a

basis of comparison with the corresponding person in any other

batch. And similarly with those who occupy other relative positions

than that of the middlemost.

18. I refer to the introductory and concluding chapters: the bulk of

the book is, from the nature of the case, mainly occupied with

statistical and biographical details.

19. See Galton's \_Hereditary Genius\_, pp. 336-350, "On the comparative

worth of different races."

CHAPTER III.

\_ON THE CAUSAL PROCESS BY WHICH THE GROUPS OR SERIES OF PROBABILITY ARE

BROUGHT ABOUT.\_

1. In discussing the question whether all the various

groups and series with which Probability is concerned are of

precisely one and the same type, we made some examination

of the process by which they are naturally produced, but we

must now enter a little more into the details of this process.

All events are the results of numerous and complicated

antecedents, far too numerous and complicated

in fact for it to be possible for us to determine or take

them all into account. Now, though it is strictly true that

we can never determine them all, there is a broad distinction

between the case of Induction, in which we can

make out enough of them, and with sufficient accuracy, to

satisfy a reasonable certainty, and Probability, in which we

cannot do so. To Induction we shall return in a future

chapter, and therefore no more need be said about it here.

We shall find it convenient to begin with a division

which, though not pretending to any philosophical accuracy,

will serve as a preliminary guide. It is the simple division

into objects, and the agencies which affect them. All the

phenomena with which Probability is concerned (as indeed

most of those with which science of any kind is concerned)

are the product of certain objects natural and artificial,

acting under the influence of certain agencies natural and

artificial. In the tossing of a penny, for instance, the objects

would be the penny or pence which were successively

thrown; the agencies would be the act of throwing, and

everything which combined directly or indirectly with this

to make any particular face come uppermost. This is a

simple and intelligible division, and can easily be so extended

in meaning as to embrace every class of objects with

which we are concerned.

Now if, in any two or more cases, we had the same

object, or objects indistinguishably alike, and if they were

exposed to the influence of agencies in all respects precisely

alike, we should expect the results to be precisely similar.

By one of the applications of the familiar principle of the

uniformity of nature we should be confident that exact

likeness in the antecedents would be followed by exact

likeness in the consequents. If the same penny, or similar

pence, were thrown in exactly the same way, we should

invariably find that the same face falls uppermost.

2. What we actually find is, of course, very far removed

from this. In the case of the objects, when they

are artificial constructions, e.g. dice, pence, cards, it is true

that they are purposely made as nearly as possible indistinguishably

alike. We either use the same thing over and

over again or different ones made according to precisely

the same model. But in natural objects nothing of the

sort prevails. In fact when we come to examine them, we

find reproduced in them precisely the same characteristics

as those which present themselves in the final result which

we were asked to explain, so that unless we examine them

a stage further back, as we shall have to do to some extent

at any rate, we seem to be merely postulating again the

very peculiarity of the phenomena which we were undertaking

to explain. They will be found, for instance, to

consist of large classes of objects, throughout all the individual

members of which a general resemblance extends.

Suppose that we were considering the length of life. The

objects here are the human beings, or that selected class

of them, whose lives we are considering. The resemblance

existing among them is to be found in the strength and

soundness of their principal vital organs, together with all

the circumstances which collectively make up what we call

the goodness of their constitutions. It is true that most of

these circumstances do not admit of any approach to actual

measurement; but, as was pointed out in the last chapter,

very many of the circumstances which do admit of such

measurement have been measured, and found to display

the characteristics in question. Hence, from the known

analogy and correlation between our various organs, there

can be no reasonable doubt that if we could arrange human

constitutions in general, or the various elements which compose

them in particular, in the order of their strength, we

should find just such an aggregate regularity and just such

groupings about the mean, as the final result (viz. in this

case the length of their lives) presents to our notice.

3. It will be observed therefore that for this purpose

the existence of natural kinds or groups is necessary.

In our games of chance of course the same die may be

thrown, or a card be drawn from the same pack, as often

as we please; but many of the events which occur to

human beings either cannot be repeated at all, or not often

enough to secure in the case of the single individual any

sufficient statistical uniformity. Such regularity as we trace

in nature is owing, much more than is often suspected,

to the arrangement of things in natural kinds, each of

them containing a large number of individuals. Were each

kind of animals or vegetables limited to a single pair, or

even to but a few pairs, there would not be much scope

left for the collection of statistical tables amongst them.

Or to take a less violent supposition, if the numbers

in each natural class of objects were much smaller than

they are at present, or the differences between their varieties

and sub-species much more marked, the consequent

difficulty of extracting from them any sufficient length of

statistical tables, though not fatal, might be very serious.

A large number of objects in the class, together with that

general similarity which entitles the objects to be fairly

comprised in one class, seem to be important conditions

for the applicability of the theory of Probability to any

phenomenon. Something analogous to this excessive paucity

of objects in a class would be found in the attempt to

apply special Insurance offices to the case of those trades

where the numbers are very limited, and the employment

so dangerous as to put them in a class by themselves. If

an insurance society were started for the workmen in

gunpowder mills alone, a premium would have to be charged

to avoid possible ruin, so high as to illustrate the extreme

paucity of appropriate statistics.

4. So much (at present) for the objects. If we turn

to what we have termed the agencies, we find much the

same thing again here. By the adjustment of their relative

intensity, and the respective frequency of their occurrence,

the total effects which they produce are found to be also

tolerably uniform. It is of course conceivable that this

should have been otherwise. It might have been found

that the second group of conditions so exactly corrected the

former as to convert the merely general uniformity into

an absolute one; or it might have been found, on the

other hand, that the second group should aggravate or

disturb the influence of the former to such an extent

as to destroy all the uniformity of its effects. Practically

neither is the case. The second condition simply varies the

details, leaving the uniformity on the whole of precisely

the same general description as it was before. Or if the

objects were supposed to be absolutely alike, as in the case

of successive throws of a penny, it may serve to bring about

a uniformity. Analysis will show these agencies to be

thus made up of an almost infinite number of different

components, but it will detect the same peculiarity that

we have so often had occasion to refer to, pervading almost

all these components. The proportions in which they are

combined will be found to be nearly, though not quite, the

same; the intensity with which they act will be nearly

though not quite equal. And they will all unite and blend

into a more and more perfect regularity as we proceed to

take the average of a larger number of instances.

Take, for instance, the length of life. As we have seen,

the constitutions of a very large number of persons selected

at random will be found to present much the same feature;

general uniformity accompanied by individual irregularity.

Now when these persons go out into the world, they are

exposed to a variety of agencies, the collective influence

of which will assign to each the length of life allotted to

him. These agencies are of course innumerable, and their

mutual interaction complicated beyond all power of analysis

to extricate. Each effect becomes in its turn a cause, is

interwoven inextricably with an indefinite number of other

causes, and reacts upon the final result. Climate, food,

clothing, are some of these agencies, or rather comprise

aggregate groups of them. The nature of a man's work

is also important. One man overworks himself, another

follows an unhealthy trade, a third exposes himself to infection,

and so on.

The result of all this interaction between what we have

thus called objects and agencies is that the final outcome

presents the same general characteristics of uniformity as may

be detected separately in the two constituent elements. Or

rather, as we shall proceed presently to show, it does so

in the great majority of cases.

5. It may be objected that such an explanation as

the above does not really amount to anything deserving

of the name, for that instead of explaining how a particular

state of things is caused it merely points out that the

same state exists elsewhere. There is a uniformity discovered

in the objects at the stage when they are commonly

submitted to calculation; we then grope about

amongst the causes of them, and after all only discover

a precisely similar uniformity existing amongst these causes.

This is to some extent true, for though part of the objection

can be removed, it must always remain the case that the

foundations of an objective science will rest in the last resort

upon the mere fact that things are found to be of such and

such a character.

6. This division, into objects and the agencies which

affect them, is merely intended for a rough practical arrangement,

sufficient to point out to the reader the immediate

nature of the causes which bring about our familiar

uniformities. If we go back a step further, it might fairly

be maintained that they may be reduced to one, namely,

to the agencies. The objects, as we have termed them,

are not an original creation in the state in which we now

find them. No one supposes that whole groups or classes

were brought into existence simultaneously, with all their

general resemblances and particular differences fully developed.

Even if it were the case that the first parents

of each natural kind had been specially created, instead

of being developed out of pre-existing forms, it would still

be true that amongst the numbers of each that now present

themselves the characteristic differences and resemblances

are the result of what we have termed agencies. Take, for

instance, a single characteristic only, say the height; what

determines this as we find it in any given group of men?

Partly, no doubt, the nature of their own food, clothing,

employment, and so on, especially in the earliest years of

their life; partly also, very likely, similar conditions and

circumstances on the part of their parents at one time or

another. No one, I presume, in the present state of knowledge,

would attempt to enumerate the remaining causes,

or even to give any indication of their exact nature; but

at the same time few would entertain any doubt that

agencies of this general description have been the determining

causes at work.

If it be asked again, Into what may these agencies

themselves be ultimately analysed? the answer to this

question, in so far as it involves any detailed examination

of them, would be foreign to the plan of this essay. In so

far as any general remarks, applicable to nearly all classes

alike of such agencies, are called for, we are led back to

the point from which we started in the previous chapter,

when we were discussing whether there is necessarily one

fixed law according to which all our series are formed. We

there saw that every event might be regarded as being

brought about by a comparatively few important causes, of

the kind which comprises all of which ordinary observation

takes any notice, and an indefinitely numerous group of

small causes, too numerous, minute, and uncertain in their

action for us to be able to estimate them or indeed to take

them individually into account at all. The important ones,

it is true, may also in turn be themselves conceived to be

made up of aggregates of small components, but they are

still best regarded as being by comparison simple and distinct,

for their component parts act mostly in groups collectively,

appearing and disappearing together, so that they

possess the essential characteristics of unity.

7. Now, broadly speaking, it appears to me that the

most suitable conditions for Probability are these: that the

important causes should be by comparison fixed and permanent,

and that the remaining ones should on the average

continue to act as often in one direction as in the other.

This they may do in two ways. In the first place we

may be able to predicate nothing more of them than the

mere fact that they act[1] as often in one direction as the

other; what we should then obtain would be merely the

simple statistical uniformity that is described in the first

chapter. But it may be the case, and in practice generally

is so more or less approximately, that these minor causes

act also in independence of one another. What we then

get is a group of uniformities such as was explained and

illustrated in the second chapter. Every possible combination

of these causes then occurring with a regular degree

of frequency, we find one peculiar kind of uniformity

exhibited, not merely in the mere fact of excess and defect

(of whatever may be the variable quality in question), but

also in every particular amount of excess and defect.

Hence, in this case, we get what some writers term a

'mean' or 'type,' instead of a simple average. For instance,

suppose a man throwing a quoit at a mark. Here

our fixed causes are his strength, the weight of the quoit,

and the intention of aiming at a given point. These we

must of course suppose to remain unchanged, if we are

to obtain any such uniformity as we are seeking. The

minor and variable causes are all those innumerable little

disturbing influences referred to in the last chapter. It

might conceivably be the case that we were only able to

ascertain that these acted as often in one direction as in

the other; what we should then find was that the quoit

tended to fall short of the mark as often as beyond it.

But owing to these little causes being mostly independent

of one another, and more or less equal in their influence,

we find also that every \_amount\_ of excess and defect presents

the same general characteristics, and that in a large number

of throws the quantity of divergences from the mark, of any

given amount, is a tolerably determinate function, according

to a regular law, of that amount of divergence.[2]

8. The necessity of the conditions just hinted at

will best be seen by a reference to cases in which any of

them happen to be missing. Thus we know that the length

of life is on the whole tolerably regular, and so are the

numbers of those who die in successive years or centuries

of most of the commoner diseases. But it does not seem

to be the case with all diseases. What, for instance, of

the Sweating Sickness, the Black Death, the Asiatic

Cholera? The two former either do not recur, or, if they

do, recur in such a mild form as not to deserve the same

name. What in fact of any of the diseases which are

epidemic rather than endemic? All these have their causes

doubtless, and would be produced again by the recurrence

of the conditions which caused them before. But some of

them apparently do not recur at all. They seem to have

depended upon such rare conditions that their occurrence

was almost unique. And of those which do recur the course

is frequently so eccentric and irregular, often so much dependent

upon human will or want of will, as to entirely

deprive their results (that is, the annual number of deaths

which they cause) of the statistical uniformity of which we

are speaking.

The explanation probably is that one of the principal

causes in such cases is what we commonly call contagion.

If so, we have at once a cause which so far from being fixed is

subject to the utmost variability. Stringent caution may

destroy it, carelessness may aggravate it to any extent. The

will of man, as finding its expression either on the part of

government, of doctors, or of the public, may make of it

pretty nearly what is wished, though against the possibility

of its entrance into any community no precautions can absolutely

insure us.

9. If it be replied that this want of statistical regularity

only arises from the fact of our having confined ourselves

to too limited a time, and that we should find

irregularity disappear here, as elsewhere, if we kept our

tables open long enough, we shall find that the answer will

suggest another case in which the requisite conditions for

Probability are wanting. Such a reply would only be conclusive

upon the supposition that the ways and thoughts of

men are in the long run invariable, or if variable, subject to

periodic changes only. On the assumption of a steady progress

in society, either for the better or the worse, the argument

falls to the ground at once. From what we know of

the course of the world, these fearful pests of the past may

be considered as solitary events in our history, or at least

events which will not be repeated. No continued uniformity

would therefore be found in the deaths which they occasion,

though the registrar's books were kept open for a thousand

years. The reason here is probably to be sought in the

gradual alteration of those indefinitely numerous conditions

which we term collectively progress or civilization. Every

little circumstance of this kind has some bearing upon the

liability of any one to catch a disease. But when a kind of

slow and steady tide sets in, in consequence of which these

influences no longer remain at about the same average

strength, warring on about equal terms with hostile influences,

but on the contrary show a steady tendency to increase

their power, the statistics will, with consequent steadiness

and permanence, take the impress of such a change.

10. Briefly then, if we were asked where the distinctive

characteristics of Probability are most prominently

to be found, and where they are most prominently absent,

we might say that (1) they prevail principally in the properties

of natural kinds, both in the ultimate and in the derivative

or accidental properties. In all the characteristics of

natural species, in all they do and in all which happens to

them, so far as it depends upon their properties, we seldom

fail to detect this regularity. Thus in men; their height,

strength, weight, the age to which they live, the diseases of

which they die; all present a well-known uniformity. Life

insurance tables offer the most familiar instance of the importance

of these applications of Probability.

(2) The same peculiarity prevails again in the force

and frequency of most natural agencies. Wind and weather

are seen to lose their proverbial irregularity when examined

on a large scale. Man's work therefore, when operated on

by such agencies as these, even though it had been made in

different cases absolutely alike to begin with, afterwards

shows only a general regularity. I may sow exactly the

same amount of seed in my field every year. The yield may

one year be moderate, the next year be abundant through

favourable weather, and then again in turn be destroyed by

hail. But in the long run these irregularities will be equalized

in the result of my crops, because they are equalized in the

power and frequency of the productive agencies. The

business of underwriters, and offices which insure the crops

against hail, would fall under this class; though, as already

remarked, there is no very profound distinction between them

and the former class.

The reader must be reminded again that this fixity is

only temporary, that is, that even here the series belong to

the class of those which possess a fluctuating type. Those

indeed who believe in the fixity of natural species will have

the best chance of finding a series of the really permanent

type amongst them, though even they will admit that some

change in the characteristic is attainable in length of time.

In the case of the principal natural agencies, it is of course

incontestable that the present average is referable to the

present geological period only. Our average temperature

and average rainfall have in former times been widely

different from what they now are, and doubtless will be so again.

Any fuller investigation of the process by which, on the

Theory of Evolution, out of a primeval simplicity and uniformity

the present variety was educed, hardly belongs to

the scope of the present work: at most, a few hints must suffice.

11. The above, then, are instances of natural objects

and natural agencies. There seems reason to believe that it

is in such things only, as distinguished from things artificial,

that the property in question is to be found. This is an assertion

that will need some discussion and explanation. Two

instances, in apparent opposition, will at once occur to the

mind of some readers; one of which, from its great intrinsic

importance, and the other, from the frequency of the problems

which it furnishes, will demand a few minutes' separate

examination.

(1) The first of these is the already mentioned case of

instrumental observations. In the use of astronomical and

other instruments the utmost possible degree of accuracy is

often desired, a degree which cannot be reasonably hoped for

in any one single observation. What we do therefore in

these cases is to make a large number of successive observations

which are naturally found to differ somewhat from each

other in their results; by means of these the true value

(as explained in a future chapter, on the Method of Least

Squares) is to be determined as accurately as possible. The

subjects then of calculation here are a certain number of

elements, slightly incorrect elements, given by successive

observations. Are not these observations artificial, or the

direct product of voluntary agency? Certainly not: or rather,

the answer depends on what we understand by voluntary.

What is really intended and aimed at by the observer, is of

course, perfect accuracy, that is, the true observation, or the

voluntary steps and preliminaries on which this observation

depends. Whether voluntary or not, this result only can be

called intentional. But this result is not obtained. What

we actually get in its place is a series of deviations from it,

containing results more or less wide of the truth. Now by

what are these deviations caused? By just such agencies as

we have been considering in some of the earlier sections in

this chapter. Heat and its irregular warping influence,

draughts of air producing their corresponding effects, dust

and consequent friction in one part or another, the slight

distortion of the instrument by strains or the slow uneven

contraction which continues long after the metal was cast;

these and such as these are some of the causes which divert

us from the truth. Besides this group, there are others

which certainly do depend upon human agency, but which

are not, strictly speaking, voluntary. They are such as the

irregular action of the muscles, inability to make our various

organs and members execute precisely the purposes we have

in mind, perhaps different rates in the rapidity of the nervous

currents, or in the response to stimuli, in the same or

different observers. The effect produced by some of these,

and the allowance that has in consequence to be made, are

becoming familiar even to the outside world under the name

of the 'personal equation' in astronomical, psychophysical,

and other observations.

12. (2) The other example, alluded to above, is the stock

one of cards and dice. Here, as in the last case, the result

is remotely voluntary, in the sense that deliberate volition

presents itself at one stage. But subsequently to this stage,

the result is produced or affected by so many involuntary

agencies that it owes its characteristic properties to these.

The turning up, for example, of a particular face of a die is

the result of voluntary agency, but it is not an immediate

result. That particular face was not chosen, though the fact

of its being chosen was the remote consequence of an act of

choice. There has been an intermediate chaos of conflicting

agencies, which no one can calculate before or distinguish

afterwards. These agencies seem to show a uniformity in

the long run, and thence to produce a similar uniformity in

the result. The drawing of a card from a pack is indeed

more directly volitional, as in cutting for partners in a game

of whist. But no one continues to do this long without

having the pack well shuffled in the interval, whereby a host

of involuntary influences are let in.

13. The once startling but now familiar uniformities

exhibited in the cases of suicides and misdirected letters, do

not belong to the same class. The final resolution, or want

of it, which leads to these results, is in each case indeed an

important ingredient in the individual's action or omission;

but, in so far as volition has anything to do with the results

\_as a whole\_, it instantly disturbs them. If the voice of the

Legislature speaks out, or any great preacher or moralist

succeeds in deterring, or any impressive example in influencing,

our moral statistics are instantly tampered with.

Some further discussion will be devoted to this subject in a

future chapter; it need only be remarked here that (always

excluding such common or general influence as those just

mentioned) the average volition, potent as it is in each

separate case, is on the whole swayed by non-voluntary conditions,

such as those of health, the casualties of employment, &c.,

in fact the various circumstances which influence

the length of a man's life.

14. Such distinctions as those just insisted on may

seem to some persons to be needless, but serious errors have

occasionally arisen from the neglect of them. The immediate

products of man's mind, so far indeed as we can make

an attempt to obtain them, do not seem to possess this

essential characteristic of Probability. Their characteristic

seems rather to be, either perfect mathematical accuracy or

utter want of it, either law unfailing or mere caprice. If,

e.g., we find the trees in a forest growing in straight lines,

we unhesitatingly conclude that they were planted by man

as they stand. It is true on the other hand, that if we find

them not regularly planted, we cannot conclude that they

were not planted by man; partly because the planter may

have worked without a plan, partly because the subsequent

irregularities brought on by nature may have obscured the

plan. Practically the mind has to work by the aid of imperfect

instruments, and is subjected to many hindrances

through various and conflicting agencies, and by these means

the work loses its original properties. Suppose, for instance,

that a man, instead of producing numerical results by imperfect

observations or by the cast of dice, were to select

them at first hand for himself by simply thinking of them

at once; what sort of series would he obtain? It would be

about as difficult to obtain in this way any such series as

those appropriate to Probability as it would be to keep his

heart or pulse working regularly by direct acts of volition,

supposing that he had the requisite control over these organs.

But the mere suggestion is absurd. A man must have an

object in thinking, he must think according to a rule or formula;

but unless he takes some natural series as a copy, he

will never be able to construct one mentally which shall permanently

imitate the originals. Or take another product of

human efforts, in which the intention can be executed with

tolerable success. When any one builds a house, there are

many slight disturbing influences at work, such as shrinking

of bricks and mortar, settling of foundations, &c. But the

effect which these disturbances are able to produce is so inappreciably

small, that we may fairly consider that the result

obtained is the direct product of the mind, the accurate

realization of its intention. What is the consequence?

Every house in the row, if designed by one man and at one

time, is of exactly the same height, width, &c. as its

neighbours; or if there are variations they are few, definite,

and regular. The result offers no resemblance whatever to

the heights, weights, &c. of a number of men selected at

random. The builder probably had some regular design in

contemplation, and he has succeeded in executing it.

15. It may be replied that if we extend our observations,

say to the houses of a large city, we shall then detect

the property under discussion. The different heights of a

great number, when grouped together, might be found to

resemble those of a great number of human beings under

similar treatment. Something of this kind might not improbably

be found to be the case, though the resemblance

would be far from being a close one. But to raise this

question is to get on to different ground, for we were

speaking (as remarked above) not of the work of different

minds with their different aims, but of that of one mind.

In a multiplicity of designs, there may be that variable uniformity,

for which we may look in vain in a single design.

The heights which the \_different\_ builders contemplated

might be found to group themselves into something of the

same kind of uniformity as that which prevails in most

other things which they should undertake to do independently.

We might then trace the action of the same two

conditions,--a uniformity in the multitude of their different

designs, a uniformity also in the infinite variety of the

influences which have modified those designs. But this is a

very different thing from saying that the work of one man

will show such a result as this. The difference is much like

that between the tread of a thousand men who are stepping

without thinking of each other, and their tread when they

are drilled into a regiment. In the former case there is

the working, in one way or another, of a thousand minds;

in the latter, of one only.

The investigations of this and the former chapter

constitute a sufficiently close examination into the detailed

causes by which the peculiar form of statistical results with

which we are concerned is actually produced, to serve the

purpose of a work which is occupied mainly with the \_methods\_

of the Science of Probability. The great importance, however,

of certain statistical or sociological enquiries will demand

a recurrence in a future chapter to one particular

application of these statistics, viz. to those concerned with

some classes of human actions.

16. The only important addition to, or modification of,

the foregoing remarks which I have found occasion to make

is due to Mr Galton. He has recently pointed out,--and was

I believe the first to do so,--that in certain cases some

analysis of the causal processes can be effected, and is in fact

absolutely necessary in order to account for the facts observed.

Take, for instance, the \_heights\_ of the population of

any country. If the distribution or dispersion of these about

their mean value were left to the unimpeded action of those

myriad productive agencies alluded to above, we should certainly

obtain such an arrangement in the posterity of any

one generation as had already been exhibited in the parents.

That is, we should find repeated in the previous stage the

same kind of order as we were trying to account for in the

following stage.

But then, as Mr Galton insists, if such agencies acted

freely and independently, though we should get the same

\_kind\_ of arrangement or distribution, we should not get the

same \_degree\_ of it: there would, on the contrary, be a tendency

towards further dispersion. The 'curve of facility' (v. the

diagram on p. 29) would belong to the same class, but would

have a different modulus. We shall see this at once if we

take for comparison a case in which similar agencies work

their way without any counteraction whatever. Suppose, for

instance, that a large number of persons, whose fortunes

were equal to begin with, were to commence gambling or

betting continually for some small sum. If we examine

their circumstances after successive intervals of time, we

should expect to find their fortunes distributed according to

the same general law,--i.e. the now familiar law in question,--but

we should also expect to find that the poorest

ones were slightly poorer, and the richest ones slightly

richer, on each successive occasion. We shall see more

about this in a future chapter (on \_Gambling\_), but it may

be taken for granted here that there is nothing in the laws

of chance to resist this tendency towards intensifying the extremes.

Now it is found, on the contrary, in the case of vital

phenomena,--for instance in that of height, and presumably

of most of the other qualities which are in any way characteristic

of natural kinds,--that there is, through a number of

successive generations, a remarkable degree of fixity. The

tall men are not taller, and the short men are not shorter,

per cent. of the population in successive generations: always

supposing of course that some general change of circumstances,

such as climate, diet, &c. has not set in. There

must therefore here be some cause at work which tends, so

to say, to draw in the extremes and thus to check the otherwise

continually increasing dispersion.

17. The facts were first tested by careful experiment.

At the date of Mr Galton's original paper on the subject,[3]

there were no available statistics of heights of human beings;

so a physical element admitting of careful experiment (viz.

the size or weight of certain seeds) was accurately estimated.

From these data the actual amount of reversion from the

extremes, that is, of the slight pressure continually put upon

the extreme members with the result of crowding them back

towards the mean, was determined, and this was compared

with what theory would require in order to keep the characteristics

of the species permanently fixed. Since then,

statistics have been obtained to a large extent which deal

directly with the heights of human beings.

The general conclusion at which we arrive is that there

are several causes at work which are neither slight nor independent.

There is, for instance, the observed fact that the

extremes are as a rule not equally fertile with the means,

nor equally capable of resisting death and disease. Hence

as regards their mere numbers, there is a tendency for them

somewhat to thin out. Then again there is a distinct

positive cause in respect of 'reversion.' Not only are the

offspring of the extremes less numerous, but these offspring

also tend to cluster about a mean which is, so to say, shifted

a little towards the true centre of the whole group; i.e. towards

the mean offspring of the mean parents.

18. For a full discussion of these characteristics, and

for a variety of most ingenious illustrations of their mode of

agency and of their comparative efficacy, the reader may be

referred to Mr Galton's original articles. For our present

purpose it will suffice to say that these characteristics tend

towards maintaining the fixity of species; and that though

they do not affect what may be called the general nature of

the 'probability curve' or 'law of facility', they do determine

its precise value in the cases in question. If, indeed, it be

asked why there is no need for any such corrective influence

in the case of, say, firing at a mark: the answer is that there

is no opening for it except where a \_cumulative\_ influence is introduced.

The reason why the fortunes of our betting party

showed an ever increasing divergency, and why some special

correction was needed in order to avert such a tendency in

the case of vital phenomena, was that the new starting-point

at every step was slightly determined by the results of the

previous step. The man who has lost a shilling one time

starts, next time, worse off by just a shilling; and, but for

the corrections we have been indicating, the man who was

born tall would, so to say, throw off his descendants from a

vantage ground of superior height. The true parallel in the

case of the marksmen would be to suppose that their new

points of aim were always shifted a little in the direction of

the last divergence. The spreading out of the shot-marks

would then continue without limit, just as would the divergence

of fortunes of the supposed gamblers.

1. As stated above, this is really little more than a re-statement, a

stage further back, of the existence of the same kind of uniformity

as that which we are called upon to explain in the concrete details

presented to us in experience.

2. "It would seem in fact that in coarse and rude observations the

errors proceed from a \_very few\_ principal causes, and in

consequence our hypothesis [as to the Exponential Law of Error] will

probably represent the facts only imperfectly, and the frequency of

the errors will only approximate roughly and vaguely to the law

which follows from it. But when astronomers, not content with the

degree of accuracy they had reached, prosecuted their researches

into the remaining sources of error, they found that not three or

four, but a \_great number\_ of minor sources of error of nearly

co-ordinate importance began to reveal themselves, having been till

then masked and overshadowed by the graver errors which had been now

approximately removed.... There were errors of graduation, and many

others in the contraction of instruments; other errors of their

adjustments; errors (technically so called) of \_observation\_; errors

from the changes of temperature, of weather, from slight irregular

motions and vibrations; in short, the thousand minute disturbing

influences with which modern astronomers are familiar." (Extracted

from a paper by Mr Crofton in the Vol. of the \_Philosophical

Transactions\_ for 1870, p. 177.)

3. \_Typical Laws of Heredity\_; read before the Royal Institution,

Feb. 9, 1877. See also \_Journal of the Anthrop. Inst.\_ Nov. 1885.

CHAPTER IV.

\_ON THE MODES OF ESTABLISHING AND DETERMINING THE EXISTENCE AND

NUMERICAL PROPORTIONS OF THE CHARACTERISTIC PROPERTIES OF OUR SERIES

OR GROUPS.\_

1. At the point which we have now reached, we are

supposed to be in possession of series or groups of a certain

kind, lying at the bottom, as one may say, and forming the

foundation on which the Science of Probability is to be

erected. We have described with sufficient particularity the

characteristics of such a series, and have indicated the process

by which it is, as a rule, actually brought about in

nature. The next enquiries which have to be successively

made are, how in any particular case we are to establish

their existence and determine their special character and

properties? and secondly,[1] when we have obtained them, in

what mode are they to be employed for logical purposes?

The answer to the former enquiry does not seem difficult.

Experience is our sole guide. If we want to discover what is

in reality a series of \_things\_, not a series of our own conceptions,

we must appeal to the things themselves to obtain it,

for we cannot find much help elsewhere. We cannot tell

how many persons will be born or die in a year, or how

many houses will be burnt or ships wrecked, without actually

counting them. When we thus speak of 'experience' we

mean to employ the term in its widest signification; we

mean experience supplemented by all the aids which inductive

or deductive logic can afford. When, for instance,

we have found the series which comprises the numbers of

persons of any assigned class who die in successive years, we

have no hesitation in extending it some way into the future

as well as into the past. The justification of such a procedure

must be sought in the ordinary canons of Induction.

As a special discussion will be given upon the connection

between Probability and Induction, no more need be said

upon this subject here; but nothing will be found there at

variance with the assertion just made, that the series we

employ are ultimately obtained by experience only.

2. In many cases it is undoubtedly true that we do not

resort to direct experience at all. If I want to know what is

my chance of holding ten trumps in a game of whist, I do

not enquire how often such a thing has occurred before. If

all the inhabitants of the globe were to divide themselves up

into whist parties they would have to keep on at it for a

great many years, if they wanted to settle the question

satisfactorily in that way. What we do of course is to calculate

algebraically the proportion of possible combinations

in which ten trumps can occur, and take this as the answer

to our problem. So again, if I wanted to know the chance

of throwing six with a die whose faces were unequal, it

would be a question if my best way would not be to calculate

geometrically the solid angle subtended at the centre of

gravity by the opposite face, and the ratio of this to the

whole surface of a sphere would represent sufficiently closely

the chance required.

It is quite true that in such examples as the above, especially

the former one, nobody would ever think of appealing

to statistics. This would be a tedious process to adopt when,

as here, the mechanical and other conditions upon which the

production of the events depend are comparatively few, determinate,

and admit of isolated consideration, whilst the

enormous number of combinations which can be constructed

out of them causes an enormous consequent multiplicity of

ways in which the events can possibly happen. Hence, in

practice, \_à priori\_ determination is often easy, whilst \_à posteriori\_

appeal to experience would be not merely tedious but

utterly impracticable. This, combined with the frequent

simplicity and attractiveness of such examples when deductively

treated, has made them very popular, and produced the

impression in many quarters that they are the proper typical

instances to illustrate the theory of chance. Whereas, had

the science been concerned with those kinds of events only

which in practice are commonly made subjects of insurance,

probably no other view would ever have been taken than

that it was based upon direct appeal to experience.

3. When, however, we look a little closer, we find that

there is no occasion for such a sharp distinction as that apparently

implied between the two classes of examples just

indicated. In such cases as those of dice and cards, even, in

which we appear to reason directly from the determining

conditions, or possible variety of the events, rather than from

actual observation of their occurrence, we shall find that this

procedure is only valid by the help of a tacit assumption

which can never be determined otherwise than by direct

experience. It is, no doubt, an exceedingly natural and

obvious assumption, and one which is continually deriving

fresh weight from every-day observation, but it is one which

ought not to be admitted without consideration. As this is a

very important matter, not so much in itself as in connection

with the light which it throws upon the theory of the subject,

we will enter into a somewhat detailed examination of it.

Let us take a very simple example, that of tossing up a

penny. Suppose that I am contemplating a succession of

two throws; I can see that the only possible events are[2]

HH, HT, TH, TT. So much is certain. We are moreover

tolerably well convinced from experience that these

events occur, in the long run, about equally often. This is

of course admitted on all hands. But on the view commonly

maintained, it is contended that we might have known the

fact beforehand on grounds which are applicable to an indefinite

number of other and more complex cases. The form in

which this view would generally be advanced is, that we are

enabled to state beforehand that the four throws above mentioned

are \_equally likely\_. If in return we ask what is meant

by the expression 'equally likely', it appears that there are

two and only two possible forms of reply. One of these seeks

the explanation in the state of mind of the observer, the

other seeks it in some characteristic of the things observed.

(1) It might, for instance, be said on the one hand, that

what is meant is that the four events contemplated are

equally easy to imagine, or, more accurately, that our expectation

or belief in their occurrence is equal. We could

hardly be content with this reply, for the further enquiry

would immediately be urged, On what ground is this to be

believed? What are the characteristics of events of which

our expectation is equal? If we consented to give an answer

to this further enquiry, we should be led to the second form

of reply, to be noticed directly; if we did not consent we

should, it seems, be admitting that Probability was only a

portion of Psychology, confined therefore to considering states

of mind in themselves, rather than in their reference to facts,

viz. as being true or false. We should, that is, be ceasing

to make it a science of inference about things. This point

will have to be gone into more thoroughly in another chapter;

but it is impossible to direct attention too prominently

to the fact that Logic (and therefore Probability as a branch

of Logic) is not concerned with what men \_do\_ believe, but

with what they ought to believe, if they are to believe

correctly.

(2) In the other form of reply the explanation of the

phrase in question would be sought, not in a state of mind,

but in a quality of the things contemplated. We might assign

the following as the meaning, viz. that the events really

would occur with equal frequency in the long run. The

ground of this assertion would probably be found in past experience,

and it would doubtless be impossible so to frame

the answer as to exclude the notion of our belief altogether.

But still there is a broad distinction between seeking an

equality in the amount of our belief, as before, and in the

frequency of occurrence of the events themselves, as here.

4. When we have got as far as this it can readily be

shown that an appeal to experience cannot be long evaded.

For \_can\_ the assertion in question (viz. that the throws of the

penny will occur equally often) be safely made \_à priori\_?

Those who consider that it can seem hardly to have fully

faced the difficulties which meet them. For when we begin

to enquire seriously whether the penny will really do what

is expected of it, we find that restrictions have to be introduced.

In the first place it must be an ideal coin, with

its sides equal and fair. This restriction is perfectly intelligible;

the study of solid geometry enables us to idealize a

penny into a circular or cylindrical lamina. But this condition

by itself is not sufficient, others are wanted as well. The

penny was supposed to be tossed up, as we say 'at random.'

What is meant by this, and how is this process to be idealized?

To ask this is to introduce no idle subtlety; for it

would scarcely be maintained that the heads and tails would

get their fair chances if, immediately before the throwing,

we were so to place the coin in our hands as to start it

always with the same side upwards. The difference that

would result in consequence, slight as its cause is, would

tend in time to show itself in the results. Or, if we persisted

in starting with each of the two sides alternately

upwards, would the longer repetitions of the same side get

their fair chance?

Perhaps it will be replied that if we think nothing whatever

about these matters all will come right of its own accord.

It may, and doubtless will be so, but this is falling back upon

experience. It is here, then, that we find ourselves resting on

the experimental assumption above mentioned, and which

indeed cannot be avoided. For suppose, lastly, that the

circumstances of nature, or my bodily or mental constitution,

were such that the same side always \_is\_ started upwards, or

indeed that they are started in any arbitrary order of our

own? Well, it will be replied, it would not then be a fair

trial. If we press in this way for an answer to such enquiries,

we shall find that these tacit restrictions are really nothing

else than a mode of securing an experimental result. They

are only another way of saying, Let a series of actions be

performed in such a way as to secure a sequence of a particular

kind, viz., of the kind described in the previous chapters.

5. An intermediate way of evading the direct appeal

to experience is sometimes found by defining the probability

of an event as being measured by the ratio which the

number of cases favourable to the event bears to the total

number of cases which are possible. This seems a somewhat

loose and ambiguous way of speaking. It is clearly not

enough to \_count\_ the number of cases merely, they must also

be \_valued\_, since it is not certain that each is equally potent

in producing the effect. This, of course, would never be

denied, but sufficient importance does not seem to be attached

to the fact that we have really no other way of

valuing them except by estimating the effects which they

actually do, or would produce. Instead of thus appealing to

the proportion of cases favourable to the event, it is far better

(at least as regards the foundation of the science, for we are

not at this moment discussing the practical method of facilitating

our calculations) to appeal at once to the proportion

of cases in which the event actually occurs.

6. The remarks above made will apply, of course, to

most of the other common examples of chance; the throwing

of dice, drawing of cards, of balls from bags, &c. In the

last case, for instance, one would naturally be inclined to

suppose that a ball which had just been put back would

thereby have a better chance of coming out again next time,

since it will be more in the way for that purpose. How is

this to be prevented? If we designedly thrust it to the

middle or bottom of the others, we may overdo the precaution;

and are in any case introducing human design, that

element so essentially hostile to all that we understand by

chance. If we were to trust to a good shake setting matters

right, we may easily be deceived; for shaking the bag can

hardly do more than diminish the disposition of those balls

which were already in each other's neighbourhood, to remain

so. In the consequent interaction of each upon all, the

arrangement in which they start cannot but leave its impress

to some extent upon their final positions. In all such cases,

therefore, if we scrutinize our language, we shall find that

any supposed \_à priori\_ mode of stating a problem is little else

than a compendious way of saying, Let means be taken for

obtaining a given result. Since it is upon this result that

our inferences ultimately rest, it seems simpler and more

philosophical to appeal to it at once as the groundwork of

our science.

7. Let us again take the instance of the tossing of a

penny, and examine it somewhat more minutely, to see what

can be actually proved about the results we shall obtain.

We are willing to give the pence fair treatment by assuming

that they are perfect, that is, that in the long run they show

no preference for either head or tail; the question then

remains, Will the repetitions of the same face obtain the

proportional shares to which they are entitled by the

usual interpretations of the theory? Putting then, as before,

for the sake of brevity, H for head, and HH for heads twice

running, we are brought to this issue;--Given that the

chance of H is 1/2, does it follow necessarily that the chance of HH

(with two pence) is 1/4? To say nothing of 'H ten times'

occurring once in 1024 times (with ten pence), need it occur

at all? The mathematicians, for the most part, seem to think

that this conclusion follows necessarily from first principles;

to me it seems to rest upon no more certain evidence than a

reasonable extension by Induction.

Taking then the possible results which can be obtained

from a pair of pence, what do we find? Four different

results may follow, namely, (1) HT, (2) HH, (3) TH, (4) TT.

If it can be proved that these four are equally probable, that

is, occur equally often, the commonly accepted conclusions

will follow, for a precisely similar argument would apply to

all the larger numbers.

8. The proof usually advanced makes use of what is

called the Principle of Sufficient Reason. It takes this

form;--Here are four kinds of throws which may happen;

once admit that the separate elements of them, namely, H

and T, happen equally often, and it will follow that the above

combinations will also happen equally often, for no reason can

be given in favour of one of them that would not equally hold

in favour of the others.

To a certain extent we must admit the validity of the

principle for the purpose. In the case of the throws given

above, it would be valid to prove the equal frequency of (1)

and (3) and also of (2) and (4); for there is no difference

existing between these pairs except what is introduced by our

own notation.[3] TH is the same as HT, except in the order

of the occurrence of the symbols H and T, which we do not

take into account. But either of the pair (1) and (3) \_is\_

different from either of the pair (2) and (4). Transpose the

notation, and there would still remain here a distinction which

the mind can recognize. A succession of the same thing

twice running is distinguished from the conjunction of two

different things, by a distinction which does not depend

upon our arbitrary notation only, and would remain entirely

unaltered by a change in this notation. The principle therefore

of Sufficient Reason, if admitted, would only prove that

doublets of the two kinds, for example (2) and (4), occur

equally often, but it would not prove that they must each

occur once in four times. It cannot be proved indeed in this

way that they need ever occur at all.

9. The formula, then, not being demonstrable \_à priori\_,

(as might have been concluded,) can it be obtained by experience?

To a certain extent it can; the present experience

of mankind in pence and dice seems to show that the smaller

successions of throws do really occur in about the proportions

assigned by the theory. But how nearly they do so no one

can say, for the amount of time and trouble to be expended

before we could feel that we have verified the fact, even for

small numbers, is very great, whilst for large numbers it

would be simply intolerable. The experiment of throwing

often enough to obtain 'heads ten times' has been actually

performed by two or three persons, and the results are given

by De Morgan, and Jevons.[4] This, however, being only

sufficient on the average to give 'heads ten times' a single

chance, the evidence is very slight; it would take a considerable

number of such experiments to set the matter

nearly at rest.

Any such rule, then, as that which we have just been

discussing, which professes to describe what will take place

in a long succession of throws, is only conclusively proved by

experience within very narrow limits, that is, for small repetitions

of the same face; within limits less narrow, indeed,

we feel assured that the rule cannot be flagrantly in error,

otherwise the variation would be almost sure to be detected.

From this we feel strongly inclined to infer that the same

law will hold throughout. In other words, we are inclined

to extend the rule by Induction and Analogy. Still there

are so many instances in nature of proposed laws which hold

within narrow limits but get egregiously astray when we

attempt to push them to great lengths, that we must give at

best but a qualified assent to the truth of the formula.

10. The object of the above reasoning is simply to

show that we cannot be certain that the rule is true. Let

us now turn for a minute to consider the causes by which

the succession of heads and tails is produced, and we may

perhaps see reasons to make us still more doubtful.

It has been already pointed out that in calculating probabilities

\_à priori\_, as it is called, we are only able to do so by

introducing restrictions and suppositions which are in reality

equivalent to assuming the expected results. We use words

which in strictness mean, Let a given process be performed;

but an analysis of our language, and an examination of

various tacit suppositions which make themselves felt the

moment they are not complied with, soon show that our real

meaning is, Let a series of a given kind be obtained; it is to

this series only, and not to the conditions of its production,

that all our subsequent calculations properly apply. The

physical process being performed, we want to know whether

anything resembling the contemplated series really will be

obtained.

Now if the penny were invariably set the same side uppermost,

and thrown with the same velocity of rotation and

to the same height, &c.--in a word, subjected to the same

conditions,--it would always come down with the same side

uppermost. Practically, we know that nothing of this kind

occurs, for the individual variations in the results of the

throws are endless. Still there will be an \_average\_ of these

conditions, about which the throws will be found, as it were,

to cluster much more thickly than elsewhere. We should be

inclined therefore to infer that if the same side were always

set uppermost there would really be a departure from the

sort of series which we ordinarily expect. In a very large

number of throws we should probably begin to find, under

such circumstances, that either head or tail was having a

preference shown to it. If so, would not similar effects be

found to be connected with the way in which we started

each successive \_pair\_ of throws? According as we chose to

make a practice of putting HH or TT uppermost, might

there not be a disturbance in the proportion of successions of

two heads or two tails? Following out this train of reasoning,

it would seem to point with some likelihood to the conclusion

that in order to obtain a series of the kind we

expect, we should have to dispose the antecedents in a

similar series at the start. The changes and chances produced

by the act of throwing might introduce infinite individual

variations, and yet there might be found, in the very

long run, to be a close similarity between these two series.

11. This is, to a certain extent, only shifting the difficulty,

I admit; for the claim formerly advanced about the

possibility of proving the proportions of the throws in the

former series, will probably now be repeated in favour of

those in the latter. Still the question is very much narrowed,

for we have reduced it to a series of \_voluntary\_ acts.

A man may put whatever side he pleases uppermost. He

may act consciously, as I have said, or he may think nothing

whatever about the matter, that is, throw at random; if so,

it will probably be asserted by many that he will involuntarily

produce a series of the kind in question. It may be

so, or it may not; it does not seem that there are any

easily accessible data by which to decide. All that I am

concerned with here is to show the likelihood that the commonly

received result does in reality depend upon the fulfilment

of a certain condition at the outset, a condition which

it is certainly optional with any one to fulfil or not as he

pleases. The short successions doubtless will take care of

themselves, owing to the infinite complications produced by

the casual variations in throwing; but the long ones may

suffer, unless their interest be consciously or unconsciously

regarded at the outset.

12. The advice, 'Only try long enough, and you will

sooner or later get any result that is possible,' is plausible,

but it rests only on Induction and Analogy; mathematics do

not prove it. As has been repeatedly stated, there are two

distinct views of the subject. Either we may, on the one

hand, take a series of symbols, call them heads and tails;

H, T, &c.; and make the assumption that each of these, and

each pair of them, and so on, will occur in the long run with

a regulated degree of frequency. We may then calculate

their various combinations, and the consequences that may

be drawn from the data assumed. This is a purely algebraical

process; it is infallible; and there is no limit whatever to the

extent to which it may be carried. This way of looking at

the matter may be, and undoubtedly should be, nothing

more than the counterpart of what I have called the substituted

or idealized series which generally has to be introduced

as the basis of our calculation. The danger to be guarded

against is that of regarding it too purely as an algebraical

conception, and thence of sinking into the very natural

errors both of too readily evolving it out of our own consciousness,

and too freely pushing it to unwarranted lengths.

Or on the other hand, we may consider that we are treating

of the behaviour of \_things\_;--balls, dice, births, deaths, &c.;

and drawing inferences about them. But, then, what

were in the former instance allowable assumptions, become

here propositions to be tested by experience. Now the whole

theory of Probability as a practical science, in fact as anything

more than an algebraical truth, depends of course upon

there being a close correspondence between these two views

of the subject, in other words, upon our substituted series

being kept in accordance with the actual series. Experience

abundantly proves that, between considerable limits, in the

example in question, there does exist such a correspondence.

But let no one attempt to enforce our assent to every remote

deduction that mathematicians can draw from their formulæ.

When this is attempted the distinction just traced becomes

prominent and important, and we have to choose our side.

Either we go over to the mathematics, and so lose all right

of discussion about the things; or else we take part with the

things, and so defy the mathematics. We do not question

the formal accuracy of the latter within their own province,

but either we dismiss them as somewhat irrelevant, as applying

to data of whose correctness we cannot be certain, or

we take the liberty of remodelling them so as to bring them

into accordance with facts.

13. A critic of any doctrine can hardly be considered

to have done much more than half his duty when he has

explained and justified his grounds for objecting to it. It

still remains for him to indicate, if only in a few words,

what he considers its legitimate functions and position to be,

for it can seldom happen that he regards it as absolutely

worthless or unmeaning. I should say, then, that when

Probability is thus divorced from direct reference to objects,

as it substantially is by not being founded upon experience,

it simply resolves itself into the common algebraical or arithmetical

doctrine of Permutations and Combinations.[5] The

considerations upon which these depend are purely formal

and necessary, and can be fully reasoned out without any

appeal to experience. We there start from pure considerations

of number or magnitude, and we terminate with them,

having only arithmetical calculations to connect them together.

I wish, for instance, to find the chance of throwing

heads three times running with a penny. All I have to do

is first to ascertain the possible number of throws. Permutations

tell me that with two things thus in question (viz.

head and tail) and three times to perform the process, there

are eight possible forms of the result. Of these eight one

only being favourable, the chance in question is pronounced

to be one-eighth.

Now though it is quite true that the actual calculation of

every chance problem must be of the above character, viz. an

algebraical or arithmetical process, yet there is, it seems to

me, a broad and important distinction between a material

science which employs mathematics, and a formal one which

consists of nothing but mathematics. When we cut ourselves

off from the necessity of any appeal to experience, we

are retaining only the intermediate or calculating part of the

investigation; we may talk of dice, or pence, or cards, but

these are really only names we choose to give to our symbols.

The H's and T's with which we deal have no bearing on objective

occurrences, but are just like the x's and y's with

which the rest of algebra deals. Probability in fact, when so

treated, seems to be absolutely nothing else than a system of

applied Permutations and Combinations.

It will now readily be seen how narrow is the range of

cases to which any purely deductive method of treatment can

apply. It is almost entirely confined to such employments

as games of chance, and, as already pointed out, can only be

regarded as really trustworthy even there, by the help of

various tacit restrictions. This alone would be conclusive

against the theory of the subject being rested upon such a

basis. The experimental method, on the other hand, is, in the

same theoretical sense, of universal application. It would

include the ordinary problems furnished by games of chance,

as well as those where the dice are loaded and the pence are

not perfect, and also the indefinitely numerous applications

of statistics to the various kinds of social phenomena.

14. The particular view of the deductive character of

Probability above discussed, could scarcely have intruded

itself into any other examples than those of the nature of

games of chance, in which the conditions of occurrence are

by comparison few and simple, and are amenable to accurate

numerical determination. But a doctrine, which is in reality

little else than the same theory in a slightly disguised form,

is very prevalent, and has been applied to truths of the most

purely empirical character. This doctrine will be best introduced

by a quotation from Laplace. After speaking of

the irregularity and uncertainty of nature as it appears at

first sight, he goes on to remark that when we look closer we

begin to detect "a striking regularity which seems to suggest

a design, and which some have considered a proof of

Providence. But, on reflection, it is soon perceived that this

regularity is nothing but the development of the respective

probabilities of the simple events, which ought to occur more

frequently according as they are more probable."[6]

If this remark had been made about the succession of

heads and tails in the throwing up of a penny, it would

have been intelligible. It would simply mean this: that the

constitution of the body was such that we could anticipate

with some confidence what the result would be when it was

treated in a certain way, and that experience would justify

our anticipation in the long run. But applied as it is in a

more general form to the facts of nature, it seems really to

have but little meaning in it. Let us test it by an instance.

Amidst the irregularity of individual births, we find that the

male children are to the female, in the long run, in about

the proportion of 106 to 100. Now if we were told that

there is nothing in this but "the development of their respective

probabilities," would there be anything in such a

statement but a somewhat pretentious re-statement of the

fact already asserted? The probability \_is\_ nothing but that

proportion, and is unquestionably in this case derived from

no other source but the statistics themselves; in the above

remark the attempt seems to be made to invert this process,

and to derive the sequence of events from the mere numerical

statement of the proportions in which they occur.

15. It will very likely be replied that by the probability

above mentioned is meant, not the mere numerical

proportion between, the births, but some fact in our constitution

upon which this proportion depends; that just as

there was a relation of equality between the two sides of the

penny, which produced the ultimate equality in the number

of heads and tails, so there may be something in our constitution

or circumstances in the proportion of 106 to 100,

which produces the observed statistical result. When this

something, whatever it might be, was discovered, the observed

numbers might be supposed capable of being determined

beforehand. Even if this were the case, however, it

must not be forgotten that there could hardly fail to be, in

combination with such causes, other concurrent conditions in

order to produce the ultimate result; just as besides the shape

of the penny, we had also to take into account the nature of

the 'randomness' with which it was tossed. What these

may be, no one at present can undertake to say, for the best

physiologists seem indisposed to hazard even a guess upon

the subject.[7] But without going into particulars, one may

assert with some confidence that these conditions cannot well

be altogether independent of the health, circumstances, manners

and customs, &c. (to express oneself in the vaguest way)

of the parents; and if once these influencing elements are

introduced, even as very minute factors, the results cease to

be dependent only on fixed and permanent conditions. We

are at once letting in other conditions, which, if they also

possess the characteristics that distinguish Probability (an

exceedingly questionable assumption), must have that fact

specially proved about them. That this should be the case

indeed seems not merely questionable, but almost certainly

impossible; for these conditions partaking of the nature of

what we term generally, Progress and Civilization, cannot be

expected to show any permanent disposition to hover about

an average.

16. The reader who is familiar with Probability is of

course acquainted with the celebrated theorem of James

Bernoulli. This theorem, of which the examples just adduced

are merely particular cases, is generally expressed

somewhat as follows:--in the long run all events will tend

to occur with a relative frequency proportional to their

objective probabilities. With the mathematical proof of this

theorem we need not trouble ourselves, as it lies outside the

province of this work; but indeed if there is any value in the

foregoing criticism, the basis on which the mathematics rest

is faulty, owing to there being really nothing which we can

with propriety call an objective probability.

If one might judge by the interpretation and uses to

which this theorem is sometimes exposed, we should regard

it as one of the last remaining relics of Realism, which after

being banished elsewhere still manages to linger in the remote

province of Probability. It would be an illustration of

the inveterate tendency to objectify our conceptions, even in

cases where the conceptions had no right to exist at all. A

uniformity is observed; sometimes, as in games of chance, it

is found to be so connected with the physical constitution of

the bodies employed as to be capable of being inferred beforehand;

though even here the connection is by no means

so necessary as is commonly supposed, owing to the fact that

in addition to these bodies themselves we have also to take

into account their relation to the agencies which influence

them. This constitution is then converted into an 'objective

probability', supposed to develop into the sequence which

exhibits the uniformity. Finally, this very questionable objective

probability is assumed to exist, with the same faculty

of development, in all the cases in which uniformity is observed,

however little resemblance there may be between

these and games of chance.

17. How utterly inappropriate any such conception is

in most of the cases in which we find statistical uniformity,

will be obvious on a moment's consideration. The observed

phenomena are generally the product, in these cases, of very

numerous and complicated antecedents. The number of

crimes, for instance, annually committed in any society, is a

function amongst other things, of the strictness of the law,

the morality of the people, their social condition, and the

vigilance of the police, each of these elements being in itself

almost infinitely complex. Now, as a result of all these

agencies, there is some degree of uniformity; but what has

been called above the change of type, which it sooner or

later tends to display, is unmistakeable. The average annual

numbers do not show a steady gradual approach towards

what might be considered in some sense a limiting value,

but, on the contrary, fluctuate in a way which, however it

may depend upon causes, shows none of the permanent uniformity

which is characteristic of games of chance. This

fact, combined with the obvious arbitrariness of singling out,

from amongst the many and various antecedents which produced

the observed regularity, a few only, which should constitute

the objective probability (if we took all, the events

being absolutely determined, there would be no occasion for

an appeal to probability in the case), would have been sufficient

to prevent any one from assuming the existence of

any such thing, unless the mistaken analogy of other cases

had predisposed him to seek for it.

There is a familiar practical form of the same error, the

tendency to which may not improbably be derived from a

similar theoretical source. It is that of continuing to accumulate

our statistical data to an excessive extent. If the

type were absolutely fixed we could not possibly have too

many statistics; the longer we chose to take the trouble

of collecting them the more accurate our results would be.

But if the type is changing, in other words, if some of the

principal causes which aid in their production have, in regard

to their present degree of intensity, strict limits of time or

space, we shall do harm rather than good if we overstep

these limits. The danger of stopping too soon is easily seen,

but in avoiding it we must not fall into the opposite error of

going on too long, and so getting either gradually or suddenly

under the influence of a changed set of circumstances.

18. This chapter was intended to be devoted to a

consideration, not of the processes by which nature produces

the series with which we are concerned, but of the theoretic

basis of the methods by which we can determine the existence

of such series. But it is not possible to keep the two enquiries

apart, for here, at any rate, the old maxim prevails that to

know a thing we must know its causes. Recur for a minute

to the considerations of the last chapter. We there saw

that there was a large class of events, the conditions of production

of which could be said to consist of (1) a comparatively

few nearly unchangeable elements, and (2) a vast number

of independent and very changeable elements. At least if

there were any other elements besides these, we are assumed

either to make special allowance for them, or to omit them

from our enquiry. Now in certain cases, such as games of

chance, the unchangeable elements may without practical

error be regarded as really unchangeable throughout any

range of time and space. Hence, as a result, the deductive

method of treatment becomes in their case at once the most

simple, natural, and conclusive; but, as a further consequence,

the statistics of the events, if we choose to appeal to them,

may be collected \_ad libitum\_ with better and better approximation

to truth. On the other hand, in all social

applications of Probability, the unchangeable causes can only

be regarded as really unchangeable under many qualifications.

We know little or nothing of them directly; they are

often in reality numerous, indeterminate, and fluctuating;

and it is only under the guarantee of stringent restrictions of

time and place, that we can with any safety attribute to

them sufficient fixity to justify our theory. Hence, as a

result, the deductive method, under whatever name it may

go, becomes totally inapplicable both in theory and practice;

and, as a further consequence, the appeal to statistics has to

be made with the caution in mind that we shall do mischief

rather than good if we go on collecting too many of them.

19. The results of the last two chapters may be

summed up as follows:--We have extended the conception

of a series obtained in the first chapter; for we have found

that these series are mostly presented to us in groups.

These groups are found upon examination to be formed upon

approximately the same type throughout a very wide and

varied range of experience; the causes of this agreement we

discussed and explained in some detail. When, however, we

extend our examination by supposing the series to run to a

very great length, we find that they may be divided into two

classes separated by important distinctions. In one of these

classes (that containing the results of games of chance) the

conditions of production, and consequently the laws of statistical

occurrence, may be practically regarded as absolutely

fixed; and the extent of the divergences from the mean

seem to know no finite limit. In the other class, on the

contrary (containing the bulk of ordinary statistical enquiries),

the conditions of production vary with more or less rapidity,

and so in consequence do the results. Moreover it is often

impossible that variations from the mean should exceed a

certain amount. The former we may term \_ideal\_ series. It

is they alone which show the requisite characteristics with

any close approach to accuracy, and to make the theory of

the subject tenable, we have really to substitute one of this

kind for one of the less perfect ones of the other class, when

these latter are under treatment. The former class have,

however, been too exclusively considered by writers on the

subject; and conceptions appropriate only to them, and not

always even to them, have been imported into the other

class. It is in this way that a general tendency to an excessive

deductive or \_à priori\_ treatment of the science has

been encouraged.

1. This latter enquiry belongs to what may be termed the more purely

logical part of this volume, and is entered on in the course of

Chapter VI.

2. For the use of those not acquainted with the common notation

employed in this subject, it may be remarked that HH is simply an

abbreviated way of saying that the two successive throws of the

penny give head; HT that the first of them gives head, and the

second tail; and so on with the remaining symbols.

3. I am endeavouring to treat this rule of Sufficient Reason in a way

that shall be legitimate in the opinion of those who accept it, but

there seem very great doubts whether a contradiction is not involved

when we attempt to extract results from it. If the sides are

absolutely alike, how can there he any difference between the terms

of the series? The succession seems then reduced to a dull

uniformity, a mere iteration of the same thing many times; the

series we contemplated has disappeared. If the sides are not

absolutely alike, what becomes of the applicability of the rule?

4. \_Formal Logic\_, p. 185. \_Principles of Science\_, p. 208.

5. The close connection between these subjects is well indicated in

the title of Mr Whitworth's treatise, \_Choice and Chance\_.

6. \_Essai Philosophique\_. Ed. 1825, p. 74.

7. An opinion prevailed rather at one time (quoted and supported by

Quetelet amongst others) that the relative ages of the parents had

something to do with the sex of the offspring. If this were so, it

would quite bear out the above remarks. As a matter of fact, it

should be observed, that the proportion of 106 to 100 does not seem

by any means universal in all countries or at all times. For various

statistical tables on the subject see Quetelet, \_Physique Sociale\_,

Vol. I. 166, 173, 238.

CHAPTER V.

\_THE CONCEPTION \_RANDOMNESS\_ AND ITS SCIENTIFIC TREATMENT.\_

1. There is a term of frequent occurrence in treatises

on Probability, and which we have already had repeated occasion

to employ, viz. the designation \_random\_ applied to an

event, as in the expression 'a random distribution'. The

scientific conception involved in the correct use of this term

is, I apprehend, nothing more than that of aggregate order

and individual irregularity (or apparent irregularity), which

has been already described in the preceding chapters. A

brief discussion of the requisites in this scientific conception,

and in particular of the nature and some of the reasons for

the departure from the popular conception, may serve to

clear up some of the principal remaining difficulties which

attend this part of our subject.

The original,[1] and still popular, signification of the term

is of course widely different from the scientific. What it

looks to is the origin, not the results, of the random performance,

and it has reference rather to the single action

than to a group or series of actions. Thus, when a man

draws a bow 'at a venture', or 'at random', we mean only

to point out the aimless character of the performance;

we are contrasting it with the definite intention to hit a

certain mark. But it is none the less true, as already

pointed out, that we can only apply processes of inference to

such performances as these when we regard them as being

capable of frequent, or rather of indefinitely extended repetition.

Begin with an illustration. Perhaps the best typical

example that we can give of the scientific meaning of

random distribution is afforded by the arrangement of the

drops of rain in a shower. No one can give a guess whereabouts

at any instant a drop will fall, but we know that if

we put out a sheet of paper it will gradually become uniformly

spotted over; and that if we were to mark out any

two equal areas on the paper these would gradually tend

to be struck equally often.

2. I. Any attempt to draw inferences from the assumption

of random arrangement must postulate the occurrence

of this particular state of things at some stage or

other. But there is often considerable difficulty, leading occasionally

to some arbitrariness, in deciding the particular

stage at which it ought to be introduced.

(1) Thus, in many of the problems discussed by mathematicians,

we look as entirely to the results obtained, and

think as little of the actual process by which they are obtained,

as when we are regarding the arrangement of the drops of

rain. A simple example of this kind would be the following.

A pawn, diameter of base one inch, is placed at random on

a chess-board, the diameter of the squares of which is one inch

and a quarter: find the chance that its base shall lie across

one of the intersecting lines. Here we may imagine the

pawns to be so to say rained down vertically upon the board,

and the question is to find the ultimate proportion of those

which meet a boundary line to the total of those which fall.

The problem therefore becomes a merely geometrical one,

viz. to determine the ratio of a certain area on the board to

the whole area. The determination of this ratio is all that

the mathematician ever takes into account.

Now take the following. A straight brittle rod is broken

at random in two places: find the chance that the pieces can

make a triangle.[2] Since the only condition for making a

triangle with three straight lines is that each two shall be

greater than the third, the problem seems to involve the

same general conception as in the former case. We must

conceive such rods breaking at one pair of spots after

another,--no one can tell precisely where,--but showing the

same ultimate tendency to distribute these spots throughout

the whole length uniformly. As in the last case, the mathematician

thinks of nothing but this final result, and pays no

heed to the process by which it may be brought about. Accordingly

the problem is again reduced to one of mensuration,

though of a somewhat more complicated character.

3. (2) In another class of cases we have to contemplate

an intermediate process rather than a final result; but the

same conception has to be introduced here, though it is now

applied to the former stage, and in consequence will not in

general apply to the latter.

For instance: a shot is fired at random from a gun whose

maximum range (i.e. at 45° elevation) is 3000 yards: what is

the chance that the actual range shall exceed 2000 yards?

The ultimately uniform (or random) distribution here is

commonly assumed to apply to the various directions in

which the gun can be pointed; all possible directions above

the horizontal being equally represented in the long run.

We have therefore to contemplate a surface of uniform distribution,

but it will be the surface, not of the ground, but

of a hemisphere whose centre is occupied by the man who

fires. The ultimate distribution of the bullets on the spots

where they strike the ground will not be uniform. The

problem is in fact to discover the law of variation of the

density of distribution.

The above is, I presume, the treatment generally adopted

in solving such a problem. But there seems no absolute

necessity for any such particular choice. It is surely open to

any one to maintain[3] that his conception of the randomness of

the firing is assigned by saying that it is likely that a man

should begin by facing towards any point of the compass indifferently,

and then proceed to raise his gun to any angle

indifferently. The stage of ultimately uniform distribution

here has receded a step further back. It is not assigned

directly to the surface of an imaginary hemisphere, but to

the lines of altitude and azimuth drawn on that surface.

Accordingly, the distribution over the hemisphere itself will

not now be uniform,--there will be a comparative crowding

up towards the pole,--and the ultimate distribution over the

ground will not be the same as before.

4. Difficulties of this kind, arising out of the uncertainty

as to what stage should be selected for that of uniform

distribution, will occasionally present themselves. For

instance: let a book be taken at random out of a bookcase;

what is the chance of hitting upon some assigned volume?

I hardly know how this question would commonly be treated.

If we were to set our man opposite the middle of the shelf

and inquire what would generally happen in practice, supposing

him blindfolded, there cannot be much doubt that

the volumes would not be selected equally often. On the

contrary, it is likely that there would be a tendency to increased

frequency about a centre indicated by the height

of his shoulder, and (unless he be left-handed) a trifle to the

right of the point exactly opposite his starting point.

If the question were one which it were really worth

while to work out on these lines we should be led a long

way back. Just as we imagined our rifleman's position (on

the second supposition) to be determined by two independent

coordinates of assumed continuous and equal facility,

so we might conceive our making the attempt to analyse the

man's movements into a certain number of independent

constituents. We might suppose all the various directions

from his starting point, along the ground, to be equally

likely; and that when he reaches the shelves the random

motion of his hand is to be regulated after the fashion of a

shot discharged at random.

The above would be one way of setting about the statement

of the problem. But the reader will understand that

all which I am here proposing to maintain is that in these,

as in every similar case, we always encounter, under this

conception of 'randomness', at some stage or other, this

postulate of ultimate uniformity of distribution over some

assigned magnitude: either time; or space, linear, superficial,

or solid. But the selection of the stage at which this is to

be applied may give rise to considerable difficulty, and even

arbitrariness of choice.

5. Some years ago there was a very interesting discussion

upon this subject carried on in the mathematical part of

the \_Educational Times\_ (see, especially, Vol. VII.). As not

unfrequently happens in mathematics there was an almost

entire accord amongst the various writers as to the assumptions

practically to be made in any particular case, and therefore

as to the conclusion to be drawn, combined with a very

considerable amount of difference as to the axioms and definitions

to be employed. Thus Mr M. W. Crofton, with the

substantial agreement of Mr Woolhouse, laid it down unhesitatingly

that "at random" has "a very clear and definite

meaning; one which cannot be better conveyed than by Mr

Wilson's definition, '\_according to no law\_'; and in this sense

alone I mean to use it." According to any scientific interpretation

of 'law' I should have said that where there was

no law there could be no inference. But ultimate tendency

towards equality of distribution is as much taken for granted

by Mr Crofton as by any one else: in fact he makes this a

deduction from his definition:--"As this infinite system of

parallels are drawn according to no law, they are as thickly

disposed along any part of the [common] perpendicular as

along any other" (VII. p. 85). Mr Crofton holds that any

kind of \_unequal\_ distribution would imply law,--"If the points

[on a plane] tended to become denser in any part of the plane

than in another, there must be some law attracting them

there" (ib. p. 84). The same view is enforced in his paper

on \_Local Probability\_ (in the \_Phil. Trans.\_, Vol. 158). Surely

if they tend to become \_equally\_ dense this is just as much

a case of regularity or law.

It may be remarked that wherever any serious practical

consequences turn upon duly securing the desired randomness,

it is always so contrived that no design or awkwardness

or unconscious one-sidedness shall disturb the result. The

principal case in point here is of course afforded by games of

chance. What we want, when we toss a die, is to secure that

all numbers from 1 to 6 shall be equally often represented in

the long run, but that no person shall be able to predict the

individual occurrence. We might, in our statement of a

problem, as easily postulate 'a number \_thought of\_ at random'

as 'a shot fired at random', but no one would risk his chances

of gain and loss on the supposition that this would be done

with continued fairness. Accordingly, we construct a die

whose sides are accurately alike, and it is found that we may

do almost what we like with this, at any previous stage to

that of its issue from the dice box on to the table, without

interfering with the random nature of the result.

6. II. Another characteristic in which the scientific

conception seems to me to depart from the popular or original

signification is the following. The area of distribution which

we take into account must be a finite or limited one. The

necessity for this restriction may not be obvious at first sight,

but the consideration of one or two examples will serve to

indicate the point at which it makes itself felt. Suppose

that one were asked to choose a number at random, not

from a finite range, but from the inexhaustible possibilities

of enumeration. In the popular sense of the term,--i.e. of

uttering a number without pausing to choose,--there is no

difficulty. But a moment's consideration will show that no

arrangement even tending towards ultimately uniform distribution

can be secured in this way. No \_average\_ could be

struck with ever increasing steadiness. So with spatial

infinity. We can rationally speak of choosing a point at

random in a given straight line, area, or volume. But if we

suppose the line to have no end, or the selection to be made

in infinite space, the basis of ultimate tendency towards

what may be called the equally thick deposit of our random

points fails us utterly.

Similarly in any other example in which one of the

magnitudes is unlimited. Suppose I fling a stick at random

in a horizontal plane against a row of iron railings and

inquire for the chance of its passing through without touching

them. The problem bears some analogy to that of the

chessmen, and so far as the motion of translation of the

stick is concerned (if we begin with this) it presents no

difficulty. But as regards the rotation it is otherwise. For

any assigned linear velocity there is a certain angular velocity

\_below\_ which the stick may pass through without contact,

but \_above\_ which it cannot. And inasmuch as the former

range is limited and the latter is unlimited, we encounter

the same impossibility as before in endeavouring to conceive

a uniform distribution. Of course we might evade this

particular difficulty by beginning with an estimate of the

angular velocity, when we should have to repeat what has

just been said, mutatis mutandis, in reference to the linear

velocity.

7. I am of course aware that there are a variety of

problems current which seem to conflict with what has just

been said, but they will all submit to explanation. For instance;

What is the chance that three straight lines, taken

or drawn at random, shall be of such lengths as will admit of

their forming a triangle? There are two ways in which we

may regard the problem. We may, for one thing, start with

the assumption of three lines not greater than a certain

length n, and then determine towards what limit the chance

tends as n increases unceasingly. Or, we may maintain that

the question is merely one of \_relative proportion\_ of the three

lines. We may then start with any magnitude we please to

represent one of the lines (for simplicity, say, the longest of

them), and consider that all possible shapes of a triangle

will be represented by varying the lengths of the other two.

In either case we get a definite result without need to make

an attempt to conceive any random selection from the infinity

of possible length.

So in what is called the "three-point problem":--Three

points in space are selected at random; find the chance of

their forming an acute-angled triangle. What is done is to

start with a closed volume,--say a sphere, from its superior

simplicity,--find the chance (on the assumption of uniform

distribution within this volume); and then conceive the continual

enlargement without limit of this sphere. So regarded

the problem is perfectly consistent and intelligible, though I

fail to see why it should be termed a random selection \_in

space\_ rather than in a sphere. Of course if we started with

a different volume, say a cube, we should get a different

result; and it is therefore contended (e.g. by Mr Crofton in

the \_Educational Times\_, as already referred to) that infinite

space is more naturally and appropriately regarded as tended

towards by the enlargement of a sphere than by that of a

cube or any other figure.

Again: A group of integers is taken at random; show

that the number thus taken is more likely to be odd than

even. What we do in answering this is to start with any

finite number n, and show that of all the possible combinations

which can be made within this range there are

more odd than even. Since this is true irrespective of the

magnitude of n, we are apt to speak as if we could conceive

the selection being made at random from the true infinity

contemplated in numeration.

8. Where these conditions cannot be secured then it

seems to me that the attempt to assign any finite value to

the probability fails. For instance, in the following problem,

proposed by Mr J. M. Wilson, "Three straight lines are

drawn at random on an infinite plane, and a fourth line is

drawn at random to intersect them: find the probability of

its passing through the triangle formed by the other three"

(\_Ed. Times\_, Reprint, Vol. V. p. 82), he offers the following

solution: "Of the four lines, two must and two must not pass

within the triangle formed by the remaining three. Since

all are drawn at random, the chance that the last drawn

should pass through the triangle formed by the other three

is consequently 1/2."

I quote this solution because it seems to me to illustrate

the difficulty to which I want to call attention. As the

problem is worded, a triangle is supposed to be assigned by

three straight lines. However large it may be, its size bears

no finite ratio whatever to the indefinitely larger area outside

it; and, so far as I can put any intelligible construction

on the supposition, the chance of drawing a fourth random

line which should happen to intersect this finite area must

be reckoned as zero. The problem Mr Wilson has solved

seems to me to be a quite different one, viz. "Given four

intersecting straight lines, find the chance that we should,

at random, select one that passes through the triangle formed

by the other three."

The same difficulty seems to me to turn up in most other

attempts to apply this conception of randomness to real

infinity. The following seems an exact analogue of the

above problem:--A number is selected at random, find the

chance that another number selected at random shall be

greater than the former;--the answer surely must be that

the chance is unity, viz. certainty, because the range above

any assigned number is infinitely greater than that below it.

Or, expressed in the only language in which I can understand

the term 'infinity', what I mean is this. If the first

number be m and I am restricted to selecting up to n

(n > m) then the chance of exceeding m is n - m : n; if I

am restricted to 2n then it is 2n - m : 2n and so on. That

is, however large n and m may be the expression is always

intelligible; but, \_m being chosen first\_, n may be made as

much larger than m as we please: i.e. the chance may be

made to approach as near to unity as we please.

I cannot but think that there is a similar fallacy in De Morgan's

admirably suggestive paper on \_Infinity\_ (\_Camb.

Phil. Trans.\_ Vol. 11.) when he is discussing the "three-point

problem":--i.e. given three points taken at random find the

chance that they shall form an acute-angled triangle. All

that he shows is, that if we start with \_one side as given\_ and

consider the subsequent possible positions of the opposite

vertex, there are infinitely as many such positions which

would form an acute-angled triangle as an obtuse: but, as

before, this is solving a different problem.

9. The nearest approach I can make towards true indefinite

randomness, or random selection from true indefiniteness,

is as follows. Suppose a circle with a tangent line extended

indefinitely in each direction. Now from the centre draw

radii at random; in other words, let the semicircumference

which lies towards the tangent be ultimately uniformly intersected

by the radii. Let these radii be then produced so as

to intersect the tangent line, and consider the distribution

of these points of intersection. We shall obtain in the result

\_one\_ characteristic of our random distribution; i.e. no portion

of this tangent, however small or however remote, but will

find itself in the position ultimately of any small portion of

the pavement in our supposed continual rainfall. That is,

any such elementary patch will become more and more closely

dotted over with the points of intersection. But the other

essential characteristic, viz. that of ultimately \_uniform\_ distribution,

will be missing. There will be a special form of

distribution,--what in fact will have to be discussed in a

future chapter under the designation of a 'law of error',--by

virtue of which the concentration will tend to be greatest at

a certain point (that of contact with the circle), and will thin

out from here in each direction according to an easily

calculated formula. The existence of such a state of things

as this is quite opposed to the conception of true randomness.

10. III. Apart from definitions and what comes of

them, perhaps the most important question connected with

the conception of Randomness is this: How in any given

case are we to determine whether an observed arrangement

is to be considered a random one or not? This question will

have to be more fully discussed in a future chapter, but we

are already in a position to see our way through some of the

difficulties involved in it.

(1) If the events or objects under consideration are supposed

to be continued indefinitely, or if we know enough

about the mode in which they are brought about to detect

their ultimate tendency,--or even, short of this, if they are

numerous enough to be beyond practical counting,--there is

no great difficulty. We are simply confronted with a question

of fact, to be settled like other questions of fact. In the

case of the rain-drops, watch two equal squares of pavement

or other surfaces, and note whether they come to be more

and more densely uniformly and evenly spotted over: if they

do, then the arrangement is what we call a random one. If

I want to know whether a tobacco-pipe really breaks at random,

and would therefore serve as an illustration of the

problem proposed some pages back, I have only to drop

enough of them and see whether pieces of all possible lengths

are equally represented in the long run. Or, I may argue

deductively, from what I know about the strength of materials

and the molecular constitution of such bodies, as to

whether fractures of small and large pieces are all equally

likely to occur.

11. The reader's attention must be carefully directed

to a source of confusion here, arising out of a certain cross-division.

What we are now discussing is a question of fact,

viz. the nature of a certain ultimate arrangement; we are

not discussing the particular way in which it is brought

about. In other words, the antithesis is between what is and

what is not random: it is not between what is random and

what is designed. As we shall see in a few moments it is

quite possible that an arrangement which is the result,--if

ever anything were so,--of 'design', may nevertheless present

the unmistakeable stamp of randomness of arrangement.

Consider a case which has been a good deal discussed,

and to which we shall revert again: the arrangement of the

stars. The question here is rather complicated by the fact

that we know nothing about the actual mutual positions of

the stars, all that we can take cognizance of being their apparent

or visible places as projected upon the surface of a

supposed sphere. Appealing to what alone we can thus

observe, it is obvious that the arrangement, as a whole, is

not of the random sort. The Milky Way and the other resolvable

nebulæ, as they present themselves to us, are as obvious

an infraction of such an arrangement as would be the

occurrence here and there of patches of ground in a rainfall

which received a vast number more drops than the spaces

surrounding them. If we leave these exceptional areas out

of the question and consider only the stars which are visible

by the naked eye or by slight telescopic power, it seems

equally certain that the arrangement \_is\_, for the most part, a

fairly representative random one. By this we mean nothing

more than the fact that when we mark off any number of

equal areas on the visible sphere these are found to contain

approximately the same number of stars.

The actual arrangement of the stars in space \_may\_ also

be of the same character: that is, the apparently denser

aggregation may be apparent only, arising from the fact that

we are looking through regions which are not more thickly

occupied but are merely more extensive. The alternative

before us, in fact, is this. If the whole volume, so to say, of

the starry heavens is tolerably regular in shape, then the

arrangement of the stars is not of the random order; if that

volume is very irregular in shape, it is possible that the arrangement

within it may be throughout of that order.

12. (2) When the arrangement in question includes

but a comparatively small number of events or objects, it

becomes much more difficult to determine whether or not it

is to be designated a random one. In fact we have to shift

our ground, and to decide not by what has been actually

observed but by what we have reason to conclude would be

observed if we could continue our observation much longer.

This introduces what is called 'Inverse Probability', viz. the

determination of the nature of a cause from the nature of the

observed effect; a question which will be fully discussed in

a future chapter. But some introductory remarks may be

conveniently made here.

Every problem of Probability, as the subject is here understood,

introduces the conception of an ultimate limit, and

therefore presupposes an indefinite possibility of repetition.

When we have only a finite number of occurrences before us,

\_direct\_ evidence of the character of their arrangement fails us,

and we have to fall back upon the nature of the agency

which produces them. And as the number becomes smaller

the confidence with which we can estimate the nature of the

agency becomes gradually less.

Begin with an intermediate case. There is a small lawn,

sprinkled over with daisies: is this a random arrangement?

We feel some confidence that it is so, on mere inspection;

meaning by this that (negatively) no trace of any regular

pattern can be discerned and (affirmatively) that if we take

any moderately small area, say a square yard, we shall find

much about the same number of the plants included in it.

But we can help ourselves by an appeal to the known agency

of distribution here. We know that the daisy spreads by

seed, and considering the effect of the wind and the continued

sweeping and mowing of the lawn we can detect causes at

work which are analogous to those by which the dealing of

cards and the tossing of dice are regulated.

In the above case the appeal to the process of production

was subsidiary, but when we come to consider the nature of

a very small succession or group this appeal becomes much

more important. Let us be told of a certain succession of

'heads' and 'tails' to the number of ten. The range here is

far too small for decision, and unless we are told whether the

agent who obtained them was tossing or designing we are

quite unable to say whether or not the designation of 'random'

ought to be applied to the result obtained. The truth

must never be forgotten that though 'design' is sure to

break down in the long run if it make the attempt to produce

directly the semblance of randomness,[4] yet for a short

spell it can simulate it perfectly. Any short succession, say

of heads and tails, may have been equally well brought

about by tossing or by deliberate choice.

13. The reader will observe that this question of

randomness is being here treated as simply one of ultimate

statistical fact. I have fully admitted that this is not the

primitive conception, nor is it the popular interpretation,

but to adopt it seems the only course open to us if we are to

draw inferences such as those contemplated in Probability.

When we look to the producing agency of the ultimate

arrangement we may find this very various. It may prove

itself to be (a few stages back) one of conscious deliberate

purpose, as in drawing a card or tossing a die: it may be

the outcome of an extremely complicated interaction of many

natural causes, as in the arrangement of the flowers scattered

over a lawn or meadow: it may be of a kind of which we

know literally nothing whatever, as in the case of the actual

arrangement of the stars relatively to each other.

This was the state of things had in view when it was

said a few pages back that randomness and design would

result in something of a cross-division. Plenty of arrangements

in which design had a hand, a stage or two back, can

be mentioned, which would be quite indistinguishable in

their results from those in which no design whatever could

be traced. Perhaps the most striking case in point here is

to be found in the arrangement of the digits in one of the

natural arithmetical constants, such as π or e, or in a table

of logarithms. If we look to the process of production of

these digits, no extremer instance can be found of what we

mean by the antithesis of randomness: every figure has its

necessarily pre-ordained position, and a moment's flagging of

intention would defeat the whole purpose of the calculator.

And yet, if we look to results only, no better instance can

be found than one of these rows of digits if it were intended

to illustrate what we practically understand by a chance

arrangement of a number of objects. Each digit occurs

approximately equally often, and this tendency develops as

we advance further: the mutual juxtaposition of the digits

also shows the same tendency, that is, any digit (say 5) is

just as often followed by 6 or 7 as by any of the others. In

fact, if we were to take the whole row of hitherto calculated

figures, cut off the first five as familiar to us all, and contemplate

the rest, no one would have the slightest reason

to suppose that these had not come out as the results of a

die with ten equal faces.

14. If it be asked \_why\_ this is so, a rather puzzling

question is raised. Wherever physical causation is involved

we are generally understood to have satisfied the demand

implied in this question if we assign antecedents which will

be followed regularly by the event before us; but in geometry

and arithmetic there is no opening for antecedents. What

we then commonly look for is a demonstration, i.e. the resolution

of the observed fact into axioms if possible, or at

any rate into admitted truths of wider generality. I do not

know that a demonstration can be given as to the existence

of this characteristic of statistical randomness in such successions

of digits as those under consideration. But the

following remarks may serve to shift the onus of unlikelihood

by suggesting that the preponderance of analogy is

rather in favour of the existence.

Take the well-known constant π for consideration. This

stands for a quantity which presents itself in a vast number

of arithmetical and geometrical relations; let us take for

examination the best known of these, by regarding it as

standing for the ratio of the circumference to the diameter

of a circle. So regarded, it is nothing more than a simple

case of the measurement of a magnitude by an arbitrarily

selected unit. Conceive then that we had before us a rod

or line and that we wished to measure it with absolute

accuracy. We must suppose--if we are to have a suitable

analogue to the determination of π to several hundred

figures,--that by the application of continued higher magnifying

power we can detect ever finer subdivisions in the

graduation. We lay our rod against the scale and find it,

say, fall between 31 and 32 inches; we then look at the

next division of the scale, viz. that into tenths of an inch.

Can we see the slightest reason why the number of these

tenths should be other than independent of the number of

whole inches? The "piece over" which we are measuring

may in fact be regarded as an entirely new piece, which had

fallen into our hands after that of 31 inches had been

measured and done with; and similarly with every successive

piece over, as we proceed to the ever finer and finer divisions.

Similar remarks may be made about most other incommensurable

quantities, such as irreducible roots. Conceive

two straight lines at right angles, and that we lay off a

certain number of inches along each of these from the point

of intersection; say two and five inches, and join the extremities

of these so as to form the diagonal of a right-angled

triangle. If we proceed to measure this diagonal in terms

of either of the other lines we are to all intents and purposes

extracting a square root. We should expect, rather than

otherwise, to find here, as in the case of π, that incommensurability

and resultant randomness of order in the digits

was the rule, and commensurability was the exception. Now

and then, as when the two sides were three and four, we

should find the diagonal commensurable with them; but

these would be the occasional exceptions, or rather they

would be the comparatively finite exceptions amidst the

indefinitely numerous cases which furnished the rule.

15. The best way perhaps of illustrating the truly

random character of such a row of figures is by appealing to

graphical aid. It is not easy here, any more than in ordinary

statistics, to grasp the import of mere figures; whereas the

arrangement of groups of points or lines is much more

readily seized. The eye is very quick in detecting any

symptoms of regularity in the arrangement, or any tendency

to denser aggregation in one direction than in another.

How then are we to dispose our figures so as to force them

to display their true character? I should suggest that we

set about \_drawing a line at random\_; and, since we cannot

trust our own unaided efforts to do this, that we rely upon

the help of such a table of figures to do it for us, and then

examine with what sort of efficiency they can perform the

task. The problem of drawing \_straight\_ lines at random,

under various limitations of direction or intersection, is

familiar enough, but I do not know that any one has suggested

the drawing of a line whose shape as well as position

shall be of a purely random character. For simplicity we

suppose the line to be confined to a plane.

The definition of such a line does not seem to involve

any particular difficulty. Phrased in accordance with the

ordinary language we should describe it as the path (i.e. any

path) traced out by a point which at every moment is as

likely to move in any one direction as in any other. That

we could not ourselves draw such a line, and that we could

not get it traced by any physical agency, is certain. The

mere inertia of any moving body will always give it a

tendency, however slight, to go on in a straight line at each

moment, instead of being instantly responsive to instantaneously

varying dictates as to its direction of motion. Nor can

we conceive or picture such a line in its ultimate or ideal condition.

But it is easy to give a graphical approximation to it,

and it is easy also to show how this approximation may be

carried on as far as we please towards the ideal in question.

We may proceed as follows. Take a sheet of the ordinary

ruled paper prepared for the graphical exposition of curves.

Select as our starting point the intersection of two of these

lines, and consider the eight 'points of the compass' indicated

by these lines and the bisections of the contained

right angles.[5] For suggesting the random selection amongst

these directions let them be numbered from 0 to 7, and

let us say that a line measured due 'north' shall be designated

by the figure 0, 'north-east' by 1, and so on. The

selection amongst these numbers, and therefore directions, at

every corner, might be handed over to a die with eight faces;

but for the purpose of the illustration in view we select the

digits 0 to 7 as they present themselves in the calculated

value of π. The sort of path along which we should

travel by a series of such steps thus taken at random

may be readily conceived; it is given at the end of this

chapter.

For the purpose with which this illustration was proposed,

viz. the graphical display of the succession of digits

in any one of the incommensurable constants of arithmetic

or geometry, the above may suffice. After actually testing

some of them in this way they seem to me, so far as the eye,

or the theoretical principles to be presently mentioned, are

any guide, to answer quite fairly to the description of randomness.

16. As we are on the subject, however, it seems worth

going farther by enquiring how near we could get to the

ideal of randomness of direction. To carry this out completely

two improvements must be made. For one thing,

instead of confining ourselves to eight directions we must

admit an infinite number. This would offer no great difficulty;

for instead of employing a small number of digits we

should merely have to use some kind of circular teetotum

which would rest indifferently in any direction. But in the

next place instead of short finite steps we must suppose them

indefinitely short. It is here that the actual unattainability

makes itself felt. We are familiar enough with the device,

employed by Newton, of passing from the discontinuous

polygon to the continuous curve. But we can resort to this

device because the ideal, viz. the curve, is as easily drawn

(and, I should say, as easily conceived or pictured) as any of

the steps which lead us towards it. But in the case before

us it is otherwise. The line in question will remain discontinuous,

or rather angular, to the last: for its angles do

not tend even to lose their sharpness, though the fragments

which compose them increase in number and diminish in

magnitude without any limit. And such an ideal is not conceivable

as an ideal. It is as if we had a rough body under

the microscope, and found that as we subjected it to higher

and higher powers there was no tendency for the angles to

round themselves off. Our 'random line' must remain as

'spiky' as ever, though the size of its spikes of course

diminishes without any limit.

The case therefore seems to be this. It is easy, in words,

to indicate the conception by speaking of a line which at

every instant is as likely to take one direction as another.

It is easy moreover to draw such a line with any degree

of minuteness which we choose to demand. But it is not

possible to conceive or picture the line in its ultimate form.[6]

There is in fact no 'limit' here, intelligible to the understanding

or picturable by the imagination (corresponding to

the asymptote of a curve, or the continuous curve to the

incessantly developing polygon), towards which we find ourselves

continually approaching, and which therefore we are

apt to conceive ourselves as ultimately attaining. The usual

assumption therefore which underlies the Newtonian infinitesimal

geometry and the Differential Calculus, ceases to

apply here.

17. If we like to consider such a line in one of its

approximate stages, as above indicated, it seems to me that

some of the usual theorems of Probability, where large

numbers are concerned, may safely be applied. If it be

asked, for instance, whether such a line will ultimately tend

to stray indefinitely far from its starting point, Bernoulli's

'Law of Large Numbers' may be appealed to, in virtue of

which we should say that it was excessively unlikely that its

divergence should be relatively great. Recur to our graphical

illustration, and consider first the resultant deviation

of the point (after a great many steps) right or left of the

vertical line through the starting point. Of the eight admissible

motions at each stage two will not affect this relative

position, whilst the other six are equally likely to move us a

step to the right or to the left. Our resultant 'drift' therefore

to the right or left will be analogous to the resultant

difference between the number of heads and tails after a

great many tosses of a penny. Now the well-known outcome

of such a number of tosses is that ultimately the

\_proportional\_ approximation to the \_à priori\_ probability, i.e. to

equality of heads and tails, is more and more nearly carried

out, but that the \_absolute\_ deflection is more and more widely

displayed.

Applying this to the case in point, and remembering

that the results apply equally to the horizontal and vertical

directions, we should say that after any very great number

of such 'steps' as those contemplated, the ratio of our distance

from the starting point to the whole distance travelled

will pretty certainly be small, whereas the actual distance

from it would be large. We should also say that the longer

we continued to produce such a line the more pronounced

would these tendencies become. So far as concerns this test,

and that afforded by the general appearance of the lines

drawn,--this last, as above remarked, being tolerably trustworthy,--I

feel no doubt as to the generally 'random'

character of the rows of figures displayed by the incommensurable

or irrational ratios in question.

As it may interest the reader to see an actual specimen

of such a path I append one representing the arrangement

of the eight digits from 0 to 7 in the value of π. The data

are taken from Mr Shanks' astonishing performance in the

calculation of this constant to 707 places of figures (\_Proc. of

R. S.\_, XXI. p. 319). Of these, after omitting 8 and 9, there

remain 568; the diagram represents the course traced out

by following the direction of these as the clue to our path.

Many of the steps have of course been taken in opposite

directions twice or oftener. The result seems to me to

furnish a very fair graphical indication of randomness. I

have compared it with corresponding paths furnished by

rows of figures taken from logarithmic tables, and in other

ways, and find the results to be much the same.

[Figure: Digits of π as a path.]

1. According to Prof. Skeat (\_Etymological Dictionary\_) the earliest

known meaning is that of \_furious\_ action, as in a charge of

cavalry. The etymology, he considers, is connected with the

Teutonic word \_rand\_ (brim), and implies the furious and irregular

action of a river full to the brim.

2. See the problem paper of Jan. 18, 1854, in the Cambridge

Mathematical Tripos.

3. As, according to Mr H. Godfray, the majority of the candidates

\_did\_ assume when the problem was once proposed in an

examination. See the \_Educational Times\_ (Reprint, Vol. VII.

p. 99.)

4. Vide p. 68.

5. It would of course be more complete to take \_ten\_ alternatives of

direction, and thus to omit none of the digits; but this is much

more troublesome in practice than to confine ourselves to eight.

6. Any more than we picture the shape of an equiangular spiral \_at the

centre\_.

CHAPTER VI.[1]

\_THE SUBJECTIVE SIDE OF PROBABILITY. MEASUREMENT OF BELIEF.\_

1. Originally written in somewhat of a spirit of protest against what

seemed to me the prevalent disposition to follow De Morgan in taking

too subjective a view of the science. In reading it through now I

cannot find any single sentence to which I could take distinct

objection, though I must admit that if I were writing it entirely

afresh I should endeavour to express myself with less emphasis, and

I have made alterations in that direction. The reader who wishes to

see a view not substantially very different from mine, but expressed

with a somewhat opposite emphasis, can refer to Mr F. Y. Edgeworth's

article on "The Philosophy of Chance" (\_Mind\_, Vol. IX.)

1. Having now obtained a clear conception of a

certain kind of series, the next enquiry is, What is to be

done with this series? How is it to be employed as a means

of making inferences? The general step that we are now

about to take might be described as one from the objective

to the subjective, from the things themselves to the state of

our minds in contemplating them.

The reader should observe that a substitution has, in a

great number of cases, already been made as a first stage

towards bringing the things into a shape fit for calculation.

This substitution, as described in former chapters, is, in a

measure, a process of \_idealization\_. The series we actually

meet with are apt to show a changeable type, and the individuals

of them will sometimes transgress their licensed irregularity.

Hence they have to be pruned a little into shape, as

natural objects almost always have before they are capable of

being accurately reasoned about. The form in which the

series emerges is that of a series with a fixed type. This

imaginary or ideal series is the basis of our calculation.

2. It must not be supposed that this is at all at variance

with the assertion previously made, that Probability is a

science of inference about real things; it is only by a substitution

of the above kind that we are enabled to reason about

the things. In nature nearly all phenomena present themselves

in a form which departs from that rigorously accurate

one which scientific purposes mostly demand, so we have to

introduce an imaginary series, which shall be free from any

such defects. The only condition to be fulfilled is, that the

substitution is to be as little arbitrary, that is, to vary from

the truth as slightly, as possible. This kind of substitution

generally passes without notice when natural objects of any

kind are made subjects of exact science. I direct distinct

attention to it here simply from the apprehension that want

of familiarity with the subject-matter might lead some readers

to suppose that it involves, in this case, an exceptional deflection

from accuracy in the formal process of inference.

It may be remarked also that the adoption of this imaginary

series offers no countenance whatever to the doctrine

criticised in the last chapter, in accordance with which it was

supposed that our series possessed a fixed unchangeable type

which was merely the "development of the probabilities" of

things, to use Laplace's expression. It differs from anything

contemplated on that hypothesis by the fact that it is to be

recognized as a necessary substitution of our own for the

actual series, and to be kept in as close conformity with facts as

possible. It is a mere fiction or artifice necessarily resorted

to for the purpose of calculation, and for this purpose only.

This caution is the more necessary, because in the example

that I shall select, and which belongs to the most favourite

class of examples in this subject, the substitution becomes

accidentally unnecessary. The things, as has been repeatedly

pointed out, may sometimes need no trimming, because in

the form in which they actually present themselves they \_are\_

almost idealized. In most cases a good deal of alteration is

necessary to bring the series into shape, but in some--prominently

in the case of games of chance--we find the alterations,

for all practical purposes, needless.

3. We start then, from such a series as this, upon the

enquiry, What kind of inference can be made about it? It

may assist the logical reader to inform him that our first step

will be analogous to one class of what are commonly known

as \_immediate\_ inferences,--inferences, that is, of the type,--'All

men are mortal, therefore any particular man or men

are mortal.' This case, simple and obvious as it is in Logic,

requires very careful consideration in Probability.

It is obvious that we must be prepared to form an opinion

upon the propriety of taking the step involved in making

such an inference. Hitherto we have had as little to do as

possible with the irregular individuals; we have regarded

them simply as fragments of a regular series. But we cannot

long continue to neglect all consideration of them. Even if

these events in the gross be tolerably certain, it is not only

in the gross that we have to deal with them; they constantly

come before us a few at a time, or even as individuals,

and we have to form some opinion about them in this

state. An insurance office, for instance, deals with numbers

large enough to obviate most of the uncertainty, but

each of their transactions has another party interested in

it--What has the man who insures to say to their proceedings?

for to him this question becomes an individual one.

And even the office itself receives its cases singly, and would

therefore like to have as clear views as possible about these

single cases. Now, the remarks made in the preceding chapters

about the subjects which Probability discusses might seem to

preclude all enquiries of this kind, for was not ignorance of

the individual presupposed to such an extent that even (as

will be seen hereafter) causation might be denied, within

considerable limits, without affecting our conclusions? The

answer to this enquiry will require us to turn now to the

consideration of a totally distinct side of the question, and

one which has not yet come before us. Our best introduction

to it will be by the discussion of a special example.

4. Let a penny be tossed up a very great many

times; we may then be supposed to know for certain this

fact (amongst many others) that in the long run head and

tail will occur about equally often. But suppose we consider

only a moderate number of throws, or fewer still, and so

continue limiting the number until we come down to three

or two, or even one? We have, as the extreme cases, certainty

or something undistinguishably near it, and utter

uncertainty. Have we not, between these extremes, all

gradations of belief? There is a large body of writers, including

some of the most eminent authorities upon this

subject, who state or imply that we are distinctly conscious of

such a variation of the amount of our belief, and that this

state of our minds can be measured and determined with

almost the same accuracy as the external events to which

they refer. The principal mathematical supporter of this

view is De Morgan, who has insisted strongly upon it in all

his works on the subject. The clearest exposition of his

opinions will be found in his \_Formal Logic\_, in which work he

has made the view which we are now discussing the basis of

his system. He holds that we have a certain amount of belief

of every proposition which may be set before us, an amount

which in its nature admits of determination, though we may

practically find it difficult in any particular case to determine

it. He considers, in fact, that Probability is a sort of sister

science to Formal Logic,[1] speaking of it in the following

words: "I cannot understand why the study of the effect,

which partial belief of the premises produces with respect to

the conclusion, should be separated from that of the consequences

of supposing the former to be absolutely true."[2]

In other words, there is a science--Formal Logic--which investigates

the rules according to which one proposition can be

necessarily inferred from another; in close correspondence

with this there is a science which investigates the rules according

to which the amount of our belief of one proposition

varies with the amount of our belief of other propositions

with which it is connected.

The same view is also supported by another high authority,

the late Prof. Donkin, who says (\_Phil. Mag.\_ May, 1851), "It

will, I suppose, be generally admitted, and has often been

more or less explicitly stated, that the subject-matter of

calculation in the mathematical theory of Probabilities is

\_quantity of belief\_."

5. Before proceeding to criticise this opinion, one remark

may be made upon it which has been too frequently

overlooked. It should be borne in mind that, even were this

view of the subject not actually incorrect, it might be objected

to as insufficient for the purpose of a definition, on the ground

that variation of belief is not confined to Probability. It is

a property with which that science is concerned, no doubt,

but it is a property which meets us in other directions as

well. In every case in which we extend our inferences by

Induction or Analogy, or depend upon the witness of others,

or trust to our own memory of the past, or come to a conclusion

through conflicting arguments, or even make a long and

complicated deduction by mathematics or logic, we have a

result of which we can scarcely feel as certain as of the premises

from which it was obtained. In all these cases then

we are conscious of varying quantities of belief, but are the

laws according to which the belief is produced and varied the

same? If they cannot be reduced to one harmonious scheme,

if in fact they can at best be brought to nothing but a number

of different schemes, each with its own body of laws and rules,

then it is vain to endeavour to force them into one science.

This opinion is strengthened by observing that most of

the writers who adopt the definition in question do practically

dismiss from consideration most of the above-mentioned

examples of diminution of belief, and confine their attention

to classes of events which have the property discussed in

Chap I., viz. 'ignorance of the few, knowledge of the many.'

It is quite true that considerable violence has to be done to

some of these examples, by introducing exceedingly arbitrary

suppositions into them, before they can be forced to assume

a suitable form. But still there is little doubt that, if we

carefully examine the language employed, we shall find that

in almost every case assumptions are made which virtually

imply that our knowledge of the individual is derived from

propositions given in the typical form described in Chap I.

This will be more fully proved when we come to consider

some common misapplications of the science.

6. Even then, if the above-mentioned view of the

subject were correct, it would yet, I consider, be insufficient

for the purpose of a definition; but it is at least very doubtful

whether it is correct. Before we could properly assign to

the belief side of the question the prominence given to it by

De Morgan and others, certainly before the science could be

defined from that side, it would be necessary, it appears, to

establish the two following positions, against both of which

strong objections can be brought.

(1) That our belief of every proposition is a thing which

we can, strictly speaking, be said to measure; that

there must be a certain amount of it in every case,

which we can realize somehow in consciousness and

refer to some standard so as to pronounce upon its

value.

(2) That the value thus apprehended is the correct one

according to the theory, viz. that it is the exact

fraction of full conviction that it should be. This

statement will perhaps seem somewhat obscure at

first; it will be explained presently.

7. (I.) Now, in the first place, as regards the difficulty

of obtaining any measure of the amount of our belief. One

source of this difficulty is too obvious to have escaped notice;

this is the disturbing influence produced on the quantity of

belief by any strong emotion or passion. A deep interest in

the matter at stake, whether it excite hope or fear, plays great

havoc with the belief-meter, so that we must assume the

mind to be quite unimpassioned in weighing the evidence.

This is noticed and acknowledged by Laplace and others;

but these writers seem to me to assume it to be the only

source of error, and also to be of comparative unimportance.

Even if it were the only source of error I cannot see that it

would be unimportant. We experience hope or fear in so

very many instances, that to omit such influences from consideration

would be almost equivalent to saying that whilst

we profess to consider the whole quantity of our belief we

will in reality consider only a portion of it. Very strong

feelings are, of course, exceptional, but we should nevertheless

find that the emotional element, in some form or other,

makes itself felt on almost every occasion. It is very seldom

that we cannot speak of our surprise or expectation in reference

to any particular event. Both of these expressions, but

especially the former, seem to point to something more than

mere belief. It is true that the word 'expectation' is generally

defined in treatises on Probability as equivalent to

belief; but it seems doubtful whether any one who attends

to the popular use of the terms would admit that they were

exactly synonymous. Be this however as it may, the emotional

element is present upon almost every occasion, and its

disturbing influence therefore is constantly at work.

8. Another cause, which co-operates with the former,

is to be found in the extreme complexity and variety of the

evidence on which our belief of any proposition depends.

Hence it results that our actual belief at any given moment

is one of the most fugitive and variable things possible, so

that we can scarcely ever get sufficiently clear hold of it to

measure it. This is not confined to the times when our

minds are in a turmoil of excitement through hope or fear.

In our calmest moments we shall find it no easy thing to

give a precise answer to the question, How firmly do I hold

this or that belief? There may be one or two prominent

arguments in its favour, and one or two corresponding objections

against it, but this is far from comprising all the

causes by which our state of belief is produced. Because

such reasons as these are all that can be practically introduced

into oral or written controversies, we must not conclude

that it is by these only that our conviction is influenced.

On the contrary, our conviction generally rests upon a sort

of chaotic basis composed of an infinite number of inferences

and analogies of every description, and these moreover distorted

by our state of feeling at the time, dimmed by the

degree of our recollection of them afterwards, and probably

received from time to time with varying force according to

the way in which they happen to combine in our consciousness

at the moment. To borrow a striking illustration from

Abraham Tucker, the substructure of our convictions is not

so much to be compared to the solid foundations of an ordinary

building, as to the piles of the houses of Rotterdam

which rest somehow in a deep bed of soft mud. They bear

their weight securely enough, but it would not be easy to

point out accurately the dependence of the different parts

upon one another. Directly we begin to think of the amount

of our belief, we have to think of the arguments by which it

is produced--in fact, these arguments will intrude themselves

without our choice. As each in turn flashes through the

mind, it modifies the strength of our conviction; we are like

a person listening to the confused hubbub of a crowd, where

there is always something arbitrary in the particular sound

we choose to listen to. There may be reasons enough to

suffice abundantly for our ultimate choice, but on examination

we shall find that they are by no means apprehended

with the same force at different times. The belief produced

by some strong argument may be very decisive at the moment,

but it will often begin to diminish when the argument

is not actually before the mind. It is like being dazzled by

a strong light; the impression still remains, but begins almost

immediately to fade away. I think that this is the

case, however we try to limit the sources of our conviction.

9. (II.) But supposing that it were possible to strike

a sort of average of this fluctuating state, should we find this

average to be of the amount assigned by theory? In other

words, is our natural belief in the happening of two different

events in direct proportion to the frequency with which those

events happen in the long run? There is a lottery with 100

tickets and ten prizes; is a man's belief that he will get a

prize fairly represented by one-tenth of certainty? The mere

reference to a lottery should be sufficient to disprove this.

Lotteries have flourished at all times, and have never failed

to be abundantly supported, in spite of the most perfect conviction,

on the part of many, if not of most, of those who put

into them, that in the long run all will lose. Deductions

should undoubtedly be made for those who act from superstitious

motives, from belief in omens, dreams, and so on.

But apart from these, and supposing any one to come fortified

by all that mathematics can do for him, it is difficult to

believe that his natural impressions about single events would

be always what they should be according to theory. Are

there many who can honestly declare that they would have

no desire to buy a single ticket? They would probably say

to themselves that the sum they paid away was nothing

worth mentioning to lose, and that there was a chance of

gaining a great deal; in other words, they are not apportioning

their belief in the way that theory assigns.

What bears out this view is, that the same persons who

would act in this way in single instances would often not

think of doing so in any but single instances. In other

words, the natural tendency here is to attribute too great an

amount of belief where it is or should be small; i.e. to depreciate

the risk in proportion to the contingent advantage.

They would very likely, when argued with, attach disparaging

epithets to this state of feeling, by calling it an unaccountable

fascination, or something of that kind, but of

its existence there can be little doubt. We are speaking

now of what is the natural tendency of our minds, not of

that into which they may at length be disciplined by education

and thought. If, however, educated persons have succeeded

for the most part in controlling this tendency in

games of chance, the spirit of reckless speculation has

scarcely yet been banished from commerce. On examination,

this tendency will be found so prevalent in all ages, ranks,

and dispositions, that it would be inadmissible to neglect it in

order to bring our supposed instincts more closely into accordance

with the commonly received theories of Probability.

10. There is another aspect of this question which has

been often overlooked, but which seems to deserve some

attention. Granted that we have an instinct of credence,

why should it be assumed that this must be just of that intensity

which subsequent experience will justify? Our

instincts are implanted in us for good purposes, and are intended

to act immediately and unconsciously. They are,

however, subject to control, and have to be brought into

accordance with what we believe to be true and right. In

other departments of psychology we do not assume that

every spontaneous prompting of nature is to be left just as

we find it, or even that on the average, omitting individual

variations, it is set at that pitch that will be found in the

end to be the best when we come to think about it and assign

its rules. Take, for example, the case of resentment. Here

we have an instinctive tendency, and one that on the whole

is good in its results. But moralists are agreed that almost

all our efforts at self-control are to be directed towards subduing

it and keeping it in its right direction. It is assumed

to be given as a sort of rough protection, and to be set, if

one might so express oneself, at too high a pitch to be

deliberately and consciously acted on in society. May not

something of this kind be the case also with our belief?

I only make a passing reference to this point here, as on

the theory of Probability adopted in this work it does not

appear to be at all material to the science. But it seems

a strong argument against the expediency of commencing

the study of the science from the subjective side, or even of

assigning any great degree of prominence to this side.

That men \_do\_ not believe in exact accordance with this

theory must have struck almost every one, but this has

probably been considered as mere exception and irregularity;

the assumption being made that on the average, and in far

the majority of cases, they do so believe. As stated above,

it is very doubtful whether the tendency which has just

been discussed is not so widely prevalent that it might with

far more propriety be called the rule than the exception.

And it may be better that this should be so: many good

results may follow from that cheerful disposition which induces

a man sometimes to go on trying after some great

good, the chance of which he overvalues. He will keep on

through trouble and disappointment, without serious harm

perhaps, when the cool and calculating bystander sees plainly

that his 'measure of belief' is much higher than it should

be. So, too, the tendency also so common, of underrating

the chance of a great evil may also work for good. By many

men death might be looked upon as an almost infinite evil,

at least they would so regard it themselves; suppose they

kept this contingency constantly before them at its right

value, how would it be possible to get through the practical

work of life? Men would be stopping indoors because if

they went out they might be murdered or bitten by a mad

dog. To say this is not to advocate a return to our instincts;

indeed when we have once reached the critical and conscious

state, it is hardly possible to do so; but it should be noticed

that the advantage gained by correcting them is at best but

a balanced one.[3] What is most to our present purpose, it

suggests the inexpediency of attempting to found an exact

theory on what may afterwards prove to be a mere instinct,

unauthorized in its full extent by experience.

11. It may be replied, that though people, as a matter

of fact, do not apportion belief in this exact way, yet they

\_ought\_ to do so. The purport of this remark will be examined

presently; it need only be said here that it grants all that

is now contended for. For it admits that the degree of our

belief is capable of modification, and may need it. But in

accordance with what is the belief to be modified? obviously

in accordance with experience; it cannot be trusted to by

itself, but the fraction at which it is to be rated must be

determined by the comparative frequency of the events to

which it refers. Experience then furnishing the standard, it

is surely most reasonable to start from this experience, and to

found the theory of our processes upon it.

If we do not do this, it should be observed that we are

detaching Probability altogether from the study of things

external to us, and making it nothing else in effect than a

portion of Psychology. If we refuse to be controlled by

experience, but confine our attention to the laws according

to which belief is naturally or instinctively compounded and

distributed in our minds, we have no right then to appeal to

experience afterwards even for illustrations, unless under the

express understanding that we do not guarantee its accuracy.

Our belief in some single events, for example, might be correct,

and yet that in a compound of several (if derived merely

from our instinctive laws of belief) very possibly might not

be correct, but might lead us into practical mistakes if we

determined to act upon it. Even if the two were in accordance,

this accordance would have to be proved, which would

lead us round, by what I cannot but think a circuitous

process, to the point which has been already chosen for

commencing with.

12. De Morgan seems to imply that the doctrine

criticised above finds a justification from the analogy of

Formal Logic. If the laws of necessary inference can be

studied apart from all reference to external facts (except

by way of illustration), why not those of probable inference?

There does not, however, seem to be much force in any such

analogy. Formal Logic, at any rate under its modern or

Kantian mode of treatment, is based upon the assumption

that there are laws of thought as distinguished from laws of

things, and that these laws of thought can be ascertained and

studied without taking into account their reference to any

particular object. Now so long as we are confined to necessary

or irreversible laws, as is of course the case in ordinary

Formal Logic, this assumption leads to no special difficulties.

We mean by this, that no conflict arises between these subjective

and objective necessities. The two exist in perfect

harmony side by side, the one being the accurate counterpart

of the other. So precise is the correspondence between

them, that few persons would notice, until study of metaphysics

had called their attention to such points, that there

were these two sides to the question. They would make

their appeal to either with equal confidence, saying indifferently,

'the thing must be so,' or, 'we cannot conceive its being

otherwise.' In fact it is only since the time of Kant that

this mental analysis has been to any extent appreciated and

accepted. And even now the dominant experience school of

philosophy would not admit that there are here two really

distinct sides to the phenomenon; they maintain either that

the subjective necessity is nothing more than the consequence

by inveterate association of the objective uniformity,

or else that this so-called necessity (say in the Law of Contradiction)

is after all merely verbal, merely a different way of

saying the same thing over again in other words. Whatever

the explanation adopted, the general result is that fallacies,

as real acts of thought, are impossible within the domain of

pure logic; error within that province is only possibly by a

momentary lapse of attention, that is of consciousness.

13. But though this perfect harmony between subjective

and objective uniformities or laws may exist within

the domain of pure logic, it is far from existing within that

of probability. The moment we make the \_quantity\_ of our

belief an integral part of the subject to be studied, any such

invariable correspondence ceases to exist. In the former

case, we could not consciously think erroneously even though

we might try to do so; in the latter, we not only can believe

erroneously but constantly do so. Far from the quantity of

our belief being so exactly adjusted in conformity with the

facts to which it refers that we cannot even in imagination

go astray, we find that it frequently exists in excess or defect

of that which subsequent judgment will approve. Our instincts

of credence are unquestionably in frequent hostility

with experience; and what do we do then? We simply

modify the instincts into accordance with the things. We

are constantly performing this practice, and no cultivated

mind would find it possible to do anything else. No man

would think of divorcing his belief from the things on which

it was exercised, or would suppose that the former had anything

else to do than to follow the lead of the latter. Hence

it results that that separation of the subjective necessity from

the objective, and that determination to treat the former

as a science apart by itself, for which a plausible defence

could be made in the case of pure logic, is entirely inadmissible

in the case of probability. However we might

contrive to '\_think\_' aright without appeal to facts, we cannot

\_believe\_ aright without incessantly checking our proceedings

by such appeals. Whatever then may be the

claims of Formal Logic to rank as a separate science, it

does not appear that it can furnish any support to the

theory of Probability at present under examination.

14. The point in question is sometimes urged as

follows. Suppose a man with two, and only two, alternatives

before him, one of which he knows must involve

success and the other failure. He knows nothing more

about them than this, and he is forced to act. Would he

not regard them with absolutely similar and equal feelings

of confidence, without the necessity of referring them to any

real or imaginary series? If so, is not this equivalent to

saying that his belief of either, since one of them must

come to pass, is equal to that of the other, and therefore that

his belief of each is one-half of full confidence? Similarly

if there are more than two alternatives: let it be supposed

that there are any number of them, amongst which no

distinctions whatever can be discerned except in such particulars

as we know for certain will not affect the result;

should we not feel equally confident in respect of each of

them? and so here again should we riot have a fractional

estimate of our absolute amount of belief? It is thus

attempted to lay the basis of a pure science of Probability,

determining the distribution and combination of our belief

hypothetically; viz. \_if\_ the contingencies are exactly alike,

then our belief is so apportioned, the question whether the

contingencies are equal being of course decided as the objective

data of Logic or Mathematics are decided.

To discuss this question fully would require a statement

at some length of the reasons in favour of the objective or

material view of Logic, as opposed to the Formal or Conceptualist.

I shall have to speak on this subject in another

chapter, and will not therefore enter upon it here. But one

conclusive objection which is applicable more peculiarly to

Probability may be offered at once. To pursue the line of

enquiry just indicated, is, as already remarked, to desert

the strictly logical ground, and to take up that appropriate

to psychology; the proper question, in all these cases, being

not what \_do\_ men believe, but what ought they to believe?

Admitting, as was done above, that in the case of Formal

Logic these two enquiries, or rather those corresponding to

them, practically run into one, owing to the fact that men

cannot consciously 'think' wrongly; it cannot be too strongly

insisted on that in Probability the two are perfectly separable

and distinct. It is of no use saying what men do or

will believe, we want to know what they will be right in

believing; and this can never be settled without an appeal

to the phenomena themselves.

15. But apart from the above considerations, this way

of putting the case does not seem to me at all conclusive.

Take the following example. A man[4] finds himself on the

sands of the Wash or Morecambe Bay, in a dense mist, when

the spring-tide is coming in; and knows therefore that to

be once caught by the tide would be fatal. He hears a

church-bell at a distance, but has no means of knowing

whether it is on the same side of the water with himself or

on the opposite side. He cannot tell therefore whether by

following its sound he will be led out into the mid-stream

and be lost, or led back to dry land and safety. Here there

can be no repetition of the event, and the cases are indistinguishably

alike, to him, in the only circumstances which

can affect the issue: is not then his prospect of death, it

will be said, necessarily equal to one-half? A proper analysis

of his state of mind would be a psychological rather than

a logical enquiry, and in any case, as above remarked, the

decision of this question does not touch our logical position.

But according to the best introspection I can give I should

say that what really passes through the mind in such a case

is something of this kind: In most doubtful positions and

circumstances we are accustomed to decide our conduct by

a consideration of the relative advantages and disadvantages

of each side, that is by the observed or inferred frequency

with which one or the other alternative has succeeded. In

proportion as these become more nearly balanced, we are

more frequently mistaken in the individual cases; that is, it

becomes more and more nearly what would be called 'a

mere toss up' whether we are right or wrong. The case

in question seems merely the limiting case, in which it has

been contrived that there shall be no appreciable difference

between the alternatives, by which to decide in favour of

one or other, and we accordingly feel no confidence in the

particular result. Having to decide, however, we decide according

to the precedent of similar cases which have occurred

before. To stand still and wait for better information is

certain death, and we therefore appeal to and employ the

only rule we know of; or rather we feel, or endeavour to

feel, as we have felt before when acting in the presence of

alternatives as nearly balanced as possible. But I can

neither perceive in my own case, nor feel convinced in that

of others, that this appeal, in a case which cannot be repeated,[5]

to a rule acted on and justified in cases which can be

and are repeated, at all forces us to admit that our state of

mind is the same in each case.

16. This example serves to bring out very clearly a point

which has been already mentioned, and which will have to be

insisted upon again, viz. that all which Probability discusses

is the statistical frequency of events, or, if we prefer so to

put it, the quantity of belief with which any one of these

events should be individually regarded, but leaves all the

subsequent conduct dependent upon that frequency, or that

belief, to the choice of the agents. Suppose there are two

travellers in the predicament in question: shall they keep

together, or separate in opposite directions? In either case

alike the chance of safety to each is the same, viz. one-half,

but clearly their circumstances must decide which course it

is preferable to adopt. If they are husband and wife, they will

probably prefer to remain together; if they are sole depositaries

of an important state secret, they may decide to part.

In other words, we have to select here between the two alternatives

of the certainty of a single loss, and the even chance

of a double loss; alternatives which the common mathematical

statement of their chances has a decided tendency

to make us regard as indistinguishable from one another.

But clearly the decision must be grounded on the desires,

feelings, and conscience of the agents. Probability cannot

say a word upon this question. As I have pointed out elsewhere,

there has been much confusion on this matter in

applications of the science to betting, and in the discussion

of the Petersburg problem.

We have thus examined the doctrine in question with

a minuteness which may seem tedious, but in consequence of

the eminence of its supporters it would have been presumptuous

to have rejected it without the strongest grounds. The

objections which have been urged might be summarised as

follows:--the amount of our belief of any given proposition,

supposing it to be in its nature capable of accurate determination

(which does not seem to be the case), depends upon a

great variety of causes, of which statistical frequency--the

subject of Probability--is but one. That even if we confine

our attention to this one cause, the natural amount of our

belief is not necessarily what theory would assign, but has to

be checked by appeal to experience. The subjective side of

Probability therefore, though very interesting and well deserving

of examination, seems a mere appendage of the objective,

and affords in itself no safe ground for a science of

inference.

17. The conception then of the science of Probability

as a science of the laws of belief seems to break down at

every point. We must not however rest content with such

merely negative criticism. The degree of belief we entertain

of a proposition may be hard to get at accurately, and

when obtained may be often wrong, and may need therefore

to be checked by an appeal to the objects of belief. Still in

popular estimation we do seem to be able with more or less

accuracy to form a graduated scale of intensity of belief.

What we have to examine now is whether this be possible,

and, if so, what is the explanation of the fact?

That it is generally believed that we can form such a

scale scarcely admits of doubt. There is a whole vocabulary

of common expressions such as, 'I feel almost sure,' 'I do not

feel quite certain,' 'I am less confident of this than of that,'

and so on. When we make use of any one of these phrases

we seldom doubt that we have a distinct meaning to convey

by means of it. Nor do we feel much at a loss, under any

given circumstances, as to which of these expressions we

should employ in preference to the others. If we were asked

to arrange in order, according to the intensity of the belief

with which we respectively hold them, things broadly marked

off from one another, we could do it from our consciousness

of belief alone, without a fresh appeal to the evidence upon

which the belief depended. Passing over the looser propositions

which are used in common conversation, let us take but

one simple example from amongst those which furnish numerical

data. Do I not feel more certain that some one will die

this week in the whole town, than in the particular street in

which I live? and if the town is known to contain a population

one hundred times greater than that in the street, would

not almost any one be prepared to assert on reflection that

he felt a hundred times more sure of the first proposition

than of the second? Or to take a non-numerical example, are

we not often able to say unhesitatingly which of two propositions

we believe the most, and to some rough degree how

much more we believe one than the other, at a time when all

the evidence upon which each rests has faded from the

mind, so that each has to be judged, as we may say, solely on

its own merits?

Here then a problem proposes itself. If popular opinion,

as illustrated in common language, be correct,--and very

considerable weight must of course be attributed to it,--there

does exist something which we call partial belief in reference

to any proposition of the numerical kind described

above. Now what we want to do is to find some test or

justification of this belief, to obtain in fact some intelligible

answer to the question, Is it correct? We shall find incidentally

that the answer to this question will throw a good deal

of light upon another question nearly as important and far

more intricate, viz. What is the meaning of this partial belief?

18. We shall find it advisable to commence by ascertaining

how such enquiries as the above would be answered

in the case of ordinary full belief. Such a step would not

offer the slightest difficulty. Suppose, to take a simple

example, that we have obtained the following proposition,--whether

by induction, or by the rules of ordinary deductive

logic, does not matter for our present purpose,--that a certain

mixture of oxygen and hydrogen is explosive. Here we have

an inference, and consequent belief of a proposition. Now

suppose there were any enquiry as to whether our belief

were correct, what should we do? The simplest way of

settling the matter would be to find out by a distinct appeal

to experience whether the proposition was true. Since we

are reasoning about things, the justification of the belief, that

is, the test of its correctness, would be most readily found in

the truth of the proposition. If by any process of inference

I have come to believe that a certain mixture will explode, I

consider my belief to be justified, that is to be correct, if

under proper circumstances the explosion always does occur;

if it does not occur the belief was wrong.

Such an answer, no doubt, goes but a little way, or rather

no way at all, towards explaining what is the nature of belief

in itself; but it is sufficient for our present purpose, which is

merely that of determining what is meant by the correctness

of our belief, and by the test of its correctness. In all inferences

about things, in which the \_amount\_ of our belief is not

taken into account, such an explanation as the above is quite

sufficient; it would be the ordinary one in any question of

science. It is moreover perfectly intelligible, whether the

conclusion is particular or universal. Whether we believe

that 'some men die', or that 'all men die', our belief may with

equal ease be tested by the appropriate train of experience.

19. But when we attempt to apply the same test to

\_partial\_ belief, we shall find ourselves reduced to an awkward

perplexity. A difficulty now emerges which has been singularly

overlooked by those who have treated of the subject.

As a simple example will serve our purpose, we will take the

case of a penny. I am about to toss one up, and I therefore

half believe, to adopt the current language, that it will give

head. Now it seems to be overlooked that if we appeal to

the event, as we did in the case last examined, our belief

must inevitably be wrong, and therefore the test above mentioned

will fail. For the thing must either happen or not

happen: i.e. in this case the penny must either give head, or

not give it; there is no third alternative. But whichever

way it occurs, our half-belief, so far as such a state of mind

admits of interpretation, must be wrong. If head does come,

I am wrong in not having expected it enough; for I only half

believed in its occurrence. If it does not happen, I am

equally wrong in having expected it too much; for I half

believed in its occurrence, when in fact it did not occur at all.

The same difficulty will occur in every case in which we

attempt to justify our state of partial belief in a single contingent

event. Let us take another example, slightly differing

from the last. A man is to receive £1 if a die gives six,

to pay 1\_s\_. if it gives any other number. It will generally be

admitted that he ought to give 2\_s\_. 6\_d\_. for the chance, and

that if he does so he will be paying a fair sum. This example

only differs from the last in the fact that instead of

simple belief in a proposition, we have taken what mathematicians

call 'the \_value\_ of the expectation'. In other words,

we have brought into a greater prominence, not merely the belief,

but the conduct which is founded upon the belief. But

precisely the same difficulty recurs here. For appealing to the

event,--the single event, that is,--we see that one or other

party must lose his money without compensation. In what

sense then can such an expectation be said to be a fair one?

20. A possible answer to this, and so far as appears the

only possible answer, will be, that what we really mean by

saying that we half believe in the occurrence of head is to

express our conviction that head will certainly happen on

the average every other time. And similarly, in the second

example, by calling the sum a fair one it is meant that in

the long run neither party will gain or lose. As we shall

recur presently to the point raised in this form of answer, the

only notice that need be taken of it at this point is to call

attention to the fact that it entirely abandons the whole

question in dispute, for it admits that this partial belief does

not in any strict sense apply to the individual event, since it

clearly cannot be justified there. At such a result indeed we

cannot be surprised; at least we cannot on the theory adopted

throughout this Essay. For bearing in mind that the employment

of Probability postulates ignorance of the single

event, it is not easy to see how we are to justify any other

opinion or statement about the single event than a confession

of such ignorance.

21. So far then we do not seem to have made the

slightest approximation to a solution of the particular

question now under examination. The more closely we have

analysed special examples, the more unmistakeably are we

brought to the conclusion that in the individual instance no

justification of anything like quantitative belief is to be

found; at least none is to be found in the same sense in

which we expect it in ordinary scientific conclusions, whether

Inductive or Deductive. And yet we have to face and

account for the fact that common impressions, as attested by

a whole vocabulary of common phrases, are in favour of the

existence of this quantitative belief. How are we to account

for this? If we appeal to an example again, and analyse it

somewhat more closely, we may yet find our way to some

satisfactory explanation.

In our previous analysis (§18) we found it sufficient to stop

at an early stage, and to give as the justification of our belief

the fact of the proposition being true. Stopping however at

that stage, we have found this explanation fail altogether to

give a justification of partial belief; fail, that is, when applied

to the individual instance. The two states of belief and disbelief

correspond admirably to the two results of the event

happening and not happening respectively, and unless for

psychological purposes we saw no reason to analyse further;

but to partial belief there is nothing corresponding in the result,

for the event cannot partially happen in such cases as we

are concerned with. Suppose then we advance a step further

in the analysis, and ask again what is meant by the proposition

being true? This introduces us, of course, to a very long

and intricate path; but in the short distance along it which

we shall advance, we shall not, it is to be hoped, find any

very serious difficulty. As before, we will illustrate the

analysis by first applying it to the case of ordinary full belief.

22. Whatever opinion then may be held about the

essential nature of belief, it will probably be admitted that a

readiness to act upon the proposition believed is an inseparable

accompaniment of that state of mind. There can be no

alteration in our belief (at any rate in the case of sane persons)

without a possible alteration in our conduct, nor anything in

our conduct which is not connected with something in our

belief. We will first take an example in connection with the

penny, in which there is full belief; we will analyse it a step

further than we did before, and then attempt to apply the

same analysis to an example of a similar kind, but one in

which the belief is partial instead of full.

Suppose that I am about to throw a penny up, and contemplate

the prospect of its falling upon one of its sides and

not upon its edge. We feel perfectly confident that it will

do so. Now whatever else may be implied in our belief, we

certainly mean this; that we are ready to stake our conduct

upon its falling thus. All our betting, and everything else

that we do, is carried on upon this supposition. Any risk

whatever that might ensue upon its falling otherwise will be

incurred without fear. This, it must be observed, is equally

the case whether we are speaking of a single throw or of a

long succession of throws.

But now let us take the case of a penny falling, not upon

one side or the other, but upon a given side, \_head\_. To a

certain extent this example resembles the last. We are perfectly

ready to stake our conduct upon what comes to pass in

the long run. When we are considering the result of a large

number of throws, we are ready to act upon the supposition

that head comes every other time. If e.g. we are betting

upon it, we shall not object to paying £1 every time that

head comes, on condition of receiving £1 every time that

head does not come. This is nothing else than the \_translation\_,

as we may call it, into practice, of our belief that head

and tail occur equally often.

Now it will be obvious, on a moment's consideration, that

our conduct is capable of being slightly varied: of being

varied, that is, in form, whilst it remains identical in respect

of its results. It is clear that to pay £1 every time we lose,

and to get £1 every time we gain, comes to precisely the same

thing, in the case under consideration, as to pay ten shillings

every time without exception, and to receive £1 every time

that head occurs. It is so, because heads occur, on the

average, every other time. In the long run the two results

coincide; but there is a marked difference between the two

cases, considered individually. The difference is two-fold.

In the first place we depart from the notion of a payment

every other time, and come to that of one made every time.

In the second place, what we pay every time is half of what

we get in the cases in which we do get anything. The difference

may seem slight; but mark the effect when our conduct

is translated back again into the subjective condition

upon which it depends, viz. into our belief. It is in consequence

of such a translation, as it appears to me, that the

notion has been acquired that we have an accurately determinable

amount of belief as to every such proposition. To

have losses and gains of equal amount, and to incur them

equally often, was the experience connected with our belief

that the two events, head and tail, would occur equally often.

This was quite intelligible, for it referred to the long run.

To find that this could be commuted for a payment made

every time without exception, a payment, observe, of half the

amount of what we occasionally receive, has very naturally

been interpreted to mean that there must be a state of half-belief

which refers to each individual throw.

23. One such example, of course, does not go far towards

establishing a theory. But the reader will bear in

mind that almost all our conduct tends towards the same

result; that it is not in betting only, but in every course of

action in which we have to count the events, that such a

numerical apportionment of our conduct is possible. Hence,

by the ordinary principles of association, it would appear

exceedingly likely that, not exactly a numerical condition of

mind, but rather numerical associations, become inseparably

connected with each particular event which we know to occur

in a certain proportion of times. Once in six times a die

gives ace; a knowledge of this fact, taken in combination

with all the practical results to which it leads, produces, one

cannot doubt, an inseparable notion of one-sixth connected

with each \_single\_ throw. But it surely cannot be called belief

to the amount of one-sixth; at least it admits neither of

justification nor explanation in these single cases, to which

alone the fractional belief, if such existed, ought to apply.

It is in consequence, I apprehend, of such association that

we act in such an unhesitating manner in reference to any

single contingent event, even when we have no expectation

of its being repeated. A die is going to be thrown up once,

and once only. I bet 5 to 1 against ace, not, as is commonly

asserted, because I feel one-sixth part of certainty in the

occurrence of ace; but because I know that such conduct

would be justified in the long run of such cases, and I apply

to the solitary individual the same rule that I should apply

to it if I knew it were one of a long series. This accounts

for my conduct being the same in the two cases; by association,

moreover, we probably experience very similar feelings

in regard to them both.

24. And here, on the view of the subject adopted in

this Essay, we might stop. We are bound to explain the

'measure of our belief' in the occurrence of a single event

when we judge solely from the statistical frequency with

which such events occur, for such a series of events was our

starting-point; but we are not bound to inquire whether in

every case in which persons have, or claim to have, a certain

measure of belief there must be such a series to which to

refer it, and by which to justify it. Those who start from

the subjective side, and regard Probability as the science of

quantitative belief, are obliged to do this, but we are free

from the obligation.

Still the question is one which is so naturally raised in

connection with this subject, that it cannot be altogether

passed by. I think that to a considerable extent such a

justification as that mentioned above will be found applicable

in other cases. The fact is that we are very seldom called

upon to decide and act upon a single contingency which cannot

be viewed as being one of a series. Experience introduces

us, it must be remembered, not merely to a succession

of events neatly arranged in a single series (as we have

hitherto assumed them to be for the purpose of illustration),

but to an infinite number belonging to a vast variety of

different series. A man is obliged to be acting, and therefore

exercising his belief about one thing or another, almost

the whole of every day of his life. Any one person will have

to decide in his time about a multitude of events, each one

of which may never recur again within his own experience.

But by the very fact of there being a multitude, though they

are all of different kinds, we shall still find that order is

maintained, and so a course of conduct can be justified. In

a plantation of trees we should find that there is order of a

certain kind if we measure them in any one direction, the

trees being on an average about the same distance from each

other. But a somewhat similar order would be found if we

were to examine them in any other direction whatsoever. So

in nature generally; there is regularity in a succession of

events of the same kind. But there may also be regularity

if we form a series by taking successively a number out of

totally distinct kinds.

It is in this circumstance that we find an extension of the

practical justification of the measure of our belief. A man,

say, buys a life annuity, insures his life on a railway journey,

puts into a lottery, and so on. Now we may make a series

out of these acts of his, though each is in itself a single event

which he may never intend to repeat. His conduct, and therefore

his belief, measured by the result in each individual

instance, will not be justified, but the reverse, as shown in §19.

Could he indeed repeat each kind of action often

enough it would be justified; but from this, by the conditions

of life, he is debarred. Now it is perfectly conceivable that

in the new series, formed by his successive acts of different

kinds, there should be no regularity. As a matter of fact,

however, it is found that there is regularity. In this way the

equalization of his gains and losses, for which he cannot hope

in annuities, insurances, and lotteries taken separately, may

yet be secured to him out of these events taken collectively.

If in each case he values his chance at its right proportion

(and acts accordingly) he will in the course of his life neither

gain nor lose. And in the same way if, whenever he has the

alternative of different courses of conduct, he acts in accordance

with the estimate of his belief described above, i.e.

chooses the event whose chance is the best, he will in the end

gain more in this way than by any other course. By the existence,

therefore, of these \_cross-series\_, as we may term them,

there is an immense addition to the number of actions which

may be fairly considered to belong to those courses of conduct

which offer many successive opportunities of equalizing

gains and losses. All these cases then may be regarded as

admitting of justification in the way now under discussion.

25. In the above remarks it will be observed that we

have been giving what is to be regarded as a justification of

his belief from the point of view of the individual agent himself.

If we suppose the existence of an enlarged fellow-feeling,

the applicability of such a justification becomes still more

extensive. We can assign a very intelligible sense to the

assertion that it is 999 to 1 that I shall not get a prize in a

lottery, even if this be stated in the form that my belief in

my so doing is represented by the fraction 1/1000th of certainty.

Properly it means that in a very large number of throws I

should gain once in 1000 times. If we include other contingencies

of the same kind, as described in the last section,

each individual may be supposed to reach to something like

this experience within the limits of his own life. He could

not do it in this particular line of conduct alone, but he could

do it in this line combined with others. Now introduce the

possibility of each man feeling that the gain of others offers

some analogy to his own gains, which we may conceive his

doing except in the case of the gains of those against whom

he is directly competing, and the above justification becomes

still more extensively applicable.

The following would be a fair illustration to test this

view. I know that I must die on some day of the week, and

there are but seven days. My belief, therefore, that I shall

die on a Sunday is one-seventh. Here the contingent event

is clearly one that does not admit of repetition; and yet

would not the belief of every man have the value assigned it

by the formula? It would appear that the same principle

will be found to be at work here as in the former examples.

It is quite true that I have only the opportunity of dying

once myself, but I am a member of a class in which deaths

occur with frequency, and I form my opinion upon evidence

drawn from that class. If, for example, I had insured my

life for £1000, I should feel a certain propriety in demanding

£7000 in case the office declared that it would only pay in

the event of my dying on a Sunday. \_I\_, indeed, for my own

private part, might not find the arrangement an equitable

one; but mankind at large, in case they acted on such a

principle, might fairly commute their aggregate gains in such

a way, whilst to the Insurance Office it would not make any

difference at all.

26. The results of the last few sections might be summarised

as follows:--the different amounts of belief which

we entertain upon different events, and which are recognized

by various phrases in common use, have undoubtedly some

meaning. But the greater part of their meaning, and certainly

their only justification, are to be sought in the \_series\_

of corresponding events to which they belong; in regard to

which it may be shown that far more events are capable of

being referred to a series than might be supposed at first

sight. The test and justification of belief are to be found in

conduct; in this test applied to the series as a whole, there

is nothing peculiar, it differs in no way from the similar test

when we are acting on our belief about any single event.

But so applied, from the nature of the case it is applied

successively to each of the individuals of the series; here our

\_conduct\_ generally admits of being separately considered in

reference to each particular event; and this has been understood

to denote a certain amount of belief which should be a

fraction of certainty. Probably on the principles of association,

a peculiar condition of mind is produced in reference to

each single event. And these associations are not unnaturally

retained even when we contemplate any one of these single

events isolated from any series to which it belongs. When

it is found alone we treat it, and feel towards it, as we do

when it is in company with the rest of the series.

27. We may now see, more clearly than we could

before, why it is that we are free from any necessity of assuming

the existence of causation, in the sense of necessary

invariable sequence, in the case of the events which compose

our series. Against such a view it might very plausibly be

urged, that we constantly talk of the probability of a single

event; but how can this be done, it may reasonably be said,

if we once admit the possibility of that event occurring fortuitously?

Take an instance from human life; the average

duration of the lives of a batch of men aged thirty will be

about thirty-four years. We say therefore to any individual

of them, Your expectation of life is thirty-four years. But

how can this be said if we admit that the train of events

composing his life is liable to be destitute of all regular

sequence of cause and effect? To this it may be replied

that the denial of causation enables us to say neither more

nor less than its assertion, in reference to the length of the

individual life, for of this we are ignorant in each case alike.

By assigning, as above, an expectation in reference to the

individual, we \_mean\_ nothing more than to make a statement

about the average of his class. Whether there be causation

or not in these individual cases does not affect our knowledge

of the average, for this by supposition rests on independent

experience. The legitimate inferences are the same on either

hypothesis, and of equal value. The only difference is that

on the hypothesis of non-causation we have forced upon our

attention the impropriety of talking of the 'proper' expectation

of the individual, owing to the fact that all knowledge of

its amount is formally impossible; on the other hypothesis

the impropriety is overlooked from the fact of such knowledge

being only practically unattainable. As a matter of

fact the amount of our knowledge is the same in each case;

it is a knowledge of the average, and of that only.[6]

28. We may conclude, then, that the limits within

which we are thus able to justify the amount of our belief

are far more extensive than might appear at first sight.

Whether every case in which persons feel an amount of

belief short of perfect confidence could be forced into the

province of Probability is a wider question. Even, however,

if the belief could be supposed capable of justification on its

principles, its rules could never in such cases be made use of.

Suppose, for example, that a father were in doubt whether

to give a certain medicine to his sick child. On the one

hand, the doctor declared that the child would die unless the

medicine were given; on the other, through a mistake, the

father cannot feel quite sure that the medicine he has is the

right one. It is conceivable that some mathematicians, in

their conviction that everything has its definite numerical

probability, would declare that the man's belief had some

'value' (if they could only find out what it is), say nine-tenths;

by which they would mean that in nine cases out of

ten in which he entertained a belief of that particular value

he proved to be right. So with his belief and doubt on

the other side of the question. Putting the two together,

there is but one course which, as a prudent man and a good

father, he can possibly follow. It may be so, but when (as

here) the identification of an event in a series depends on

purely subjective conditions, as in this case upon the degree

of vividness of his conviction, of which no one else can judge,

no test is possible, and therefore no proof can be found.

29. So much then for the attempts, so frequently

made, to found the science on a subjective basis; they can

lead, as it has here been endeavoured to show, to no satisfactory

result. Still our belief is so inseparably connected with

our action, that something of a defence can be made for the

attempts described above; but when it is attempted, as is

often the case, to import other sentiments besides pure belief,

and to find a justification for them also in the results of our

science, the confusion becomes far worse. The following

extract from Archbishop Thomson's \_Laws of Thought\_ (§122,

Ed. II.) will show what kind of applications of the science are

contemplated here: "In applying the doctrine of chances to

that subject in connexion with which it was invented--games

of chance,--the principles of what has been happily termed

'moral arithmetic' must not be forgotten. Not only would

it be difficult for a gamester to find an antagonist on terms,

as to fortune and needs, precisely equal, but also it is impossible

that with such an equality the advantage of a

considerable gain should balance the harm of a serious loss.

'If two men,' says Buffon, 'were to determine to play for

their whole property, what would be the effect of this agreement?

The one would only double his fortune, and the

other reduce his to naught. What proportion is there between

the loss and the gain? The same that there is between

all and nothing. The gain of the one is but a moderate

sum,--the loss of the other is numerically infinite, and

morally so great that the labour of his whole life may not

perhaps suffice to restore his property.'"

As moral advice this is all very true and good. But if it

be regarded as a contribution to the science of the subject it

is quite inappropriate, and seems calculated to cause confusion.

The doctrine of chances pronounces upon certain

kinds of events in respect of number and magnitude; it has

absolutely nothing to do with any particular person's feelings

about these relations. We might as well append a corollary

to the rules of arithmetic, to point out that although it is

very true that twice two are four it does not follow that four,

horses will give twice as much pleasure to the owner as two

will. If two men play on equal terms their chances are

equal; in other words, if they were often to play in this

manner each would lose as frequently as he would gain. That

is all that Probability can say; what under the circumstances

may be the determination and opinions of the men in question,

it is for them and them alone to decide. There are

many persons who cannot bear mediocrity of any kind, and

to whom the prospect of doubling their fortune would outweigh

a greater chance of losing it altogether. They alone

are the judges.

If we will introduce such a balance of pleasure and pain

the individual must make the calculation for himself. The

supposition is that total ruin is very painful, partial loss

painful in a less proportion than that assigned by the ratio

of the losses themselves; the inference is therefore drawn

that on the average more pain is caused by occasional great

losses than by frequent small ones, though the money value

of the losses in the long run may be the same in each case.

But if we suppose a country where the desire of spending

largely is very strong, and where owing to abundant production

loss is easily replaced, the calculation might incline the

other way. Under such circumstances it is quite possible

that more happiness might result from playing for high than

for low stakes. The fact is that all emotional considerations

of this kind are irrelevant; they are, at most, mere applications

of the theory, and such as each individual is alone

competent to make for himself. Some more remarks will be

made upon this subject in the chapter upon Insurance and

Gambling.

30. It is by the introduction of such considerations as

these that the Petersburg Problem has been so perplexed.

Having already given some description of this problem we

will refer to it very briefly here. It presents us with a

sequence of sets of throws for each of which sets I am to

receive something, say a shilling, as the minimum receipt.

My receipts increase in proportion to the rarity of each

particular kind of set, and each kind is observed or inferred

to grow more rare in a certain definite but unlimited order.

By the wording of the problem, properly interpreted, I am

supposed never to stop. Clearly therefore, however large a

fee I pay for each of these sets, I shall be sure to make it up

in time. The mathematical expression of this is, that I

ought always to pay an infinite sum. To this the objection

is opposed, that no sensible man would think of advancing

even a large finite sum, say £50. Certainly he would not;

but why? Because neither he nor those who are to pay

him would be likely to live long enough for him to obtain

throws good enough to remunerate him for one-tenth of his

outlay; to say nothing of his trouble and loss of time. We

must not suppose that the problem, as stated in the ideal

form, will coincide with the practical form in which it

presents itself in life. A carpenter might as well object to

Euclid's second postulate, because his plane came to a stop

in six feet on the plank on which he was at work. Many

persons have failed to perceive this, and have assumed that,

besides enabling us to draw numerical inferences about the

members of a series, the theory ought also to be called upon

to justify all the opinions which average respectable men

might be inclined to form about them, as well as the conduct

they might choose to pursue in consequence. It is obvious

that to enter upon such considerations as these is to diverge

from our proper ground. We are concerned, in these cases,

with the actions of men only, as given in statistics; with the

emotions they experience in the performance of these actions

we have no direct concern whatever. The error is the same

as if any one were to confound, in political economy, value in

use with value in exchange, and object to measuring the

value of a loaf by its cost of production, because bread is

worth more to a man when he is hungry than it is just after

his dinner.

31. One class of emotions indeed ought to be excepted,

which, from the apparent uniformity and consistency

with which they show themselves in different persons

and at different times, do really present some better claim to

consideration. In connection with a science of inference

they can never indeed be regarded as more than an accident

of what is essential to the subject, but compared with other

emotions they seem to be inseparable accidents.

The reader will remember that attention was drawn in

the earlier part of this chapter to the compound nature of

the state of mind which we term belief. It is partly intellectual,

partly also emotional; it professes to rest upon

experience, but in reality the experience acts through the

distorting media of hopes and fears and other disturbing

agencies. So long as we confine our attention to the \_state

of mind\_ of the person who believes, it appears to me that

these two parts of belief are quite inseparable. Indeed, to

speak of them as two parts may convey a wrong impression;

for though they spring from different sources, they so entirely

merge in one result as to produce what might be

called an indistinguishable compound. Every kind of inference,

whether in probability or not, is liable to be disturbed

in this way. A timid man may honestly believe that he will

be wounded in a coming battle, when others, with the same

experience but calmer judgments, see that the chance is

too small to deserve consideration. But such a man's belief,

if we look only to that, will not differ in its nature from

sound belief. His conduct also in consequence of his belief

will by itself afford no ground of discrimination; he will

make his will as sincerely as a man who is unmistakeably on

his death-bed. The only resource is to check and correct

his belief by appealing to past and current experience.[7] This

was advanced as an objection to the theory on which probability

is regarded as concerned primarily with laws of belief.

But on the view taken in this Essay in which we are supposed

to be concerned with laws of inference about things,

error and difficulty from this source vanish. Let us bear clearly

in mind that we are concerned with inferences about things,

and whatever there may be in belief which does not depend

on experience will disappear from notice.

32. These emotions then can claim no notice as an

integral portion of any science of inference, and should in

strictness be rigidly excluded from it. But if any of them

are uniform and regular in their production and magnitude,

they may be fairly admitted as accidental and extraneous

accompaniments. This is really the case to some extent

with our surprise. This emotion does show a considerable

degree of uniformity. The rarer any event is the more am I,

in common with most other men, surprised at it when it does

happen. This surprise may range through all degrees, from

the most languid form of interest up to the condition which

we term 'being startled'. And since the surprise seems

to be pretty much the same, under similar circumstances,

at different times, and in the case of different persons, it is

free from that extreme irregularity which is found in most

of the other mental conditions which accompany the contemplation

of unexpected events. Hence our surprise, though,

as stated above, having no proper claim to admission into

the science of Probability, is such a constant and regular

accompaniment of that which Probability is concerned with,

that notice must often be taken of it. References will occasionally

be found to this aspect of the question in the

following chapters.

It may be remarked in passing, for the sake of further

illustration of the subject, that this emotional accompaniment

of surprise, to which we are thus able to assign something

like a fractional value, differs in two important respects

from the commonly accepted fraction of belief. In the first

place, it has what may be termed an independent existence;

it is intelligible by itself. The belief, as we endeavoured to

show, needs explanation and finds it in our consequent conduct.

Not so with the emotion; this stands upon its own

footing, and may be examined in and by itself. Hence, in

the second place, it is as applicable, and as capable of any kind

of justification, in relation to the \_single event\_, as to a series of

events. In this respect, as will be remembered, it offers a

complete contrast to our state of belief about any one contingent

event. May not these considerations help to account

for the general acceptance of the doctrine, that we have a

certain definite and measurable amount of belief about these

events? I cannot help thinking that what is so obviously

true of the emotional portion of the belief, has been unconsciously

transferred to the other or intellectual portion of the

compound condition, to which it is not applicable, and where

it cannot find a justification.

33. A further illustration may now be given of the

subjective view of Probability at present under discussion.

An appeal to common language is always of service, as

the employment of any distinct word is generally a proof

that mankind have observed some distinct properties in the

things, which have caused them to be singled out and have

that name appropriated to them. There is such a class of

words assigned by popular usage to the kind of events of

which Probability takes account. If we examine them we

shall find, I think, that they direct us unmistakeably to the

two-fold aspect of the question,--the objective and the subjective,

the quality in the events and the state of our minds

in considering them,--that have occupied our attention

during the former chapters.

The word 'extraordinary', for instance, seems to point to

the observed fact, that events are arranged in a sort of \_ordo\_

or rank. No one of them might be so exactly placed that

we could have inferred its position, but when we take a great

many into account together, running our eye, as it were,

along the line, we begin to see that they really do for the

most part stand in order. Those which stand away from the

line have this divergence observed, and are called extraordinary,

the rest ordinary, or in the line. So too 'irregular'

and 'abnormal' are doubtless used from the appearance

of things, when examined in large numbers, being that

of an arrangement by rule or measure. This only holds

when there are a good many; we could not speak of the

single events being so arranged. Again the word 'law', in

its philosophical sense, has now become quite popularised.

How the term became introduced is not certain, but

there can be little doubt that it was somewhat in this

way:--The effect of a law, in its usual application to

human conduct, is to produce regularity where it did not

previously exist; when then a regularity began to be perceived

in nature, the same word was used, whether the cause

was supposed to be the same or not. In each case there

was the same generality of agreement, subject to occasional

deflection.[8]

On the other hand, observe the words 'wonderful', 'unexpected',

'incredible'. Their connotation describes states

of mind simply; they are of course not confined to Probability,

in the sense of statistical frequency, but imply simply

that the events they denote are such as from some cause we

did not expect would happen, and at which therefore, when

they do happen, we are surprised.

Now when we bear in mind that these two classes of

words are in their origin perfectly distinct;--the one denoting

simply events of a certain character; the other,

though also denoting events, \_connoting\_ simply states of

mind;--and yet that they are universally applied to the

same events, so as to be used as perfectly synonymous, we

have in this a striking illustration of the two sides under

which Probability may be viewed, and of the universal recognition

of a close connection between them. The words are

popularly used as synonymous, and we must not press their

meaning too far; but if it were to be observed, as I am

rather inclined to think it could, that the application of the

words which denote mental states is wider than that of the

others, we should have an illustration of what has been

already observed, viz. that the province of Probability is not

so extensive as that over which variation of belief might be

observed. Probability only considers the case in which this

variation is brought about in a certain definite statistical

way.

34. It will be found in the end both interesting and

important to have devoted some attention to this subjective

side of the question. In the first place, as a mere speculative

inquiry the quantity of our belief of any proposition

deserves notice. To study it at all deeply would be to trespass

into the province of Psychology, but it is so intimately

connected with our own subject that we cannot avoid all

reference to it. We therefore discuss the laws under which

our expectation and surprise at isolated events increases or

diminishes, so as to account for these states of mind in any

individual instance, and, if necessary, to correct them when

they vary from their proper amount.

But there is another more important reason than this.

It is quite true that when the subjects of our discussion in

any particular instance lie entirely within the province

of Probability, they may be treated without any reference

to our belief. We may or we may not employ this side of

the question according to our pleasure. If, for example, I

am asked whether it is more likely that A. B. will die this

year, than that it will rain to-morrow, I may calculate the

chance (which really is at bottom the same thing as my

belief) of each, find them respectively, one-sixth and one-seventh,

say, and therefore decide that my 'expectation' of

the former is the greater, viz. that this is the more likely

event. In this case the process is precisely the same whether

we suppose our belief to be introduced or not; our mental

state is, in fact, quite immaterial to the question. But, in

other cases, it may be different. Suppose that we are comparing

two things, of which one is wholly alien to Probability,

in the sense that it is hopeless to attempt to assign

any degree of numerical frequency to it, the only ground

they have in common may be the amount of belief to which

they are respectively entitled. We cannot compare the

frequency of their occurrence, for one may occur too seldom

to judge by, perhaps it may be unique. It has been already

said, that our belief of many events rests upon a very complicated

and extensive basis. My belief may be the product

of many conflicting arguments, and many analogies more or

less remote; these proofs themselves may have mostly faded

from my mind, but they will leave their effect behind them

in a weak or strong conviction. At the time, therefore, I

may still be able to say, with some degree of accuracy,

though a very slight degree, what amount of belief I entertain

upon the subject. Now we cannot compare things that

are heterogeneous: if, therefore, we are to decide between

this and an event determined naturally and properly by

Probability, it is impossible to appeal to chances or frequency

of occurrence. The measure of belief is the only common

ground, and we must therefore compare this quantity in each

case. The test afforded will be an exceedingly rough one,

for the reasons mentioned above, but it will be better than

none; in some cases it will be found to furnish all we want.

Suppose, for example, that one letter in a million is lost

in the Post Office, and that in any given instance I wish to

know which is more likely, that a letter has been so lost, or

that my servant has stolen it? If the latter alternative

could, like the former, be stated in a numerical form, the

comparison would be simple. But it cannot be reduced to

this form, at least not consciously and directly. Still, if we

could feel that our belief in the man's dishonesty was greater

than one-millionth, we should then have homogeneous things

before us, and therefore comparison would be possible.

35. We are now in a position to give a tolerably accurate

definition of a phrase which we have frequently been

obliged to employ, or incidentally to suggest, and of which

the reader may have looked for a definition already, viz. the

probability of an event, or what is equivalent to this, the

chance of any given event happening. I consider that these

terms presuppose a series; within the indefinitely numerous

class which composes this series a smaller class is distinguished

by the presence or absence of some attribute or

attributes, as was fully illustrated and explained in a previous

chapter. These larger and smaller classes respectively

are commonly spoken of as instances of the 'event,' and of

'its happening in a given particular way.' Adopting this

phraseology, which with proper explanations is suitable

enough, we may define the probability or chance (the terms

are here regarded as synonymous) of the event happening

in that particular way as the numerical fraction which represents

the proportion between the two different classes in the

long run. Thus, for example, let the probability be that

of a given infant living to be eighty years of age. The

larger series will comprise all infants, the smaller all who live

to eighty. Let the proportion of the former to the latter be

9 to 1; in other words, suppose that one infant in ten lives

to eighty. Then the chance or probability that any given

infant will live to eighty is the numerical fraction 1/10. This

assumes that the series are of indefinite extent, and of the

kind which we have described as possessing a fixed type.

If this be not the case, but the series be supposed terminable,

or regularly or irregularly fluctuating, as might be the

case, for instance, in a society where owing to sanitary or

other causes the average longevity was steadily undergoing

a change, then in so far as this is the case the series ceases

to be a subject of science. What we have to do under these

circumstances, is to substitute a series of the right kind for

the inappropriate one presented by nature, choosing it, of

course, with as little deflection as possible from the observed

facts. This is nothing more than has to be done, and invariably

is done, whenever natural objects are made subjects

of strict science.

36. A word or two of explanation may be added about

the expression employed above, 'the proportion in the long

run.' The run must be supposed to be very long indeed, in

fact never to stop. As we keep on taking more terms of the

series we shall find the proportion still fluctuating a little,

but its fluctuations will grow less. The proportion, in fact,

will gradually approach towards some fixed numerical value,

what mathematicians term its \_limit\_. This fractional value

is the one spoken of above. In the cases in which deductive

reasoning is possible, this fraction may be obtained without

direct appeal to statistics, from reasoning about the conditions

under which the events occur, as was explained in

the fourth chapter.

Here becomes apparent the full importance of the distinction

so frequently insisted on, between the actual irregular

series before us and the substituted one of calculation, and

the meaning of the assertion (Ch. I. §13), that it was in the

case of the latter only that strict scientific inferences could

be made. For how can we have a 'limit' in the case of

those series which ultimately exhibit irregular fluctuations?

When we say, for instance, that it is an even chance that

a given person recovers from the cholera, the meaning of

this assertion is that in the long run one half of the persons

attacked by that disease do recover. But if we examined

a sufficiently extensive range of statistics, we might find

that the manners and customs of society had produced such

a change in the type of the disease or its treatment, that we

were no nearer approaching towards a fixed limit than we

were at first. The conception of an ultimate limit in the

ratio between the numbers of the two classes in the series

necessarily involves an absolute fixity of the type. When

therefore nature does not present us with this absolute fixity,

as she seldom or never does except in games of chance (and

not demonstrably there), our only resource is to introduce

such a series, in other words, as has so often been said, to

substitute a series of the right kind.

37. The above, which may be considered tolerably

complete as a definition, might equally well have been

given in the last chapter. It has been deferred however

to the present place, in order to connect with it at once a

proposition involving the conceptions introduced in this

chapter; viz. the state of our own minds, in reference to the

amount of belief we entertain in contemplating any one

of the events whose probability has just been described.

Reasons were given against the opinion that our belief admitted

of any exact apportionment like the numerical one

just mentioned. Still, it was shown that a reasonable explanation

could be given of such an expression as, 'my belief is

1/10th of certainty', though it was an explanation which pointed

unmistakeably to a series of events, and ceased to be intelligible,

or at any rate justifiable, when it was not viewed in

such a relation to a series. In so far, then, as this explanation

is adopted, we may say that our belief is in proportion

to the above fraction. This referred to the purely

intellectual part of belief which cannot be conceived to be

separable, even in thought, from the things upon which it

is exercised. With this intellectual part there are commonly

associated various emotions. These we can to a

certain extent separate, and, when separated, can measure

with that degree of accuracy which is possible in the case of

other emotions. They are moreover intelligible in reference

to the individual events. They will be found to increase

and diminish in accordance, to some extent, with the fraction

which represents the scarcity of the event. The emotion of

surprise does so with some degree of accuracy.

The above investigation describes, though in a very brief

form, the amount of truth which appears to me to be contained

in the assertion frequently made, that the fraction

expressive of the probability represents also the fractional

part of full certainty to which our belief of the individual

event amounts. Any further analysis of the matter would

seem to belong to Psychology rather than to Probability.

1. In the ordinary signification of this term. As De Morgan uses it he

makes Formal Logic \_include\_ Probability, as one of its branches, as

indicated in his title "Formal Logic, or the Calculus of Inference,

necessary and probable."

2. \_Formal Logic\_. Preface, page v.

3. An illustration of the points here insisted on has recently [1876]

been given in a quarter where few would have expected it; I allude,

as many readers will readily infer, to J. S. Mill's exceedingly

interesting Essays on Theism. It is not within our province here to

criticise any of their conclusions, but they have expressed in a

very significant way the conviction entertained by him that beliefs

which are not justified by evidence, and possibly may not be capable

of justification (those for instance of immortality and the

existence of the Deity), may nevertheless not only continue to exist

in cultivated minds, but may also be profitably encouraged there, at

any rate in the shape of hopes, for certain supposed advantages

attendant on their retention, irrespective even of their truth.

4. It is necessary to take an example in which the man is forced to

act, or we should not be able to shew that he has any belief on the

subject at all. He may declare that he neither knows nor cares

anything about the matter, and that therefore there is nothing of

the nature of belief to be extracted out of his mental condition. He

very likely would take this ground if we asked him, as De Morgan

does, with a slightly different reference (\_Formal Logic\_, p. 183),

whether he considers that there are volcanoes on the unseen side of

the moon larger than those on the side turned towards us; or, with

Jevons (\_Principles of Science\_, Ed. II. p. 212) whether he

considers that a Platythliptic Coefficient is positive. These do not

therefore seem good instances to illustrate the position that we

always entertain a certain degree of belief on every question which

can be stated, and that utter inability to give a reason in favour

of either alternative corresponds to half belief.

5. Except indeed on the principles indicated further on in §§24, 25.

6. For a fuller discussion of this, see the Chapter on Causation.

7. The best example I can recall of the distinction between judging

from the subjective and the objective side, in such cases as these,

occurred once in a railway train. I met a timid old lady who was in

much fear of accidents. I endeavoured to soothe her on the usual

statistical ground of the extreme rarity of such events. She

listened patiently, and then replied, "Yes, Sir, that is all very

well; but I don't see how the real danger will be a bit the less

because I don't believe in it."

8. This would still hold of \_empirical\_ laws which may be capable of

being broken: we now have very much shifted the word, to denote an

\_ultimate\_ law which it is supposed cannot be broken.

CHAPTER VII.

\_THE RULES OF INFERENCE IN PROBABILITY.\_

1. In the previous chapter, an investigation was made into

what may be called, from the analogy of Logic, Immediate

Inferences. Given that nine men out of ten, of any assigned

age, live to forty, what could be inferred about the prospect

of life of any particular man? It was shown that, although

this step was very far from being so simple as it is frequently

supposed to be, and as the corresponding step really is in

Logic, there was nevertheless an intelligible sense in which

we might speak of the amount of our belief in any one of

these 'proportional propositions,' as they may succinctly be

termed, and justify that amount. We must now proceed to

the consideration of inferences more properly so called, I

mean inferences of the kind analogous to those which form the

staple of ordinary logical treatises. In other words, having

ascertained in what manner particular propositions could be

inferred from the general propositions which included them,

we must now examine in what cases one general proposition

can be inferred from another. By a general proposition here

is meant, of course, a general proposition of the statistical

kind contemplated in Probability. The rules of such inference

being very few and simple, their consideration will not

detain us long. From the data now in our possession we are

able to deduce the rules of probability given in ordinary

treatises upon the science. It would be more correct to say

that we are able to deduce \_some\_ of these rules, for, as will

appear on examination, they are of two very different kinds,

resting on entirely distinct grounds. They might be divided

into those which are formal, and those which are more or less

experimental. This may be otherwise expressed by saying

that, from the kind of series described in the first chapters,

some rules will follow necessarily by the mere application of

arithmetic; whilst others either depend upon peculiar hypotheses,

or demand for their establishment continually renewed

appeals to experience, and extension by the aid of the

various resources of Induction. We shall confine our attention

at present principally to the former class; the latter can

only be fully understood when we have considered the connection

of our science with Induction.

2. The fundamental rules of Probability strictly so

called, that is the formal rules, may be divided into two

classes,--those obtained by addition or subtraction on the

one hand, corresponding to what are generally termed the

connection of exclusive or incompatible events;[1] and those

obtained by multiplication or division, on the other hand,

corresponding to what are commonly termed dependent

events. We will examine these in order.

(1) We can make inferences by simple addition. If,

for instance, there are two distinct properties observable in

various members of the series, which properties do not occur

in the same individual; it is plain that in any batch the

number that are of one kind or the other will be equal to the

sum of those of the two kinds separately. Thus 36.4 infants

in 100 live to over sixty, 35.4 in 100 die before they are

ten;[2] take a large number, say 10,000, then there will be

about 3640 who live to over sixty, and about 3540 who do

not reach ten; hence the total number who do not die within

the assigned limits will be about 2820 altogether. Of course

if these proportions were accurately assigned, the resultant

sum would be equally accurate: but, as the reader knows, in

Probability this proportion is merely the limit towards which

the numbers tend in the long run, not the precise result

assigned in any particular case. Hence we can only venture

to say that this is the limit towards which we tend as the

numbers become greater and greater.

This rule, in its general algebraic form, would be expressed

in the language of Probability as follows:--If the

chances of two exclusive or incompatible events be respectively

1/m and 1/n the chance of one or other of them

happening will be 1/m + 1/n or (m + n)/mn. Similarly if there were

more than two events of the kind in question. On the principles

adopted in this work, the rule, when thus algebraically

expressed, means precisely the same thing as when it is

expressed in the statistical form. It was shown at the conclusion

of the last chapter that to say, for example, that the

chance of a given event happening in a certain way is 1/6, is

only another way of saying that in the long run it does tend

to happen in that way once in six times.

It is plain that a sort of corollary to this rule might be

obtained, in precisely the same way, by subtraction instead of

addition. Stated generally it would be as follows:--If the

chance of one or other of two incompatible events be 1/m and

the chance of one alone be 1/n, the chance of the remaining

one will be 1/m - 1/n or (n - m)/nm.

For example, if the chance of any one dying in a year is 1/10,

and his chance of dying of some particular disease is 1/100,

his chance of dying of any other disease is 9/100.

The reader will remark here that there are two apparently

different modes of stating this rule, according as we speak

of 'one or other of two or more events happening,' or of 'the

same event happening in one or other of two or more ways.'

But no confusion need arise on this ground; either way of

speaking is legitimate, the difference being merely verbal,

and depending (as was shown in the first chapter, §8) upon

whether the distinctions between the 'ways' are or are not

too deep and numerous to entitle the event to be conventionally

regarded as the same.

We may also here point out the justification for the common

doctrine that certainty is represented by unity, just as

any given degree of probability is represented by its appropriate

fraction. If the statement that an event happens once

in m times, is equivalently expressed by saying that its chance

is 1/m, it follows that to say that it happens m times in m times,

or every time without exception, is equivalent to

saying that its chance is m/m or 1. Now an event that happens

every time is of course one of whose occurrence we are

certain; hence the fraction which represents the 'chance' of

an event which is certain becomes unity.

It will be equally obvious that given that the chance that

an event will happen is 1/m, the chance that it will not happen

is 1 - 1/m or (m - 1)/m.

3. (2) We can also make inferences by multiplication

or division. Suppose that two events instead of being

incompatible, are connected together in the sense that one

is contingent upon the occurrence of the other. Let us be

told that a given proportion of the members of the series

possess a certain property, and a given proportion again of

these possess another property, then the proportion of the

whole which possess both properties will be found by multiplying

together the two fractions which represent the above

two proportions. Of the inhabitants of London, twenty-five

in a thousand, say, will die in the course of the year; we

suppose it to be known also that one death in five is due to

fever; we should then infer that one in 200 of the inhabitants

will die of fever in the course of the year. It would of course

be equally simple, by division, to make a sort of converse

inference. Given the total mortality per cent. of the population

from fever, and the proportion of fever cases to the

aggregate of other cases of mortality, we might have inferred,

by dividing one fraction by the other, what was the total

mortality per cent. from all causes.

The rule as given above is variously expressed in the

language of Probability. Perhaps the simplest and best

statement is that it gives us the rule of dependent events.

That is; if the chance of one event is 1/m, and the chance that

if it happens another will also happen 1/n, then the chance

of the latter is 1/mn. In this case it is assumed that the latter

is so entirely dependent upon the former that though it does

not always happen with it, it certainly will not happen without

it; the necessity of this assumption however may be

obviated by saying that what we are speaking of in the

latter case is the \_joint\_ event, viz. both together if they are

simultaneous events, or the latter \_in consequence of\_ the

former, if they are successive.

4. The above inferences are necessary, in the sense in

which arithmetical inferences are necessary, and they do not

demand for their establishment any arbitrary hypothesis.

We assume in them no more than is warranted, and in fact

necessitated by the data actually given to us, and make our

inferences from these data by the help of arithmetic. In the

simple examples given above nothing is required beyond

arithmetic in its most familiar form, but it need hardly be

added that in practice examples may often present themselves

which will require much profounder methods than

these. It may task all the resources of that higher and more

abstract arithmetic known as algebra to extract a solution.

But as the necessity of appeal to such methods as these does

not touch the principles of this part of the subject we need

not enter upon them here.

5. The formula next to be discussed stands upon a

somewhat different footing from the above in respect of its

cogency and freedom from appeal to experience, or to hypothesis.

In the two former instances we considered cases in

which the data were supposed to be given under the conditions

that the properties which distinguished the different kinds of

events whose frequency was discussed, were respectively

known to be disconnected and known to be connected. Let

us now suppose that no such conditions are given to us.

One man in ten, say, has black hair, and one in twelve

is short-sighted; what conclusions could we then draw as

to the chance of any given man having one only of these

two attributes, or neither, or both? It is clearly possible

that the properties in question might be inconsistent with

one another, so as never to be found combined in the same

person; or all the short-sighted might have black hair; or

the properties might be allotted[3] in almost any other proportion

whatever. If we are perfectly ignorant upon these

points, it would seem that no inferences whatever could be

drawn about the required chances.

Inferences however \_are\_ drawn, and practically, in most

cases, quite justly drawn. An escape from the apparent

indeterminateness of the problem, as above described, is

found by assuming that, not merely will one-tenth of the

whole number of men have black hair (for this was given as

one of the data), but also that one-tenth alike of those who

are and who are not short-sighted have black hair. Let us

take a batch of 1200, as a sample of the whole. Now, from

the data which were originally given to us, it will easily be

seen that in every such batch there will be on the average

120 who have black hair, and therefore 1080 who have not.

And here in strict right we ought to stop, at least until we

have appealed again to experience; but we do not stop here.

From data which we assume, we go on to infer that of the 120,

10 (i.e. one-twelfth of 120) will be short-sighted, and

110 (the remainder) will not. Similarly we infer that of the 1080,

90 are short-sighted, and 990 are not. On the whole,

then, the 1200 are thus divided:--black-haired short-sighted, 10;

short-sighted without black hair, 90; black-haired men

who are not short-sighted, 110; men who are neither short-sighted

nor have black hair, 990.

This rule, expressed in its most general form, in the

language of Probability, would be as follows:--If the chances

of a thing being p and q are respectively 1/m and 1/n, then the

chance of its being both p and q is 1/mn, p and not q is (n - 1)/mn,

q and not p is (m - 1)/mn, not p and not q is ((m - 1)(n - 1))/mn,

where p and q are independent. The sum of these chances

is obviously unity; as it ought to be, since one or other of

the four alternatives must necessarily exist.

6. I have purposely emphasized the distinction between

the inference in this case, and that in the two preceding,

to an extent which to many readers may seem unwarranted.

But it appears to me that where a science makes

use, as Probability does, of two such very distinct sources of

conviction as the necessary rules of arithmetic and the

merely more or less cogent ones of Induction, it is hardly

possible to lay too much stress upon the distinction. Few

will be prepared to deny that very arbitrary assumptions

have been made by many writers on the subject, and none

will deny that in the case of what are called 'inverse probabilities'

assumptions are sometimes made which are at least

decidedly open to question. The best course therefore is to

make a pause and stringent enquiry at the point at which the

possibility of such error and doubtfulness first exhibits itself.

These remarks apply to some of the best writers on the subject;

in the case of inferior writers, or those who appeal to

Probability without having properly mastered its principles,

we may go further. It would really not be asserting too

much to say that they seem to think themselves justified in

assuming that where we know nothing about the distribution

of the properties alluded to we must assume them to be distributed

as above described, and therefore apportion our

belief in the same ratio. This is called 'assuming the events

to be independent,' the supposition being made that the rule

will certainly follow from this independence, and that we

have a right, if we know nothing to the contrary, to assume

that the events are independent.

The validity of this last claim has already been discussed

in the first chapter; it is only another of the attempts to

construct \_à priori\_ the series which experience will present to

us, and one for which no such strong defence can be made as

for the equality of heads and tails in the throws of a penny.

But the meaning to be assigned to the 'independence' of the

events in question demands a moment's consideration.

The circumstances of the problem are these. There are

two different qualities, by the presence and absence respectively

of each of which, amongst the individuals of a series,

two distinct pairs of classes of these individuals are produced.

For the establishment of the rule under discussion

it was found that one supposition was both necessary and

sufficient, namely, that the division into classes caused by

each of the above distinctions should subdivide each of the

classes created by the other distinction in the same ratio

in which it subdivides the whole. If the independence be

granted and so defined as to mean this, the rule of course

will stand, but, without especial attention being drawn to

the point, it does not seem that the word would naturally

be so understood.

7. The above, then, being the fundamental rules of

inference in probability, the question at once arises, What is

their relation to the great body of formulæ which are made

use of in treatises upon the science, and in practical applications

of it? The reply would be that these formulæ, in so

far as they properly belong to the science, are nothing else

in reality than applications of the above fundamental rules.

Such applications may assume any degree of complexity, for

owing to the difficulty of particular examples, in the form in

which they actually present themselves, recourse must sometimes

be made to the profoundest theorems of mathematics.

Still we ought not to regard these theorems as being anything

else than convenient and necessary abbreviations of

arithmetical processes, which in practice have become too

cumbersome to be otherwise performed.

This explanation will account for some of the rules as

they are ordinarily given, but by no means for all of them.

It will account for those which are demonstrable by the certain

laws of arithmetic, but not for those which in reality

rest only upon inductive generalizations. And it can hardly

be doubted that many rules of the latter description have

become associated with those of the former, so that in popular

estimation they have been blended into one system, of

which all the separate rules are supposed to possess a similar

origin and equal certainty. Hints have already been frequently

given of this tendency, but the subject is one of

such extreme importance that a separate chapter (that on

Induction) must be devoted to its consideration.

8. In establishing the validity of the above rules, we

have taken as the basis of our investigations, in accordance

with the general scheme of this work, the statistical frequency

of the events referred to; but it was also shown that each

formula, when established, might with equal propriety be expressed

in the more familiar form of a fraction representing

the 'chance' of the occurrence of the particular event. The

question may therefore now be raised, Can those writers who

(as described in the last chapter) take as the primary subject

of the science not the degree of statistical frequency, but the

quantity of belief, with equal consistency make this the basis

of their rules, and so also regard the fraction expressive of

the chance as a merely synonymous expression? De Morgan

maintains that whereas in ordinary logic we suppose the

premises to be absolutely true, the province of Probability is

to study 'the effect which partial belief of the premises produces

with respect to the conclusion.' It would appear

therefore as if in strictness we ought on this view to be able

to determine this consequent diminution at first hand, from

introspection of the mind, that is of the conceptions and

beliefs which it entertains; instead of making any recourse

to statistics to tell us how much we ought to believe the

conclusion.

Any readers who have concurred with me in the general

results of the last chapter, will naturally agree in the conclusion

that nothing deserving the name of logical science can

be extracted from any results of appeal to our consciousness

as to the quantity of belief we entertain of this or that proposition.

Suppose, for example, that one person in 100 dies

on the sea passage out to India, and that one in 9 dies during

a 5 years residence there. It would commonly be said

that the chance that any one, who is now going out, has of

living to start homewards 5 years hence, is 88/100; for his chance

of getting there is 99/100; and of his surviving, if he gets

there, 8/9; hence the result or dependent event is got by

multiplying these fractions together, which gives 88/100. Here

the real basis of the reasoning is statistical, and the processes

or results are merely translated afterwards into fractions.

But can we say the same when we look at the belief side of

the question? I quite admit the psychological fact that we

have degrees of belief, more or less corresponding to the

frequency of the events to which they refer. In the above example,

for instance, we should undoubtedly admit on enquiry

that our belief in the man's return was affected by each of

the risks in question, so that we had less expectation of it

than if he were subject to either risk separately; that is, we

should in some way compound the risks. But what I cannot

recognise is that we should be able to perform the process

with any approach to accuracy without appeal to the statistics,

or that, even supposing we could do so, we should

have any guarantee of the correctness of the result without

similar appeal. It appears to me in fact that but little

meaning, and certainly no security, can be attained by so

regarding the process of inference. The probabilities expressed

as degrees of belief, just as those which are expressed

as fractions, must, when we are put upon our justification,

first be translated into their corresponding facts of statistical

frequency of occurrence of the events, and then the inferences

must be drawn and justified there. This part of

the operation, as we have already shown, is mostly carried

on by the ordinary rules of arithmetic. When we have

obtained our conclusion we may, if we please, translate it

back again into the subjective form, just as we can and do

for convenience into the fractional, but I do not see how the

process of inference can be conceived as taking place in that

form, and still less how any proof of it can thus be given. If

therefore the process of inference be so expressed it must be

regarded as a symbolical process, symbolical of such an inference

about things as has been described above, and it

therefore seems to me more advisable to state and expound

it in this latter form.

\_On Inverse Probability and the Rules required for it.\_

9. It has been already stated that the only fundamental

rules of inference in Probability are the two described

in §§2, 3, but there are of course abundance of derivative

rules, the nature and use of which are best obtained from the

study of any manual upon the subject. One class of these

derivative rules, however, is sufficiently distinct in respect of

the questions to which it may give rise, to deserve special

examination. It involves the distinction commonly recognised

as that between Direct and Inverse Probability. It is

thus introduced by De Morgan:--

"In the preceding chapter we have calculated the chances

of an event, knowing the circumstances under which it is to

happen or fail. We are now to place ourselves in an inverted

position: we know the event, and ask what is the probability

which results from the event in favour of any set of circumstances

under which the same might have happened."[4] The

distinction might therefore be summarily described as that

between finding an effect when we are given the causes, and

finding a cause when we are given effects.

On the principles of the science involved in the definition

which was discussed and adopted in the earlier chapters of

this work, the reader will easily infer that no such distinction

as this can be regarded as fundamental. One common feature

was traced in all the objects which were to be referred to

Probability, and from this feature the possible rules of

inference can be immediately derived. All other distinctions

are merely those of arrangement or management.

But although the distinction is not by any means fundamental,

it is nevertheless true that the practical treatment

of such problems as those principally occurring in Inverse

Probability, does correspond to a very serious source of

ambiguity and perplexity. The arbitrary assumptions which

appear in Direct Probability are not by any means serious,

but those which invade us in a large proportion of the problems

offered by Inverse Probability are both serious and

inevitable.

10. This will be best seen by the examination of

special examples; as any, however simple, will serve our

purpose, let us take the two following:--

(1) A ball is drawn from a bag containing nine black

balls and one white: what is the chance of its being the

white ball?

(2) A ball is drawn from a bag containing ten balls, and

is found to be white; what is the chance of there having

been but that one white ball in the bag?

The class of which the first example is a simple instance

has been already abundantly discussed. The interpretation

of it is as follows: If balls be continually drawn and replaced,

the proportion of white ones to the whole number

drawn will tend towards the fraction 1/10. The contemplated

action is a single one, but we view it as one of the above

series; at least our opinion is formed upon that assumption.

We conclude that we are going to take one of a series of

events which may appear individually fortuitous, but in

which, in the long run, those of a given kind are one-tenth of

the whole; this kind (white) is then singled out by anticipation.

By stating that its chance is 1/10, we merely mean to

assert this physical fact, together with such other mental

facts, emotions, inferences, &c., as may be properly associated

with it.

11. Have we to interpret the second example in a

different way? Here also we have a single instance, but the

nature of the question would seem to decide that the only

series to which it can properly be referred is the following:--Balls

are continually drawn from \_different\_ bags each containing

ten, and are always found to be white; what is ultimately

the proportion of cases in which they will be found to have

been taken from bags with only one white ball in them?

Now it may be readily shown[5] that time has nothing to

do with the question; omitting therefore the consideration

of this element, we have for the two series from which our

opinions in these two examples respectively are to be

formed:--(1) balls of different colours presented to us in a

given ultimate ratio; (2) bags with different contents similarly

presented. From these data respectively we have to

assign their due weight to our anticipations of (1) a white

ball; (2) a bag containing but one white ball. So stated the

problems would appear to be formally identical.

When, however, we begin the practical work of solving

them we perceive a most important distinction. In the first

example there is not much that is arbitrary; balls would

under such circumstance really come out more or less accurately

in the proportion expected. Moreover, in case it

should be objected that it is difficult to prove that they will

do so, it does not seem an unfair demand to say that the balls

are to be 'well-mixed' or 'fairly distributed,' or to introduce

any of the other conditions by which, under the semblance of

judging \_à priori\_, we take care to secure our prospect of a

series of the desired kind. But we cannot say the same in

the case of the second example.

12. The line of proof by which it is generally attempted

to solve the second example is of this kind;--It is

shown that there being one white ball for certain in the bag,

the only possible antecedents are of ten kinds, viz. bags,

each of which contains ten balls, but in which the white

balls range respectively from one to ten in number. This of

course imposes limits upon the kind of terms to be found

in our series. But we want more than such limitations, we

must know the proportions in which these terms are ultimately

found to arrange themselves in the series. Now this

requires an experience about bags which may not, and indeed

in a large proportion of similar cases, cannot, be given

to us. If therefore we are to solve the question at all we

must make an assumption; let us make the following;--\_that

each of the bags described above occurs equally often\_,--and see

what follows. The bags being drawn from equally often, it

does not follow that they will each yield equal numbers of

white balls. On the contrary they will, as in the last

example, yield them in direct proportion to the number of

such balls which they contain. The bag with one white

and nine black will yield a white ball once in ten times; that

with two white, twice; and so on. The result of this, it will

be easily seen, is that in 100 drawings there will be obtained

on the average 55 white balls and 45 black. Now with

those drawings that do not yield white balls we have, by the

question, nothing to do, for that question postulated the

drawing of a white ball as an accomplished fact. The series

we want is therefore composed of those which do yield white.

Now what is the additional attribute which is found in some

members, and in some members only, of this series, and

which we mentally anticipate? Clearly it is the attribute of

having been drawn from a bag which only contained one of

these white balls. Of these there is, out of the 55 drawings,

but one. Accordingly the required chance is 1/55. That is to

say, the white ball will have been drawn from the bag containing

only that one white, once in 55 times.

13. Now, with the exception of the passage in italics,

the process here is precisely the same as in the other example;

it is somewhat longer only because we are not able to

appeal immediately to experience, but are forced to try to

deduce what the result will be, though the validity of this

deduction itself rests, of course, ultimately upon experience.

But the above passage is a very important one. It is scarcely

necessary to point out how arbitrary it is.

For \_is\_ the supposition, that the different specified kinds

of bags are equally likely, the most reasonable supposition

under the circumstances in question? One man may think

it is, another may take a contrary view. In fact in an excellent

manual[6] upon the subject a totally different supposition

is made, at any rate in one example; it is taken for granted

in that instance, not that every possible number of black and

white balls respectively is equally likely, but that every

possible way of getting each number is equally likely, whence

it follows that bags with an intermediate number of black

and white balls are far more likely than those with an extreme

number of either. On this supposition five black

and five white being obtainable in 252 ways against the

ten ways of obtaining one white and nine black, it follows

that the chance that we have drawn from a bag of

the latter description is much less than on the hypothesis

first made. The chance, in fact, becomes now 1/512 instead

of 1/55. In the one case each distinct result is considered

equally likely, in the other every distinct way of getting

each result.

14. Uncertainties of this kind are peculiarly likely to

arise in these inverse probabilities, because when we are

merely given an effect and told to look out for the chance of

some assigned cause, we are often given no clue as to the relative

prevalence of these causes, but are left to determine them

on general principles. Give us either their actual prevalence

in statistics, or the conditions by which such prevalence is

brought about, and we know what to do; but without the

help of such data we are reduced to guessing. In the above

example, if we had been told how the bag had been originally

filled, that is by what process, or under what circumstances,

we should have known what to do. If it had been filled at

random from a box containing equal numbers of black and

white balls, the supposition in Mr Whitworth's example is

the most reasonable; but in the absence of any such information

as this we are entirely in the dark, and the supposition

made in §12 is neither more nor less trustworthy and

reasonable than many others, though it doubtless possesses

the merit of superior simplicity.[7] If the reader will recur to

Ch. V. §§4, 5, he will find this particular difficulty fully

explained. Everybody practically admits that a certain

characteristic arrangement or distribution has to be introduced

at some prior stage; and that, as soon as this stage

has been selected, there are no further theoretic difficulties

to be encountered. But when we come to decide, in examples

of the class in question, at what stage it is most reasonable

to make our postulate, we are often left without any very

definite or rational guidance.

15. When, however, we take what may be called, by

comparison with the above purely artificial examples, instances

presented by nature, much of this uncertainty will disappear,

and then all real distinction between direct and inverse

probability will often vanish. In such cases the causes are

mostly determined by tolerably definite rules, instead of

being a mere cloud-land of capricious guesses. We may

either find their relative frequency of occurrence by reference

to tables, or may be able to infer it by examination of

the circumstances under which they are brought about.

Almost any simple example would then serve to illustrate

the fact that under such circumstances the distinction

between direct and inverse probability disappears altogether,

or merely resolves itself into one of \_time\_, which, as will be

more fully shown in a future chapter, is entirely foreign to

our subject.

It is not of course intended to imply that difficulties

similar to those mentioned above do not occasionally invade

us here also. As already mentioned, they are, if not inherent

in the subject, at any rate almost unavoidable in comparison

with the simpler and more direct procedure of determining

what is likely to follow from assigned conditions. What is

meant is that so long as we confine ourselves within the

comparatively regular and uniform field of natural sequences

and co-existences, statistics of causes may be just as readily

available as those of effects. There will not be much more

that is arbitrary in the one than in the other. But of course

this security is lost when, as will be almost immediately

noticed, what may be called metaphysical rather than natural

causes are introduced into the enquiry.

For instance, it is known that in London about 20 people

die per thousand each year. Suppose it also known that of

every 100 deaths there are about 4 attributable to bronchitis.

The odds therefore against any unknown person dying of

bronchitis in a given year are 1249 to 1. Exactly the same

statistics are available to solve the inverse problem:--A man

is dead, what is the chance that he died of bronchitis? Here,

since the man's death is taken for granted, we do not require

to know the general average mortality. All that we want is

the proportional mortality from the disease in question as

given above. If Probability dealt only with inferences

founded in this way upon actual statistics, and these tolerably

extensive, it is scarcely likely that any distinction such

as this between direct and inverse problems would ever have

been drawn.

16. Considered therefore as a contribution to the theory

of the subject, the distinction between Direct and Inverse Probability

must be abandoned. When the appropriate statistics

are at hand the two classes of problems become identical

in method of treatment, and when they are not we have no

more right to extract a solution in one case than in the other.

The discussion however may serve to direct renewed attention

to another and far more important distinction. It will

remind us that there is one class of examples to which the

calculus of Probability is rightfully applied, because statistical

data are all we have to judge by; whereas there are other

examples in regard to which, if we will insist upon making

use of these rules, we may either be deliberately abandoning

the opportunity of getting far more trustworthy information

by other means, or we may be obtaining solutions about

matters on which the human intellect has no right to any

definite quantitative opinion.

17. The nearest approach to any practical justification

of such judgments that I remember to have seen is afforded

by cases of which the following example is a specimen:--

"Of 10 cases treated by Lister's method, 7 did well and 3 suffered

from blood-poisoning: of 14 treated with ordinary

dressings, 9 did well and 5 had blood-poisoning; what are

the odds that the success of Lister's method was due to

chance?".[8] Or, to put it into other words, a short experience

has shown an actual superiority in one method over the

other: what are the chances that an indefinitely long experience,

under similar conditions, will confirm this superiority?

The proposer treated this as a 'bag and balls' problem,

analogous to the following: 10 balls from one bag gave

7 white and 3 black, 14 from another bag gave 9 white and

5 black: what is the chance that the actual ratio of white to

black balls was greater in the former than in the latter?--this

actual ratio being of course considered a true indication

of what would be the ultimate proportions of white and black

drawings. This seems to me to be the only reasonable way

of treating the problem, if it is to be considered capable of

numerical solution at all.

Of course the inevitable assumption has to be made here

about the equal prevalence of the different possible kinds of

bag,--or, as the supporters of the justice of the calculation

would put it, of the obligation to assume the equal \_à priori\_

likelihood of each kind,--but I think that in this particular

example the arbitrariness of the assumption is less than

usual. This is because the problem discusses simply a

balance between two extremely similar cases, and there is a

certain set-off against each other of the objectionable assumptions

on each side. Had \_one\_ set of experiments only been

proposed, and had we been asked to evaluate the probability

of continued repetition of them confirming their verdict, I

should have felt all the scruples I have already mentioned.

But here we have got two sets of experiments carried on

under almost exactly similar circumstances, and there is

therefore less arbitrariness in assuming that their unknown

conditions are tolerably equally prevalent.

18. Examples of the description commonly introduced

seem objectionable enough, but if we wish to realize to its

full extent the vagueness of some of the problems submitted

to this Inverse Probability, we have not far to seek. In

natural as in artificial examples, where statistics are unattainable

the enquiry becomes utterly hopeless, and all attempts

at laying down rules for calculation must be abandoned.

Take, for instance, the question which has given rise to some

discussion,[9] whether such and such groups of stars are or are

not to be regarded as the results of an accidental distribution;

or the still wider and vaguer question, whether such and

such things, or say the world itself, have been produced by

chance?

In cases of this kind the insuperable difficulty is in determining

what sense exactly is to be attached to the words

'accidental' and 'random' which enter into the discussion.

Some account was given, in the fourth chapter, of their

scientific and conventional meaning in Probability. There

seem to be the same objections to generalizing them out of

such relation, as there is in metaphysics to talking of the

Infinite or the Absolute. Infinite magnitude, or infinite

power, one can to some extent comprehend, or at least one

may understand what is being talked about, but '\_the\_ infinite'

seems to me a term devoid of meaning. So of anything

supposed to have been produced at random: tell us the

nature of the agency, the limits of its randomness and so on,

and we can venture upon the problem, but without such data

we know not what to do. The further consideration of such

a problem might, I think, without arrogance be relegated to

the Chapter on Fallacies. Accordingly any further remarks

which I have to make upon the subject will be found there,

and at the conclusion of the chapter on Causation and

Design.

1. It might be more accurate to speak of 'incompatible hypotheses with

respect to any individual case', or 'mutually exclusive classes of

events'.

2. The examples, of this kind, referring to human mortality are taken

from the Carlisle tables. These differ considerably, as is well

known, from other tables, but we have the high authority of De

Morgan for regarding them as the best representative of the average

mortality of the English middle classes at the present day.

3. I say, \_almost\_ any proportion, because, as may easily be seen,

arithmetic imposes certain restrictions upon the assumptions that

can be made. We could not, for instance, suppose that all the

black-haired men are short-sighted, for in any given batch of men

the former are more numerous. But the range of these restrictions is

limited, and their existence is not of importance in the above

discussion.

4. \_Essay on Probabilities\_, p. 53. I have been reminded that in his

article on Probability in the \_Encyclopædia Metropolitana\_ he has

stated that such rules involve no new principle.

5. This point will be fully discussed in a future chapter, after the

general stand-point of an objective system of logic has been

explained and illustrated.

6. Whitworth's \_Choice and Chance\_, Ed. II., p. 123. See also Boole's

\_Laws of Thought\_, p. 370.

7. Opinions differ about the defence of such suppositions, as they do

about the nature of them. Some writers, admitting the above

assumption to be doubtful, call it the most impartial

hypothesis. Others regard it as a sort of mean hypothesis.

8. \_Educational Times\_; Reprint, Vol. xxxvii. p. 40. The question was

proposed by Dr. Macalister and gave rise to considerable controversy.

As usual with problems of this inverse kind hardly any two of the

writers were in agreement as to the assumptions to be made, or

therefore as to the numerical estimate of the odds.

9. See Todhunter's \_History\_, pp. 333, 4.

There is an interesting discussion upon this question by the late

J. D. Forbes in a paper in the \_Philosophical Magazine\_ for

Dec. 1850. It was replied to in a subsequent number by Prof. Donkin.

CHAPTER VIII.

\_THE RULE OF SUCCESSION.\_[1]

1. A word of apology may be offered here for the introduction of a new

name. The only other alternative would have been to entitle the rule

one of \_Induction\_. But such a title I cannot admit, for reasons

which will be almost immediately explained.

1. In the last chapter we discussed at some length the

nature of the kinds of inference in Probability which correspond

to those termed, in Logic, immediate and mediate inferences.

We ascertained what was the meaning of saying, for

example, that the chance of any given man A. B. dying

in a year is 1/3, when concluded from the general proposition

that one man out of three in his circumstances dies. We

also discussed the nature and evidence of rules of a more

completely inferential character. But to stop at this point

would be to take a very imperfect view of the subject. If

Probability is a science of real inference about things, it

must surely lead up to something more than such merely

formal conclusions; we must be able, if not by means of it, at

any rate by some means, to step beyond the limits of what

has been actually observed, and to draw conclusions about

what is as yet unobserved. This leads at once to the question,

What is the connection of Probability with Induction?

This is a question into which it will be necessary to enter

now with some minuteness.

That there is a close connection between Probability and

Induction, must have been observed by almost every one

who has treated of either subject; I have not however seen

any account of this connection that seemed to me to be

satisfactory. An explicit description of it should rather be

sought in treatises upon the narrower subject, Probability;

but it is precisely here that the most confusion is to be

found. The province of Probability being somewhat narrow,

incursions have been constantly made from it into the adjacent

territory of Induction. In this way, amongst the

arithmetical rules discussed in the last chapter, others have

been frequently introduced which ought not in strictness to

be classed with them, as they rest on an entirely different

basis.

2. The origin of such confusion is easy of explanation;

it arises, doubtless, from the habit of laying undue

stress upon the \_subjective\_ side of Probability, upon that

which treats of the quantity of our belief upon different

subjects and the variations of which that quantity is susceptible.

It has been already urged that this variation of

belief is at most but a constant accompaniment of what is

really essential to Probability, and is moreover common to

other subjects as well. By defining the science therefore

from this side these other subjects would claim admittance

into it; some of these, as Induction, have been accepted, but

others have been somewhat arbitrarily rejected. Our belief

in a wider proposition gained by Induction is, prior to verification,

not so strong as that of the narrower generalization

from which it is inferred. This being observed, a so-called

rule of probability has been given by which it is supposed

that this diminution of assent could in many instances be

calculated.

But \_time\_ also works changes in our conviction; our belief

in the happening of almost every event, if we recur to it long

afterwards, when the evidence has faded from the mind, is

less strong than it was at the time. Why are not rules of

oblivion inserted in treatises upon Probability? If a man is

told how firmly he ought to expect the tide to rise again,

because it has already risen ten times, might he not also ask

for a rule which should tell him how firm should be his belief

of an event which rests upon a ten years' recollection?[1] The

infractions of a rule of this latter kind could scarcely be more

numerous and extensive, as we shall see presently, than those

of the former confessedly are. The fact is that the agencies,

by which the strength of our conviction is modified, are so

indefinitely numerous that they cannot all be assembled into

one science; for purposes of definition therefore the quantity

of belief had better be omitted from consideration, or at any

rate regarded as a mere appendage, and the science, defined

from the other or statistical side of the subject, in which,

as has been shown, a tolerably clear boundary-line can be

traced.

3. Induction, however, from its importance does merit

a separate discussion; a single example will show its bearing

upon this part of our subject. We are considering the prospect

of a given man, A. B. living another year, and we find

that nine out of ten men of his age do survive. In forming

an opinion about his surviving, however, we shall find that

there are in reality two very distinct causes which aid in

determining the strength of our conviction; distinct, but in

practice so intimately connected that we are very apt to

overlook one, and attribute the effect entirely to the other.

(I.) There is that which strictly belongs to Probability;

that which (as was explained in Chap VI.) measures our

belief of the individual case as deduced from the general

proposition. Granted that nine men out of ten of the kind

to which A. B. belongs do live another year, it obviously

does not follow at all necessarily that \_he\_ will. We describe

this state of things by saying, that our belief of his surviving

is diminished from certainty in the ratio of 10 to 9, or, in

other words, is measured by the fraction 9/10.

(II.) But are we certain that nine men out of ten like

him \_will\_ live another year? we know that they have so survived

in time past, but will they continue to do so? Since

A. B. is still alive it is plain that this proposition is to a

certain extent assumed, or rather obtained by Induction.

We cannot however be as certain of the inductive inference

as we are of the data from which it was inferred. Here,

therefore, is a second cause which tends to diminish our

belief; in practice these two causes always accompany each

other, but in thought they can be separated.

The two distinct causes described above are very liable

to be confused together, and the class of cases from which

examples are necessarily for the most part drawn increases

this liability. The step from the statement 'all men have

died in a certain proportion' to the inference 'they will continue

to die in that proportion' is so slight a step that it is

unnoticed, and the diminution of conviction that should

accompany it is unsuspected. In what are called \_à priori\_

examples the step is still slighter. We feel so certain about

the permanence of the laws of mechanics, that few people

would think of regarding it as an inference when they

believe that a die will in the long run turn up all its faces

equally often, because other dice have done so in time

past.

4. It has been already pointed out (in Chapter VI.)

that, so far as concerns that definition of Probability which

regards it as the science which discusses the degree and

modifications of our belief, the question at issue seems to be

simply this:--Are the causes alluded to above in (II.) capable

of being reduced to one simple coherent scheme, so that any

universal rules for the modification of assent can be obtained

from them? If they are, strong grounds will have been

shown for classing them with (I.), in other words, for considering

them as rules of probability. Even then they

would be rules practically of a very different kind, contingent

instead of necessary (if one may use these terms without

committing oneself to any philosophical system), but this

objection might perhaps be overruled by the greater simplicity

secured by classing them together. This view is, with

various modifications, generally adopted by writers on Probability,

or at least, as I understand the matter, implied by

their methods of definition and treatment. Or, on the other

hand, must these causes be regarded as a vast system, one

might almost say a chaos, of perfectly distinct agencies;

which may indeed be classified and arranged to some extent,

but from which we can never hope to obtain any rules of

perfect generality which shall not be subject to constant

exception? If so, but one course is left; to exclude them

all alike from Probability. In other words, we must assume

the general proposition, viz. that which has been described

throughout as our starting-point, to be given to us; it may

be obtained by any of the numerous rules furnished by

Induction, or it may be inferred deductively, or given by our

own observation; its value may be diminished by its depending

upon the testimony of witnesses, or its being recalled by

our own memory. Its real value may be influenced by

these causes or any combinations of them; but all these are

preliminary questions with which we have nothing directly

to do. We assume our statistical proposition to be true,

neglecting the diminution of its value by the process of

attainment; we take it up first at this point and then apply

our rules to it. We receive it in fact, if one may use the

expression, \_ready-made\_, and ask no questions about the process

or completeness of its manufacture.

5. It is not to be supposed, of course, that any writers

have seriously attempted to reduce to one system of calculation

all the causes mentioned above, and to embrace in one

formula the diminution of certainty to which the inclusion of

them subjects us. But on the other hand, they have been

unwilling to restrain themselves from all appeal to them.

From an early period in the study of the science attempts

have been made to proceed, by the Calculus of Probability,

from the observed cases to adjacent and similar cases. In

practice, as has been already said, it is not possible to avoid

some extension of this kind. But it should be observed,

that in these instances the divergence from the strict ground

of experience is not in reality recognized, at least not as a

part of our logical procedure. We have, it is true, wandered

somewhat beyond it, and so obtained a wider proposition

than our data strictly necessitated, and therefore one of less

certainty. Still we assume the conclusion given by induction

to be equally certain with the data, or rather omit all

notice of the divergence from consideration. It is assumed

that the unexamined instances will resemble the examined,

an assumption for which abundant warrant may exist; the

theory of the calculation rests upon the supposition that

there will be no difference between them, and the practical

error is insignificant simply because this difference is small.

6. But the rule we are now about to discuss, and

which may be called the Rule of Succession, is of a very

different kind. It not only recognizes the fact that we are

leaving the ground of past experience, but takes the consequences

of this divergence as the express subject of its calculation.

It professes to give a general rule for the measure of

expectation that we should have of the reappearance of a

phenomenon that has been already observed any number of

times. This rule is generally stated somewhat as follows:

"To find the chance of the recurrence of an event already

observed, divide the number of times the event has been

observed, increased by one, by the same number increased

by two."

7. It will be instructive to point out the origin of

this rule; if only to remind the reader of the necessity of

keeping mathematical formulæ to their proper province,

and to show what astonishing conclusions are apt to

be accepted on the supposed warrant of mathematics.

Revert then to the example of Inverse Probability on p. 182.

We saw that under certain assumptions, it would

follow that when a single white ball had been drawn

from a bag known to contain 10 balls which were white

or black, the chance could be determined that there was

only one white ball in it. Having done this we readily

calculate 'directly' the chance that this white ball will

be drawn next time. Similarly we can reckon the chances

of there being two, three, &c. up to ten white balls in it,

and determine on each of these suppositions the chance

of a white ball being drawn next time. Adding these

together we have the answer to the question:--a white

ball has been drawn once from a bag known to contain

ten balls, white or black; what is the chance of a second

time drawing a white ball?

So far only arithmetic is required. For the next step we

need higher mathematics, and by its aid we solve this

problem:--A white ball has been drawn m times from a

bag which contains any number, we know not what, of

balls each of which is white or black, find the chance of

the next drawing also yielding a white ball. The answer is

(m + 1)/(m + 2).

Thus far mathematics. Then comes in the physical

assumption that the universe may be likened to such a bag

as the above, in the sense that the above rule may be

applied to solve this question:--an event has been observed

to happen m times in a certain way, find the chance that

it will happen in that way next time. Laplace, for instance,

has pointed out that at the date of the writing of his \_Essai

Philosophique\_, the odds in favour of the sun's rising again

(on the old assumption as to the age of the world) were

1,826,214 to 1. De Morgan says that a man who standing

on the bank of a river has seen ten ships pass by with flags

should judge it to be 11 to 1 that the next ship will also

carry a flag.

8. It is hard to take such a rule as this seriously, for

there does not seem to be even that moderate confirmation

of it which we shall find to hold good in the case of the

application of abstract formulæ to the estimation of the

evidence of witnesses. If however its validity is to be discussed

there appear to be two very distinct lines of enquiry

along which we may be led.

(1) In the first place we may take it for what it professes

to be, and for what it is commonly understood to

be, viz. a rule which assigns the measure of expectation

we ought to entertain of the recurrence of the event under

the circumstances in question. Of course, on the view

adopted in this work, we insist on enquiring whether it is

really true that on the average events do thus repeat their

performance in accordance with this law. Thus tested, no

one surely would attempt to defend such a formula. So

far from past occurrence being a ground for belief in future

recurrence, there are (as will be more fully pointed out

in the Chapter on Fallacies) plenty of cases in which the

direct contrary holds good. Then again a rule of this kind

is subject to the very serious perplexity to be explained

in our next chapter, arising out of the necessary arbitrariness

of such inverse reference. That is, when an event has

happened but a few times, we have no certain guide; and

when it has happened but once,[2] we have no guide whatever,

as to the class of cases to which it is to be referred. In

the example above, about the flags, why did we stop short at

this notion simply, instead of specifying the size, shape, &c.

of the flags?

De Morgan, it must be remembered, only accepts this rule

in a qualified sense. He regards it as furnishing a

\_minimum\_ value for the amount of our expectation. He

terms it "the rule of probability of a \_pure induction\_," and

says of it, "The probabilities shown by the above rules

are merely \_minima\_ which may be augmented by other

sources of knowledge." That is, he recognizes only those

instances in which our belief in the Uniformity of Nature

and in the existence of special laws of causation comes in

to supplement that which arises from the mere frequency

of past occurrence. This however does not meet those cases

in which past occurrence is a positive ground of disbelief in

future recurrence.

9. (2) There is however another and very different

view which might be taken of such a rule. It is one, an

obscure recognition of which has very likely had much to

do with the acceptance which the rule has received.

What we might suppose ourselves to be thus expressing

is,--not the measure of rational expectation which might be

held by minds sufficiently advanced to be able to classify

and to draw conscious inferences, but,--the law according to

which the primitive elements of belief were started and

developed. Of course such an interpretation as this would

be equivalent to quitting the province of Logic altogether

and crossing over into that of Psychology; but it would be a

perfectly valid line of enquiry. We should be attempting

nothing more than a development of the researches of

Fechner and his followers in psychophysical measurement.

Only then we ought, like them, not to start with any analogy

of a ballot box and its contents, but to base our enquiry

on careful determination of the actual mental phenomena

experienced. We know how the law has been determined

in accordance with which the intensity of the feeling of

\_light\_ varies with that of its objective source. We see how it

is possible to measure the growth of \_memory\_ according to

the number of repetitions of a sentence or a succession

of mere syllables. In this latter case, for instance, we just

try experiments, and determine how much better a man can

remember any utterances after eight hearings than after

seven.[3]

Now this case furnishes a very close parallel to our

supposed attempt to measure the increase of intensity of

belief after repeated recurrence. That is, if it were possible

to experiment in this order of mental phenomena, we ought

simply to repeat a phenomenon a certain number of times

and then ascertain by actual introspection or by some simple

test, how fast the belief was increasing. Thus viewed the

problem seems to me a hopeless one. The difficulties are

serious enough, when we are trying to measure our simple

sensations, of laying aside the effects of past training, and of

attempting, as it were, to leave the mind open and passive

to mere reception of stimuli. But if we were to attempt

in this way to measure our belief these difficulties would

become quite insuperable. We can no more divest ourselves

of past training here than we can of intelligence or

thought. I do not see how any one could possibly avoid

classing the observed recurrences with others which he had

experienced, and of being thus guided by special analogies

and inductions instead of trusting solely to De Morgan's

'pure induction'. The same considerations tend to rebut

another form of defence for the rule in question. It is

urged, for instance, that we may at least resort to it in

those cases in which we are in entire ignorance as to the

number and nature of the antecedents. This is a position to

which I can hardly conceive it possible that we should ever

be reduced. However remote or exceptional may be the

phenomenon selected we may yet bring it into relation with

some accepted generalizations and thus draw our conclusions

from these rather than from purely \_à priori\_ considerations.

10. Since then past acquisitions cannot be laid aside

or allowed for, the only remaining resource would be to

experiment upon the infant mind. One would not like

to pronounce that any line of enquiry is impossible; but

the difficulties would certainly be enormous. And interesting

as the facts would be, supposing that we had succeeded

in securing them, they would not be of the slightest importance

in Logic. However the question were settled:--whether,

for instance, we proved that the sentiment or

emotion of belief grew up slowly and gradually from a sort

of zero point under the impress of repetition of experience;

or whether we proved that a single occurrence produced

complete belief in the repetition of the event, so that

experience gradually untaught us and weakened our convictions;--in

no case would the mature mind gain any aid

as to what it ought to believe.

I cannot but think that some such view as this must

occasionally underlie the acceptance which this rule has received.

For instance, Laplace, though unhesitatingly adopting

it as a real, that is, objective rule of inference, has gone

into so much physiological and psychological matter towards

the end of his discussion (\_Essai philosophique\_) as to suggest

that what he had in view was the natural history of belief

rather than its subsequent justification.

Again, the curious doctrine adopted by Jevons, that the

principles of Induction rest entirely upon the theory of

Probability,--a very different doctrine from that which is

conveyed by saying that all knowledge of facts is \_probable\_

only, i.e. not necessary,--seems unintelligible except on some

such interpretation. We shall have more to say on this

subject in our next chapter. It will be enough here to

remark that in our present reflective and rational stage

we find that every inference in Probability involves some

appeal to, or support from, Induction, but that it is impossible

to base either upon the other. However far back

we try to push our way, and however disposed we might be

to account for our ultimate beliefs by Association, it seems

to me that so long as we consider ourselves to be dealing

with rules of inference we must still distinguish between

Induction and Probability.

1. John Craig, in his often named work, \_Theologiæ Christianæ

Principia Mathematica\_ (Lond. 1699) attempted something in this

direction when he proposed to solve such problems as:--Quando

evanescet probabilitas cujusvis Historiæ, cujus subjectum est

transiens, vivâ tantum voce transmissæ, determinare.

2. When m = 1 the fraction becomes 2/3; i.e. the odds are 2 to 1 in

favour of recurrence. And there are writers who accept this

result. For instance, Jevons (\_Principles of Science\_ p. 258) says

"Thus on the first occasion on which a person sees a shark, and

notices that it is accompanied by a little pilot fish, the odds are

2 to 1 that the next shark will be so accompanied." To say nothing

of the fact that recognizing and naming the fish implies that they

have often been seen before, how many of the observed

characteristics of that single 'event' are to be considered

essential? Must the pilot precede; and at the same distance? Must

we consider the latitude, the ocean, the season, the species of

shark, as matter also of repetition on the next occasion? and so

on. I cannot see how the Inductive problem can be even intelligibly

stated, for quantitative purposes, on the first occurrence of any

event.

3. See in \_Mind\_ (x. 454) Mr Jacob's account of the researches of Herr

Ebbinghaus as described in his work \_Ueber das Gedächtniss\_.

CHAPTER IX.

\_INDUCTION AND ITS CONNECTION WITH PROBABILITY.\_

1. We were occupied, during the last chapter, with the

examination of a rule, the object of which was to enable

us to make inferences about instances as yet unexamined.

It was professedly, therefore, a rule of an inductive character.

But, in the form in which it is commonly expressed,

it was found to fail utterly. It is reasonable therefore to

enquire at this point whether Probability is entirely a formal

or deductive science, or whether, on the other hand, we are

able, by means of it, to make valid inferences about instances

as yet unexamined. This question has been already in part

answered by implication in the course of the last two chapters.

It is proposed in the present chapter to devote a fuller

investigation to this subject, and to describe, as minutely as

limits will allow, the nature of the connection between Probability

and Induction. We shall find it advisable for clearness

of conception to commence our enquiry at a somewhat

early stage. We will travel over the ground, however, as

rapidly as possible, until we approach the boundary of what

can properly be termed Probability.

2. Let us then conceive some one setting to work to

investigate nature, under its broadest aspect, with the view

of systematizing the facts of experience that are known, and

thence (in case he should find that this is possible) discovering

others which are at present unknown. He observes a

multitude of phenomena, physical and mental, contemporary

and successive. He enquires what connections are there

between them? what rules can be found, so that some of

these things being observed I can infer others from them?

We suppose him, let it be observed, deliberately resolving to

investigate the things themselves, and not to be turned

aside by any prior enquiry as to there being laws under

which the mind is compelled to judge of the things. This

may arise either from a disbelief in the existence of any

independent and necessary mental laws, and a consequent

conviction that the mind is perfectly competent to observe

and believe anything that experience offers, and should

believe nothing else, or simply from a preference for investigations

of the latter kind. In other words, we suppose him

to reject Formal Logic, and to apply himself to a study of

objective existences.

It must not for a moment be supposed that we are here

doing more than conceiving a fictitious case for the purpose

of more vividly setting before the reader the nature of the

inductive process, the assumptions it has to make, and the

character of the materials to which it is applied. It is not

psychologically possible that any one should come to the study

of nature with all his mental faculties in full perfection, but

void of all materials of knowledge, and free from any bias as

to the uniformities which might be found to prevail around

him. In practice, of course, the form and the matter--the

laws of belief or association, and the objects to which they

are applied--act and react upon one another, and neither

can exist in any but a low degree without presupposing the

existence of the other. But the supposition is perfectly legitimate

for the purpose of calling attention to the requirements

of such a system of Logic, and is indeed nothing more

than what has to be done at almost every step in psychological

enquiry.[1]

3. His task at first might be conceived to be a slow

and tedious one. It would consist of a gradual accumulation

of individual instances, as marked out from one another

by various points of distinction, and connected with one

another by points of resemblance. These would have to be

respectively distinguished and associated in the mind, and

the consequent results would then be summed up in general

propositions, from which inferences could afterwards be

drawn. These inferences could, of course, contain no new

facts, they would only be repetitions of what he or others

had previously observed. All that we should have so far

done would have been to make our classifications of things

and then to appeal to them again. We should therefore be

keeping well within the province of ordinary logic, the processes

of which (whatever their ultimate explanation) may

of course always be expressed, in accordance with Aristotle's

Dictum, as ways of determining whether or not we can show

that one given class is included wholly or partly within

another, or excluded from it, as the case may be.

4. But a very short course of observation would suggest

the possibility of a wide extension of his information.

Experience itself would soon detect that events were connected

together in a regular way; he would ascertain that

there are 'laws of nature.' Coming with no \_à priori\_ necessity

of believing in them, he would soon find that as a matter

of fact they do exist, though he could not feel any certainty

as to the extent of their prevalence. The discovery of this

arrangement in nature would at once alter the plan of his proceedings,

and set the tone to the whole range of his methods

of investigation. His main work now would be to find out

by what means he could best discover these laws of nature.

An illustration may assist. Suppose I were engaged in

breaking up a vast piece of rock, say slate, into small pieces.

I should begin by wearily working through it inch by inch.

But I should soon find the process completely changed owing

to the existence of \_cleavage\_. By this arrangement of things

a very few blows would do the work--not, as I might possibly

have at first supposed, to the extent of a few inches--but

right through the whole mass. In other words, by the

process itself of cutting, as shown in experience, and by

nothing else, a constitution would be detected in the things

that would make that process vastly more easy and extensive.

Such a discovery would of course change our tactics.

Our principal object would thenceforth be to ascertain the

extent and direction of this cleavage.

Something resembling this is found in Induction. The

discovery of laws of nature enables the mind to dart with its

inferences from a few facts completely through a whole class

of objects, and thus to acquire results the successive individual

attainment of which would have involved long and

wearisome investigation, and would indeed in multitudes

of instances have been out of the question. We have no

demonstrative proof that this state of things is universal;

but having found it prevail extensively, we go on with the

resolution at least to try for it everywhere else, and we are

not disappointed. From propositions obtained in this way,

or rather from the original facts on which these propositions

rest, we can make \_new\_ inferences, not indeed with absolute

certainty, but with a degree of conviction that is of the

utmost practical use. We have gained the great step of

being able to make trustworthy generalizations. We conclude,

for instance, not merely that John and Henry die,

but that all men die.

5. The above brief investigation contains, it is hoped,

a tolerably correct outline of the nature of the Inductive

inference, as it presents itself in Material or Scientific Logic.

It involves the distinction drawn by Mill, and with which

the reader of his \_System of Logic\_ will be familiar, between

an inference drawn \_according\_ to a formula and one drawn

\_from\_ a formula. We do in reality make our inference from

the data afforded by experience directly to the conclusion;

it is a mere arrangement of convenience to do so by passing

through the generalization. But it is one of such extreme

convenience, and one so necessarily forced upon us when we

are appealing to our own past experience or to that of others

for the grounds of our conclusion, that practically we find it

the best plan to divide the process of inference into two

parts. The first part is concerned with establishing the

generalization; the second (which contains the rules of ordinary

logic) determines what conclusions can be drawn from

this generalization.

6. We may now see our way to ascertaining the province

of Probability and its relation to kindred sciences.

Inductive Logic gives rules for discovering such generalizations

as those spoken of above, and for testing their correctness.

If they are expressed in universal propositions it is

the part of ordinary logic to determine what inferences can

be made from and by them; if, on the other hand, they are

expressed in proportional propositions, that is, propositions

of the kind described in our first chapter, they are handed

over to Probability. We find, for example, that three infants

out of ten die in their first four years. It belongs to Induction

to say whether we are justified in generalizing our observation

into the assertion, All infants die in that proportion.

When such a proposition is obtained, whatever may be the

value to be assigned to it, we recognize in it a series of a

familiar kind, and it is at once claimed by Probability.

In this latter case the division into two parts, the inductive

and the ratiocinative, seems decidedly more than one of

convenience; it is indeed imperatively necessary for clearness

of thought and cogency of treatment. It is true that in

almost every example that can be selected we shall find

both of the above elements existing together and combining

to determine the degree of our conviction, but when we come

to examine them closely it appears to me that the grounds

of their cogency, the kind of conviction they produce, and

consequently the rules which they give rise to, are so entirely

distinct that they cannot possibly be harmonized into

a single consistent system.

The opinion therefore according to which certain Inductive

formulæ are regarded as composing a portion of

Probability, and which finds utterance in the Rule of Succession

criticised in our last chapter, cannot, I think, be

maintained. It would be more correct to say, as stated

above, that Induction is quite distinct from Probability, yet

co-operates in almost all its inferences. By Induction we

determine, for example, whether, and how far, we can safely

generalize the proposition that four men in ten live to be

fifty-six; supposing such a proposition to be safely generalized,

we hand it over to Probability to say what sort of inferences

can be deduced from it.

7. So much then for the opinion which tends to regard

pure Induction as a subdivision of Probability. By the

majority of philosophical and logical writers a widely different

view has of course been entertained. They are mostly disposed

to distinguish these sciences very sharply from, not to

say to contrast them with, one another; the one being

accepted as philosophical or logical, and the other rejected

as mathematical. This may without offence be termed the

popular prejudice against Probability.

A somewhat different view, however, must be noticed

here, which, by a sort of reaction against the latter, seems

even to go beyond the former; and which occasionally finds

expression in the statement that all inductive reasoning of

every kind is merely a matter of Probability. Two examples

of this may be given.

Beginning with the older authority, there is an often

quoted saying by Butler at the commencement of his \_Analogy\_,

that 'probability is the very guide of life'; a saying

which seems frequently to be understood to signify that the

rules or principles of Probability are thus all-prevalent when

we are drawing conclusions in practical life. Judging by

the drift of the context, indeed, this seems a fair interpretation

of his meaning, in so far of course as there could

be said to be any such thing as a science of Probability in

those days. Prof. Jevons, in his \_Principles of Science\_

(p. 197), has expressed a somewhat similar view, of course

in a way more consistent with the principles of modern

science, physical and mathematical. He says, "I am convinced

that it is impossible to expound the methods of induction

in a sound manner, without resting them on the

theory of Probability. Perfect knowledge alone can give

certainty, and in nature perfect knowledge would be infinite

knowledge, which is clearly beyond our capacities. We have,

therefore, to content ourselves with partial knowledge,--knowledge

mingled with ignorance, producing doubt."[2]

8. There are two senses in which this disposition to

merge the two sciences into one may be understood. Using

the word Probability in its vague popular signification,

nothing more may be intended than to call attention to the

fact, that in every case alike our conclusions are nothing

more than 'probable,' that is, that they are not, and cannot

be, absolutely certain. This must be fully admitted, for of

course no one acquainted with the complexity of physical

and other evidence would seriously maintain that absolute

ideal certainty can be attained in any branch of applied

logic. Hypothetical certainty, in abstract science, may be

possible, but not absolute certainty in the domain of the

concrete. This has been already noticed in a former chapter,

where, however, it was pointed out that whatever justification

may exist, on the subjective view of logic, for regarding

this common prevalence of absence of certainty as warranting

us in fusing the sciences into one, no such justification is

admitted when we take the objective view.

9. What may be meant, however, is that the \_grounds\_

of this absence of certainty are always of the same general

character. This argument, if admitted, would have real

force, and must therefore be briefly noticed. We have seen

abundantly that when we say of a conclusion within the

strict province of Probability, that it is not certain, all that

we mean is that in some proportion of cases only will such

conclusion be right, in the other cases it will be wrong.

Now when we say, in reference to any inductive conclusion,

that we feel uncertain about its absolute cogency, are we

conscious of the same interpretation? It seems to me that

we are not. It is indeed quite possible that on ultimate

analysis it might be proved that experience of failure in

the past employment of our methods of investigation was

the main cause of our present want of perfect confidence in

them. But this, as we have repeatedly insisted, does not

belong to the province of logical, but to that of Psychological

enquiry. It is surely not the case that we are, as a rule,

consciously guided by such occasional or repeated instances

of past failure. In so far as they are at all influential, they

seem to do their work by infusing a vague want of confidence

which cannot be referred to any statistical grounds for

its justification, at least not in a quantitative way. Part of

our want of confidence is derived sympathetically from those

who have investigated the matter more nearly at first hand.

Here again, analysis might detect that a given proportion of

past failures lay at the root of the distrust, but it does not

show at the surface. Moreover, one reason why we cannot

feel perfectly certain about our inductions is, that the

\_memory\_ has to be appealed to for some of our data; and will

any one assert that the only reason why we do not place

absolute reliance on our memory of events long past is that

we have been deceived in that way before?

In any other sense, therefore, than as a needful protest

against attaching too great demonstrative force to the conclusions

of Inductive Logic, it seems decidedly misleading to

speak of its reasonings as resting upon Probability.

10. We may now see clearly the reasons for the

limits within which causation[3] is necessarily required, but

beyond which it is not needed. To be able to generalize

a formula so as to extend it from the observed to the unobserved,

it is clearly essential that there should be a certain

permanence in the order of nature; this permanence is one

form of what is implied in the term causation. If the

circumstances under which men live and die remaining the

same, we did not feel warranted in inferring that four men

out of ten would continue to live to fifty, because in the case

of those whom we had observed this proportion had hitherto

done so, it is clear that we should be admitting that the

same antecedents need not be followed by the same consequents.

This uniformity being what the Law of Causation

asserts, the truth of the law is clearly necessary to enable us

to obtain our generalizations: in other words, it is necessary

for the Inductive part of the process. But it seems to be

equally clear that causation is not necessary for that part of

the process which belongs to Probability. Provided only

that the truth of our generalizations is secured to us, in the

way just mentioned, what does it matter to us whether or

not the individual members are subject to causation? For

it is not in reality about these individuals that we make

inferences. As this last point has been already fully treated

in Chapter VI., any further allusion to it need not be made here.

11. The above description, or rather indication, of the

process of obtaining these generalizations must suffice for the

present. Let us now turn and consider the means by which

we are practically to make use of them when they are obtained.

The point which we had reached in the course of the

investigations entered into in the sixth and seventh chapters

was this:--Given a series of a certain kind, we could draw

inferences about the members which composed it; inferences,

that is, of a peculiar kind, the value and meaning of which

were fully discussed in their proper place.

We must now shift our point of view a little; instead of

starting, as in the former chapters, with a determinate series

supposed to be given to us, let us assume that the individual

only is given, and that the work is imposed upon us of finding

out the appropriate series. How are we to set about the

task? In the former case our data were of this kind:--Eight

out of ten men, aged fifty, will live eleven years more,

and we ascertained in what sense, and with what certainty,

we could infer that, say, John Smith, aged fifty, would live

to sixty-one.

12. Let us then suppose, instead, that John Smith

presents himself, how should we in this case set about obtaining

a series for him? In other words, how should we

collect the appropriate statistics? It should be borne in

mind that when we are attempting to make real inferences

about things as yet unknown, it is in this form that the

problem will practically present itself.

At first sight the answer to this question may seem to be

obtained by a very simple process, viz. by counting how

many men of the age of John Smith, respectively do and do

not live for eleven years. In reality however the process is

far from being so simple as it appears. For it must be remembered

that each individual thing has not one distinct

and appropriate class or group, to which, and to which alone,

it properly belongs. We may indeed be practically in the

habit of considering it under such a single aspect, and it may

therefore seem to us more familiar when it occupies a place

in one series rather than in another; but such a practice is

merely customary on our part, not obligatory. It is obvious

that every individual thing or event has an indefinite

number of properties or attributes observable in it, and

might therefore be considered as belonging to an indefinite

number of different classes of things. By belonging to any

one class it of course becomes at the same time a member of

all the higher classes, the genera, of which that class was a

species. But, moreover, by virtue of each accidental attribute

which it possesses, it becomes a member of a class

intersecting, so to say, some of the other classes. John Smith

is a consumptive man say, and a native of a northern climate.

Being a man he is of course included in the class of vertebrates,

also in that of animals, as well as in any higher

such classes that there may be. The property of being consumptive

refers him to another class, narrower than any of

the above; whilst that of being born in a northern climate

refers him to a new and distinct class, not conterminous with

any of the rest, for there are things born in the north which

are not men.

13. When therefore John Smith presents himself to

our notice without, so to say, any particular label attached to

him informing us under which of his various aspects he is to

be viewed, the process of thus referring him to a class becomes

to a great extent arbitrary. If he had been indicated

to us by a general name, that, of course, would have been

some clue; for the name having a determinate connotation

would specify at any rate a fixed group of attributes within

which our selection was to be confined. But names and

attributes being connected together, we are here supposed

to be just as much in ignorance what name he is to be

called by, as what group out of all his innumerable attributes

is to be taken account of; for to tell us one of these things

would be precisely the same in effect as to tell us the other.

In saying that it is thus arbitrary under which class he is

placed, we mean, of course, that there are no logical grounds

of decision; the selection must be determined by some extraneous

considerations. Mere inspection of the individual

would simply show us that he could equally be referred

to an indefinite number of classes, but would in itself give

no inducement to prefer, for our special purpose, one of these

classes to another.

This variety of classes to which the individual may be

referred owing to his possession of a multiplicity of attributes,

has an important bearing on the process of inference

which was indicated in the earlier sections of this chapter,

and which we must now examine in more special reference

to our particular subject.

14. It will serve to bring out more clearly the nature

of some of those peculiarities of the step which we are now

about to take in the case of Probability, if we first examine

the form which the corresponding step assumes in the case

of ordinary Logic. Suppose then that we wished to ascertain

whether a certain John Smith, a man of thirty, who is

amongst other things a resident in India, and distinctly

affected with cancer, will continue to survive there for

twenty years longer. The terms in which the man is thus

introduced to us refer him to different classes in the way

already indicated. Corresponding to these classes there will

be a number of propositions which have been obtained by

previous observations and inductions, and which we may

therefore assume to be available and ready at hand when

we want to make use of them. Let us conceive them to

be such as these following:--Some men live to fifty; some

Indian residents live to fifty; no man suffering thus from

cancer lives for five years. From the first and second of these

premises nothing whatever can be inferred, for they are both[4]

particular propositions, and therefore lead to no conclusion

in this case. The third answers our enquiry decisively.

To the logical reader it will hardly be necessary to point

out that the process here under consideration is that of

finding middle terms which shall serve to connect the

subject and predicate of our conclusion. This subject and

predicate in the case in question, are the individual before

us and his death within the stated period. Regarded by

themselves there is nothing in common between them, and

therefore no link by which they may be connected or disconnected

with each other. The various classes above

referred to are a set of such middle terms, and the propositions

belonging to them are a corresponding set of major

premises. By the help of any one of them we are enabled,

under suitable circumstances, to connect together the subject

and predicate of the conclusion, that is, to infer whether the

man will or will not live twenty years.

15. Now in the performance of such a logical process

there are two considerations to which the reader's attention

must for a moment be directed. They are simple enough in

this case, but will need careful explanation in the corresponding

case in Probability. In the first place, it is clear that

whenever we can make any inference at all, we can do so

with absolute certainty. Logic, within its own domain,

knows nothing of hesitation or doubt. If the middle term

is appropriate it serves to connect the extremes in such a

way as to preclude all uncertainty about the conclusion;

if it is not, there is so far an end of the matter: no conclusion

can be drawn, and we are therefore left where we were.

Assuming our premises to be correct, we either know our

conclusion for certain, or we know nothing whatever about

it. In the second place, it should be noticed that none of

the possible alternatives in the shape of such major premises

as those given above can ever contradict any of the others,

or be at all inconsistent with them. Regarded as isolated

propositions, there is of course nothing to secure such harmony;

they have very different predicates, and may seem

quite out of each other's reach for either support or opposition.

But by means of the other premise they are in each

case brought into relation with one another, and the general

interests of truth and consistency prevent them therefore

from contradicting one another. As isolated propositions

it might have been the case that all men live to fifty, and

that no Indian residents do so, but having recognised that

some men are residents in India, we see at once that these

premises are inconsistent, and therefore that one or other

of them must be rejected. In all applied logic this necessity

of avoiding self-contradiction is so obvious and imperious

that no one would think it necessary to lay down the formal

postulate that all such possible major premises are to be

mutually consistent. To suppose that this postulate is not

complied with, would be in effect to make two or more contradictory

assumptions about matters of fact.

16. But now observe the difference when we attempt

to take the corresponding step in Probability. For ordinary

propositions, universal or particular, substitute statistical

propositions of what we have been in the habit of calling

the 'proportional' kind. In other words, instead of asking

whether the man will live for twenty years, let us ask whether

he will live for one year? We shall be unable to find any

universal propositions which will cover the case, but we may

without difficulty obtain an abundance of appropriate proportional

ones. They will be of the following description:--Of

men aged 30, 98 in 100 live another year; of residents in

India a smaller proportion survive, let us for example say

90 in 100; of men suffering from cancer a smaller proportion

still, let us say 20 in 100.

Now in both of the respects to which attention has just

been drawn, propositions of this kind offer a marked contrast

with those last considered. In the first place, they do

not, like ordinary propositions, either assert unequivocally

yes or no, or else refuse to open their lips; but they give

instead a sort of qualified or hesitating answer concerning

the individuals included in them. This is of course nothing

more than the familiar characteristic of what may be called

'probability propositions.' But it leads up to, and indeed

renders possible, the second and more important point;

viz. that these various answers, though they cannot directly

and formally contradict each other (this their nature as proportional

propositions, will not as a rule permit), may yet, in

a way which will now have to be pointed out, be found to be

more or less in conflict with each other.

Hence it follows that in the attempt to draw a conclusion

from premises of the kind in question, we may be placed

in a position of some perplexity; but it is a perplexity

which may present itself in two forms, a mild and an aggravated

form. We will notice them in turn.

17. The mild form occurs when the different classes

to which the individual case may be appropriately referred

are successively included one within another; for here our

sets of statistics, though leading to different results, will

not often be found to be very seriously at variance with

one another. All that comes of it is that as we ascend in the

scale by appealing to higher and higher genera, the statistics

grow continually less appropriate to the particular

case in point, and such information therefore as they afford

becomes gradually less explicit and accurate.

The question that we originally wanted to determine,

be it remembered, is whether John Smith will die within

one year. But all knowledge of this fact being unattainable,

owing to the absence of suitable inductions, we felt

justified (with the explanation, and under the restrictions

mentioned in Chap VI.), in substituting, as the only available

equivalent for such individual knowledge, the answer to the

following statistical enquiry, What proportion of men in his

circumstances die?

18. But then at once there begins to arise some doubt

and ambiguity as to what exactly is to be understood by his

circumstances. We may know very well what these circumstances

are in themselves, and yet be in perplexity as to

how many of them we ought to take into account when

endeavouring to estimate his fate. We might conceivably,

for a beginning, choose to confine our attention to those

properties only which he has in common with all animals.

If so, and statistics on the subject were attainable, they

would presumably be of some such character as this, Ninety-nine

animals out of a hundred die within a year. Unusual as

such a reference would be, we should, logically speaking, be

doing nothing more than taking a wider class than the one

we were accustomed to. Similarly we might, if we pleased,

take our stand at the class of vertebrates, or at that of

mammalia, if zoologists were able to give us the requisite

information. Of course we reject these wide classes and

prefer a narrower one. If asked why we reject them, the

natural answer is that they are so general, and resemble the

particular case before us in so few points, that we should be

exceedingly likely to go astray in trusting to them. Though

accuracy cannot be insured, we may at least avoid any needless

exaggeration of the relative number and magnitude of

our errors.

19. The above answer is quite valid; but whilst cautioning

us against appealing to too wide a class, it seems to

suggest that we cannot go wrong in the opposite direction,

that is in taking too narrow a class. And yet we do avoid

any such extremes. John Smith is not only an Englishman;

he may also be a native of such a part of England, be living

in such a Presidency, and so on. An indefinite number of

such additional characteristics might be brought out into

notice, many of which at any rate have some bearing upon

the question of vitality. Why do we reject any consideration

of these narrower classes? We do reject them, but it is for

what may be termed a practical rather than a theoretical

reason. As was explained in the first chapters, it is essential

that our series should contain a considerable number of terms

if they are to be of any service to us. Now many of the

attributes of any individual are so rare that to take them

into account would be at variance with the fundamental

assumption of our science, viz. that we are properly concerned

only with the averages of large numbers. The more special

and minute our statistics the better, provided only that we

can get enough of them, and so make up the requisite large

number of instances. This is, however, impossible in many

cases. We are therefore obliged to neglect one attribute

after another, and so to enlarge the contents of our class; at

the avowed risk of somewhat increased variety and unsuitability

in the members of it, for at each step of this kind we

diverge more and more from the sort of instances that we

really want. We continue to do so, until we no longer gain

more in quantity than we lose in quality. We finally take

our stand at the point where we first obtain statistics drawn

from a sufficiently large range of observation to secure the

requisite degree of stability and uniformity.

20. In such an example as the one just mentioned,

where one of the successive classes--man--is a well-defined

natural kind or species, there is such a complete break in

each direction at this point, that every one is prompted to

take his stand here. On the one hand, no enquirer would

ever think of introducing any reference to the higher classes

with fewer attributes, such as animal or organized being:

and on the other hand, the inferior classes, created by our

taking notice of his employment or place of residence, &c.,

do not as a rule differ sufficiently in their characteristics

from the class \_man\_ to make it worth our while to attend

to them.

Now and then indeed these characteristics do rise into

importance, and whenever this is the case we concentrate

our attention upon the class to which they correspond, that

is, the class which is marked off by their presence. Thus,

for instance, the quality of consumptiveness separates any

one off so widely from the majority of his fellow-men in all

questions pertaining to mortality, that statistics about the

lives of consumptive men differ materially from those which

refer to men in general. And we see the result; if a consumptive

man can effect an insurance at all, he must do it

for a much higher premium, calculated upon his special

circumstances. In other words, the attribute is sufficiently

important to mark off a fresh class or series. So with insurance

against accident. It is not indeed attempted to

make a special rate of insurance for the members of each

separate trade, but the differences of risk to which they are

liable oblige us to take such facts to some degree into

account. Hence, trades are roughly divided into two or

three classes, such as the ordinary, the hazardous, and the

extra-hazardous, each having to pay its own rate of premium.

21. Where one or other of the classes thus corresponds

to natural kinds, or involves distinctions of co-ordinate importance

with those of natural kinds, the process is not

difficult; there is almost always some one of these classes

which is so universally recognised to be the appropriate one,

that most persons are quite unaware of there being any

necessity for a process of selection. Except in the cases

where a man has a sickly constitution, or follows a dangerous

employment, we seldom have occasion to collect statistics for

him from any class but that of men in general of his age in

the country.

When, however, these successive classes are not ready

marked out for us by nature, and thence arranged in easily

distinguishable groups, the process is more obviously arbitrary.

Suppose we were considering the chance of a man's

house being burnt down, with what collection of attributes

should we rest content in this instance? Should we include

all kinds of buildings, or only dwelling-houses, or confine

ourselves to those where there is much wood, or those which

have stoves? All these attributes, and a multitude of others

may be present, and, if so, they are all circumstances which

help to modify our judgment. We must be guided here by

the statistics which we happen to be able to obtain in

sufficient numbers. Here again, rough distinctions of this

kind are practically drawn in Insurance Offices, by dividing

risks into ordinary, hazardous, and extra-hazardous. We

examine our case, refer it to one or other of these classes,

and then form our judgment upon its prospects by the statistics

appropriate to its class.

22. So much for what may be called the mild form in

which the ambiguity occurs; but there is an aggravated form

in which it may show itself, and which at first sight seems

to place us in far greater perplexity.

Suppose that the different classes mentioned above are

not included successively one within the other. We may

then be quite at a loss which of the statistical tables to

employ. Let us assume, for example, that nine out of ten

Englishmen are injured by residence in Madeira, but that

nine out of ten consumptive persons are benefited by such a

residence. These statistics, though fanciful, are conceivable

and perfectly compatible. John Smith is a consumptive

Englishman; are we to recommend a visit to Madeira in his

case or not? In other words, what inferences are we to

draw about the probability of his death? Both of the statistical

tables apply to his case, but they would lead us to

directly contradictory conclusions. This does not mean, of

course, contradictory precisely in the logical sense of that

word, for one of these propositions does not assert that an

event must happen and the other deny that it must; but

contradictory in the sense that one would cause us in some

considerable degree to believe what the other would cause us

in some considerable degree to disbelieve. This refers, of

course, to the individual events; the statistics are by supposition

in no degree contradictory. Without further data,

therefore, we can come to no decision.

23. Practically, of course, if we were forced to a decision

with only these data before us, we should make our

choice by the consideration that the state of a man's lungs

has probably more to do with his health than the place of

his birth has; that is, we should conclude that the duration

of life of consumptive Englishmen corresponds much more

closely with that of consumptive persons in general than

with that of their healthy countrymen. But this is, of

course, to import empirical considerations into the question.

The data, as they are given to us, and if we confine ourselves

to them, leave us in absolute uncertainty upon the

point. It may be that the consumptive Englishmen almost

all die when transported into the other climate; it may be

that they almost all recover. If they die, this is in obvious

accordance with the first set of statistics; it will be found in

accordance with the second set through the fact of the

foreign consumptives profiting by the change of climate in

more than what might be termed their due proportion.

A similar explanation will apply to the other alternative,

viz. to the supposition that the consumptive Englishmen

mostly recover. The problem is, therefore, left absolutely

indeterminate, for we cannot here appeal to any general rule

so simple and so obviously applicable as that which, in a

former case, recommended us always to prefer the more

special statistics, when sufficiently extensive, to those which

are wider and more general. We have no means here of

knowing whether one set is more special than the other.

And in this no difficulty can be found, so long as we

confine ourselves to a just view of the subject. Let me

again recall to the reader's mind what our present position

is; we have substituted for knowledge of the individual

(finding that unattainable) a knowledge of what occurs in

the average of similar cases. This step had to be taken the

moment the problem was handed over to Probability. But

the conception of similarity in the cases introduces us to a

perplexity; we manage indeed to evade it in many instances,

but here it is inevitably forced upon our notice.

There are here two aspects of this similarity, and they

introduce us to two distinct averages. Two assertions are

made as to what happens in the long run, and both of these

assertions, by supposition, are verified. Of their truth there

need be no doubt, for both were supposed to be obtained

from experience.

24. It may perhaps be supposed that such an example

as this is a \_reductio ad absurdum\_ of the principle upon which

Life and other Insurances are founded. But a moment's

consideration will show that this is quite a mistake, and

that the principle of insurance is just as applicable to

examples of this kind as to any other. An office need find

no difficulty in the case supposed. They \_might\_ (for a reason

to be mentioned presently, they probably \_would\_ not) insure

the individual without inconsistency at a rate determined

by either average. They might say to him, "You are an

Englishman. Out of the multitude of English who come to

us nine in ten die if they go to Madeira. We will insure

you at a rate assigned by these statistics, knowing that in

the long run all will come right so far as we are concerned.

You are also consumptive, it is true, and we do not know

what proportion of the English are consumptive, nor what

proportion of English consumptives die in Madeira. But

this does not really matter for our purpose. The formula,

nine in ten die, is in reality calculated by taking into account

these unknown proportions; for, though we do not know

them in themselves, statistics tell us all that we care to

know about their results. In other words, whatever unknown

elements may exist, must, in regard to all the effects

which they can produce, have been already taken into

account, so that our ignorance about them cannot in the

least degree invalidate such conclusions as we are able to

draw. And this is sufficient for our purpose." But precisely

the same language might be held to him if he presented

himself as a consumptive man; that is to say, the office

could safely carry on its proceedings upon either alternative.

This would, of course, be a very imperfect state for the

matter to be left in. The only rational plan would be to

isolate the case of consumptive Englishmen, so as to make

a separate calculation for their circumstances. This calculation

would then at once supersede all other tables so

far as they were concerned; for though, \_in the end\_, it could

not arrogate to itself any superiority over the others, it

would in the mean time be marked by fewer and slighter

aberrations from the truth.

25. The real reason why the Insurance office could

not long work on the above terms is of a very different

kind from that which some readers might contemplate, and

belongs to a class of considerations which have been much

neglected in the attempts to construct sciences of the different

branches of human conduct. It is nothing else than

that annoying contingency to which prophets since the time

of Jonah have been subject, of uttering \_suicidal\_ prophecies;

of publishing conclusions which are perfectly certain when

every condition and cause but one have been taken into

account, that one being the effect of the prophecy itself

upon those to whom it refers.

In our example above, the office (in so far as the particular

cases in Madeira are concerned) would get on very well

until the consumptive Englishmen in question found out

what much better terms they could make by announcing

themselves as consumptives, and paying the premium appropriate

to that class, instead of announcing themselves as

Englishmen. But if they did this they would of course be

disturbing the statistics. The tables were based upon the

assumption that a certain fixed proportion (it does not

matter what proportion) of the English lives would continue

to be consumptive lives, which, under the supposed circumstances,

would probably soon cease to be true. When it is

said that nine Englishmen out of ten die in Madeira, it is

meant that of those who come to the office, as the phrase is,

at random, or in their fair proportions, nine-tenths die. The

consumptives are supposed to go there just like red-haired

men, or poets, or any other special class. Or they might go

in any proportions greater or less than those of other classes,

so long as they adhered to the same proportion throughout.

The tables are then calculated on the continuance of this

state of things; the practical contradiction is in supposing

such a state of things to continue after the people had once

had a look at the tables. If we merely make the assumption

that the publication of these tables made no such alteration

in the conduct of those to whom it referred, no hitch of

this kind need occur.

26. The assumptions here made, as has been said, are

not in any way contradictory, but they need some explanation.

It will readily be seen that, taken together, they are

inconsistent with the supposition that each of these classes is

homogeneous, that is, that the statistical proportions which

hold of the whole of either of them will also hold of any

portion of them which we may take. There are certain

individuals (viz. the consumptive Englishmen) who belong

to each class, and of course the two different sets of statistics

cannot both be true of them taken by themselves. They

might coincide in their characteristics with either class, but

not with both; probably in most practical cases they will

coincide with neither, but be of a somewhat intermediate

character. Now when it is said of any such heterogeneous

body that, say, nine-tenths die, what is meant (or rather

implied) is that the class might be broken up into smaller

subdivisions of a more homogeneous character, in some of

which, of course, more than nine-tenths die, whilst in others

less, the differences depending upon their character, constitution,

profession, &c.; the number of such divisions and the

amount of their divergence from one another being perhaps

very considerable.

Now when we speak of either class as a whole and say

that nine-tenths die, the most natural and soundest meaning

is that that would be the proportion if all without

exception went abroad, or (what comes to the same thing) if

each of these various subdivisions was represented in fair

proportion to its numbers. Or it might only be meant that

they go in some other proportion, depending upon their

tastes, pursuits, and so on. But whatever meaning be adopted

one condition is necessary, viz. that the proportion of each

class that went at the time the statistics were drawn up

must be adhered to throughout. When the class is homogeneous

this is not needed, but when it is heterogeneous the

statistics would be interfered with unless this condition were

secured.

We are here supposed to have two sets of statistics, one

for the English and one for the consumptives, so that the

consumptive English are in a sense counted twice over. If

their mortality is of an intermediate amount, therefore, they

serve to keep down the mortality of one class and to keep

up that of the other. If the statistics are supposed to be

exhaustive, by referring to the whole of each class, it follows

that actually the same individuals must be counted each

time; but if representatives only of each class are taken, the

same individuals need not be inserted in each set of tables.

27. When therefore they come to insure (our remarks

are still confined to our supposed Madeira case), we have

some English consumptives counted as English, and paying

the high rate; and others counted as consumptives and paying

the low rate. Logically indeed we may suppose them all

entered in each class, and paying therefore each rate. What

we have said above is that any individual may be conceived

to present himself for either of these classes. Conceive that

some one else pays his premium for him, so that it is a

matter of indifference to him personally at which rate he

insures, and there is nothing to prevent some of the class (or

for that matter all) going to one class, and others (or all

again) going to the other class.

So long therefore as we make the logically possible

though practically absurd supposition that some men will

continue to pay a higher rate than they need, there is nothing

to prevent the English consumptives (some or all) from

insuring in each category and paying its appropriate premium.

As soon as they gave any thought to the matter, of

course they would, in the case supposed, all prefer to insure

as consumptives. But their doing this would disturb each set

of statistics. The English mortality in Madeira would instantly

become heavier, so far as the Insurance company was

concerned, by the loss of all their best lives; whilst the consumptive

statistics (unless \_all\_ the English consumptives had

already been taken for insurance) would be in the same way

deteriorated.[5] A slight readjustment therefore of each scale

of insurance would then be needed; this is the disturbance

mentioned just above. It must be clearly understood, however,

that it is not our original statistics which have proved

to be inconsistent, but simply that there were practical

obstacles to carrying out a system of insurance upon them.

28. Examples subject to the difficulty now under

consideration will doubtless seem perplexing to the student

unacquainted with the subject. They are difficult to reconcile

with any other view of the science than that insisted on

throughout this Essay, viz. that we are only concerned with

averages. It will perhaps be urged that there are two

different values of the man's life in these cases, and that

they cannot both be true. Why not? The 'value' of his

life is simply the number of years to which men in his

circumstances do, on the average, attain; we have the man

set before us under two different circumstances; what wonder,

therefore, that these should offer different averages? In such

an objection it is forgotten that we have had to substitute

for the unattainable result about the individual, the really

attainable result about a set of men as much like him as

possible. The difficulty and apparent contradiction only

arise when people will try to find some justification for their

belief in the individual case. What can we possibly conclude,

it may be asked, about this particular man John

Smith's prospects when we are thus offered two different

values for his life? Nothing whatever, it must be replied;

nor could we in reality draw a conclusion, be it remembered,

in the former case, when we were practically confined to one

set of statistics. There also we had what we called the

'value' of his life, and since we only knew of one such value,

we came to regard it as in some sense appropriate to him as

an individual. Here, on the other hand, we have two values,

belonging to different series, and as these values are really

different it may be complained that they are discordant, but

such a complaint can only be made when we do what we

have no right to do, viz. assign a value to the individual

which shall admit of individual justification.

29. Is it then perfectly arbitrary what series or class

of instances we select by which to judge? By no means; it

has been stated repeatedly that in choosing a series, we must

seek for one the members of which shall resemble our individual

in as many of his attributes as possible, subject only

to the restriction that it must be a sufficiently extensive

series. What is meant is, that in the above case, where we

have two series, we cannot fairly call them contradictory; the

only valid charge is one of incompleteness or insufficiency for

their purpose, a charge which applies in exactly the same

sense, be it remembered, to all statistics which comprise

genera unnecessarily wider than the species with which we

are concerned. The only difference between the two different

classes of cases is, that in the one instance we are on a

path which we know will lead at the last, through many

errors, towards the truth (in the sense in which truth can be

attained here), and we took it for want of a better. In the

other instance we have two such paths, perfectly different

paths, either of which however will lead us towards the truth

as before. Contradiction can only seem to arise when it is

attempted to justify each separate step on our paths, as well

as their ultimate tendency.

Still it cannot be denied that these objections are a

serious drawback to the completeness and validity of any

anticipations which are merely founded upon statistical frequency,

at any rate in an early stage of experience, when

but few statistics have been collected. Such knowledge as

Probability can give is not in any individual case of a high

order, being subject to the characteristic infirmity of repeated

error; but even when measured by its own standard

it commences at a very low stage of proficiency. The

errors are then relatively very numerous and large compared

with what they may ultimately be reduced to.

30. Here as elsewhere there is a continuous process

of specialization going on. The needs of a gradually widening

experience are perpetually calling upon us to subdivide

classes which are found to be too heterogeneous. Sometimes

the only complaint that has to be made is that the class to

which we are obliged to refer is found to be somewhat too

broad to suit our purpose, and that it might be subdivided

with convenience. This is the case, as has been shown above,

when an Insurance office finds that its increasing business

makes it possible and desirable to separate off the men who

follow some particular trades from the rest of their fellow-countrymen.

Similarly in every other department in which

statistics are made use of. This increased demand for specificness

leads, in fact, as naturally in this direction, as does

the progress of civilization to the subdivision of trades in

any town or country. So in reference to the other kind of

perplexity mentioned above. Nothing is more common in

those sciences or practical arts, in which deduction is but

little available, and where in consequence our knowledge is

for the most part of the empirical kind, than to meet with

suggestions which point more or less directly in contrary

directions. Whenever some new substance is discovered or

brought into more general use, those who have to deal with

it must be familiar with such a state of things. The medical

man who has to employ a new drug may often find himself

confronted by the two distinct recommendations, that on

the one hand it should be employed for certain diseases, and

that on the other hand it should not be tried on certain constitutions.

A man with such a constitution, but suffering

from such a disease, presents himself; which recommendation

is the doctor to follow? He feels at once obliged to

set to work to collect narrower and more special statistics,

in order to escape from such an ambiguity.

31. In this and a multitude of analogous cases

afforded by the more practical arts it is not of course necessary

that numerical data should be quoted and appealed

to; it is sufficient that the judgment is more or less consciously

determined by them. All that is necessary to make

the examples appropriate is that we should admit that in

their case statistical data are our ultimate appeal in the

present state of knowledge. Of course if the empirical

laws can be resolved into their component causes we may

appeal to direct deduction, and in this case the employment

of statistics, and consequently the use of the theory of

Probability, may be superseded.

In this direction therefore, as time proceeds, the advance

of statistical refinement by the incessant subdivision of classes

to meet the developing wants of man is plain enough. But

if we glance backwards to a more primitive stage, we shall

soon see in what a very imperfect state the operation commences.

At this early stage, however, Probability and Induction

are so closely connected together as to be very apt to

be merged into one, or at any rate to have their functions

confounded.

32. Since the generalization of our statistics is found to

belong to Induction, this process of generalization may be

regarded as prior to, or at least independent of, Probability.

We have, moreover, already discussed (in Chapter VI.) the

step corresponding to what are termed immediate inferences,

and (in Chapter VII.) that corresponding to syllogistic inferences.

Our present position therefore is that in which we

may consider ourselves in possession of any number of generalizations,

but wish to employ them so as to make inferences

about a given individual; just as in one department of

common logic we are engaged in finding middle terms to

establish the desired conclusion. In this latter case the

process is found to be extremely simple, no accumulation of

different middle terms being able to lead to any real ambiguity

or contradiction. In Probability, however, the case is

different. Here, if we attempt to draw inferences about the

individual case before us, as often is attempted--in the Rule

of Succession for example--we shall encounter the full force

of this ambiguity and contradiction. Treat the question,

however, fairly, and all difficulty disappears. Our inference

really is not about the individuals as individuals, but about

series or successions of them. We wished to know whether

John Smith will die within the year; this, however, cannot

be known. But John Smith, by the possession of many

attributes, belongs to many different series. The multiplicity

of middle terms, therefore, is what ought to be

expected. We \_can\_ know whether a succession of men, residents

in India, consumptives, &c. die within a year. We

may make our selection, therefore, amongst these, and in the

long run the belief and consequent conduct of ourselves and

other persons (as described in Chapter VI.) will become

capable of justification. With regard to choosing one of

these series rather than another, we have two opposing

principles of guidance. On the one hand, the more special

the series the better; for, though not more right in the end,

we shall thus be more nearly right all along. But, on the

other hand, if we try to make the series too special, we shall

generally meet the practical objection arising from insufficient

statistics.

1. Some of my readers may be familiar with a very striking digression

in Buffon's Natural History (\_Natural Hist. of Man\_, §VIII.), in

which he supposes the first man in full possession of his faculties,

but with all his experience to gain, and speculates on the gradual

acquisition of his knowledge. Whatever may be thought of his

particular conclusions the passage is very interesting and

suggestive to any student of Psychology.

2. See also Dugald Stewart (Ed. by Hamilton; VII. pp. 115-119).

3. Required that is for purposes of logical inference within the

limits of Probability; it is not intended to imply any doubts as to

its actual universal prevalence, or its all-importance for

scientific purposes. The subject is more fully discussed in a future

chapter.

4. As particular propositions they are both of course identical in

form. The fact that the 'some' in the former corresponds to a

larger proportion than in the latter, is a distinction alien to pure

Logic.

5. The reason is obvious. The healthiest English lives in Madeira

(viz. the consumptive ones) have now ceased to be reckoned as

English; whereas the worst consumptive lives there (viz. the

English) are now increased in relative numbers.

CHAPTER X.

\_CHANCE AS OPPOSED TO CAUSATION AND DESIGN.\_

1. The remarks in the previous chapter will have served

to clear the way for an enquiry which probably excites more

popular interest than any other within the range of our subject,

viz. the determination whether such and such events are to

be attributed to Chance on the one hand, or to Causation or

Design on the other. As the principal difficulty seems to

arise from the ambiguity with which the problem is generally

conceived and stated, owing to the extreme generality of the

conceptions involved, it becomes necessary to distinguish

clearly between the several distinct issues which are apt to

be involved.

I. There is, to begin with, a very old objection, founded

on the assumption which our science is supposed to make of

the existence of \_Chance\_. The objection against chance is

of course many centuries older than the Theory of Probability;

and as it seems a nearly obsolete objection at the

present day we need not pause long for its consideration.

If we spelt the word with a capital C, and maintained that

it was representative of some distinct creative or administrative

agency, we should presumably be guilty of some form

of Manicheism. But the only rational meaning of the objection

would appear to be that the principles of the science

compel us to assume that events (some events, only, that is)

happen without causes, and are thereby removed from the

customary control of the Deity. As repeatedly pointed out

already this is altogether a mistake. The science of Probability

makes no assumption whatever about the way in

which events are brought about, whether by causation or

without it. All that we undertake to do is to establish and

explain a body of rules which are applicable to classes of

cases in which we do not or cannot make inferences about

the individuals. The objection therefore must be somewhat

differently stated, and appears finally to reduce itself

to this:--that the assumptions upon which the science of

Probability rests, are not inconsistent with a disbelief in

causation within certain limits; causation being of course

understood simply in the sense of regular sequence. So

stated the objection seems perfectly valid, or rather the facts

on which it is based must be admitted; though what connection

there would be between such lack of causation and

absence of Divine superintendence I quite fail to see.

As this Theological objection died away the men of

physical science, and those who sympathized with them,

began to enforce the same protest; and similar cautions are

still to be found from time to time in modern treatises.

Hume, for instance, in his short essay on \_Probability\_, commences

with the remark, "though there be no such thing as

chance in the world, our ignorance of the real cause of any

event has the same influence on the understanding, &c."

De Morgan indeed goes so far as to declare that "the

foundations of the theory of Probability have ceased to exist

in the mind that has formed the conception," "that anything

ever did happen or will happen without some particular

reason why it should have been precisely what it was and

not anything else."[1] Similar remarks might be quoted from

Laplace and others.

2. In the particular form of the controversy above

referred to, and which is mostly found in the region of the

natural and physical sciences, the contention that chance

and causation are irreconcileable occupies rather a defensive

position; the main fact insisted on being that, whenever in

these subjects we may happen to be ignorant of the details

we have no warrant for assuming as a consequence that the

details are uncaused. But this supposed irreconcileability

is sometimes urged in a much more aggressive spirit in reference

to social enquiries. Here the attempt is often made to

prove causation in the details, from the known and admitted

regularity in the averages. A considerable amount of controversy

was excited some years ago upon this topic, in great

part originated by the vigorous and outspoken support of the

necessitarian side by Buckle in his \_History of Civilization\_.

It should be remarked that in these cases the attempt is

sometimes made as it were to startle the reader into acquiescence

by the singularity of the examples chosen. Instances

are selected which, though they possess no greater logical

value, are, if one may so express it, emotionally more effective.

Every reader of Buckle's History, for instance, will remember

the stress which he laid upon the observed fact, that the number

of suicides in London remains about the same, year by year;

and he may remember also the sort of panic with which the

promulgation of this fact was accompanied in many quarters.

So too the way in which Laplace notices that the number of

undirected letters annually sent to the Post Office remains

about the same, and the comments of Dugald Stewart upon

this particular uniformity, seem to imply that they regarded

this instance as more remarkable than many analogous ones

taken from other quarters.

That there is a certain foundation of truth in the reasonings

in support of which the above examples are advanced,

cannot be denied, but their authors appear to me very much

to overrate the sort of opposition that exists between the

theory of Chances and the doctrine of Causation. As regards

first that wider conception of order or regularity which we

have termed uniformity, anything which might be called

objective chance would certainly be at variance with this in

one respect. In Probability ultimate regularity is always

postulated; in tossing a die, if not merely the individual

throws were uncertain in their results, but even the average

also, owing to the nature of the die, or the number of the

marks upon it, being arbitrarily interfered with, of course no

kind of science would attempt to take any account of it.

3. So much must undoubtedly be granted; but must

the same admission be made as regards the succession of the

individual events? Can causation, in the sense of invariable

succession (for we are here shifting on to this narrower

ground), be denied, not indeed without suspicion of scientific

heterodoxy, but at any rate without throwing uncertainty

upon the foundations of Probability? De Morgan, as we

have seen, strongly maintains that this cannot be so. I find

myself unable to agree with him here, but this disagreement

springs not so much from differences of detail, as from those

of the point of view in which we regard the science. He

always appears to incline to the opinion that the individual

judgment in probability is to admit of justification;

that when we say, for instance, that the odds in favour of

some event are three to two, that we can explain and justify

our statement without any necessary reference to a series or

class of such events. It is not easy to see how this can be

done in any case, but the obstacles would doubtless be

greater even than they are, if knowledge of the individual

event were not merely unattained, but, owing to the absence

of any causal connection, essentially unattainable. On the

theory adopted in this work we simply postulate ignorance

of the details, but it is not regarded as of any importance

on what sort of grounds this ignorance is based. It may

be that knowledge is out of the question from the nature

of the case, the causative link, so to say, being missing. It

may be that such links are known to exist, but that either

we cannot ascertain them, or should find it troublesome to

do so. It is the fact of this ignorance that makes us appeal

to the theory of Probability, the grounds of it are of no

importance.

4. On the view here adopted we are concerned only

with averages, or with the single event as deduced from an

average and conceived to form one of a series. We start

with the assumption, grounded on experience, that there is

uniformity in this average, and, so long as this is secured to

us, we can afford to be perfectly indifferent to the fate, as

regards causation, of the individuals which compose the

average. The question then assumes the following form:--Is

this assumption, of average regularity in the aggregate,

inconsistent with the admission of what may be termed

causeless irregularity in the details? It does not seem to me

that it would be at all easy to prove that this is so. As

a matter of fact the two beliefs have constantly co-existed in

the same minds. This may not count for much, but it suggests

that if there be a contradiction between them it is by

no means palpable and obvious. Millions, for instance, have

believed in the general uniformity of the seasons taken one

with another, who certainly did not believe in, and would

very likely have been ready distinctly to deny, the existence

of necessary sequences in the various phenomena which compose

what we call a season. So with cards and dice; almost

every gambler must have recognized that judgment and

foresight are of use in the long run, but writers on chance

seem to think that gamblers need a good deal of reasoning to

convince them that each separate throw is in its nature essentially

predictable.

5. In its application to moral and social subjects,

what gives this controversy its main interest is its real or

supposed bearing upon the vexed question of the freedom

of the will; for in this region Causation, and Fatalism or

Necessitarianism, are regarded as one and the same thing.

Here, as in the last case, that wide and somewhat vague

kind of regularity that we have called Uniformity, must be

admitted as a notorious fact. Statistics have put it out of

the power of any reasonably informed person to feel any

hesitation upon this point. Some idea has already been

gained, in the earlier chapters, of the nature and amount

of the evidence which might be furnished of this fact, and

any quantity more might be supplied from the works of

professed writers upon the subject. If, therefore, Free-will be

so interpreted as to imply such essential irregularity as defies

prediction both in the average, and also in the single case,

then the negation of free-will follows, not as a remote logical

consequence, but as an obvious inference from indisputable

facts of experience.

Few persons, however, would go so far as to interpret it

in this sense. All that troubles them is the fear that somehow

this general regularity may be found to carry with it

causation, certainly in the sense of regular invariable sequence,

and probably also with the further association of

compulsion. Rejecting the latter association as utterly

unphilosophical, I cannot even see that the former consequence

can be admitted as really proved, though it doubtless

gains some confirmation from this source.

6. The nature of the argument against free-will,

drawn from statistics, at least in the form in which it is very

commonly expressed, seems to me exceedingly defective.

The antecedents and consequents, in the case of our volitions,

must clearly be supposed to be very nearly \_immediately\_

in succession, if anything approaching to causation is to be

established: whereas in statistical enquiries the data are

often widely separate, if indeed they do not apply merely

to single groups of actions or results. For instance, in the

case of the misdirected letters, what it is attempted to prove

is that each writer was so much the 'victim of circumstances'

(to use a common but misleading expression) that he could

not have done otherwise than he did under his circumstances.

But really no accumulation of figures to prove that the

number of such letters remains the same year by year, can

have much bearing upon this doctrine, even though they

were accompanied by corresponding figures which should

connect the forgetfulness thus indicated with some other

characteristics in the writers. So with the number of

suicides. If 250 people do, or lately did, annually put an

end to themselves in London, the fact, as it thus stands by

itself, may be one of importance to the philanthropist and

statesman, but it needs bringing into much closer relation

with psychological elements if it is to convince us that the

actions of men are always instances of inflexible order. In

fact, instead of having secured our A and B here in closest

intimacy of succession to one another,--to employ the symbolic

notation commonly used in works on Inductive Logic

to illustrate the causal connection,--we find them separated

by a considerable interval; often indeed we merely

have an A or a B by itself.

7. Again, another deficiency in such reasoning seems

to be the laying undue weight upon the mere regularity or

persistency of the statistics. These may lead to very important

results, but they are not exactly what is wanted

for the purpose of proving anything against the freedom

of the will; it is not indeed easy to see what connection

this has with such facts as that the annual number of thefts

or of suicides remains at pretty nearly the same figure.

Statistical uniformity seems to me to establish nothing else,

at least directly, in the case of human actions, than it does

in that of physical characteristics. Take but one instance,

that of the misdirected letters. We were already aware

that the height, weight, chest measurement, and so on, of

a large number of persons preserved a tolerably regular

average amidst innumerable deflections, and we were prepared

by analogy to anticipate the same regularity in their

mental characteristics. All that we gain, by counting the

numbers of letters which are posted without addresses, is

a certain amount of direct evidence that this is the case.

Just as observations of the former kind had already shown

that statistics of the strength and stature of the human

body grouped themselves about a mean, so do those of the

latter that a similar state of things prevails in respect of the

readiness and general trustworthiness of the memory. The

evidence is not so direct and conclusive in the latter case,

for the memory is not singled out and subjected to measurement

by itself, but is taken in combination with innumerable

other influencing circumstances. Still there can be little

doubt that the statistics tell on the whole in this direction,

and that by duly varying and extending them they may

obtain considerable probative force.

The fact is that Probability has nothing more to do with

Natural Theology, either in its favour or against it, than the

general principles of Logic or Induction have. It is simply a

body of rules for drawing inferences about classes of events

which are distinguished by a certain quality. The believer

in a Deity will, by the study of nature, be led to form an

opinion about His works, and so to a certain extent about

His attributes. But it is surely unreasonable to propose

that he should abandon his belief because the sequence of

events,--not, observe, their general tendency towards happiness

or misery, good or evil,--is brought about in a way

different from what he had expected; whether it be by displaying

order where he had expected irregularity, or by

involving the machinery of secondary causes where he had

expected immediate agency.

8. It is both amusing and instructive to consider

what very different feelings might have been excited in our

minds by this co-existence of, what may be called, ignorance

of individuals and knowledge of aggregates, if they had presented

themselves to our observation in a reverse order.

Being utterly unable to make assured predictions about a

single life, or the conduct of individuals, people are sometimes

startled, and occasionally even dismayed, at the unexpected

discovery that such predictions can be confidently

made when we are speaking of large numbers. And so

some are prompted to exclaim, This is denying Providence!

it is utter Fatalism! But let us assume, for a moment, that

our familiarity with the subject had been experienced, in the

first instance, in reference to the aggregates instead of the

individual lives. It is difficult, perhaps, to carry out such a

supposition completely; though we may readily conceive

something approaching to it in the case of an ignorant clerk

in a Life Assurance Office, who had never thought of life, except

as having such a 'value' at such an age, and who had

hardly estimated it except in the form of averages. Might

we not suppose him, in some moment of reflectiveness, being

astonished and dismayed at the sudden realization of the

utter uncertainty in which the single life is involved? And

might not his exclamation in turn be, Why this is denying

Providence! It is utter chaos and chance! A belief in a

Creator and Administrator of the world is not confined to

any particular assumption about the nature of the immediate

sequence of events, but those who have been accustomed

hitherto to regard the events under one of the aspects above

referred to, will often for a time feel at a loss how to connect

them with the other.

9. So far we have been touching on a very general

question; viz. the relation of the fundamental postulates of

Probability to the conception of Order or Uniformity in the

world, physical or moral. The difficulties which thence arise

are mainly theological, metaphysical or psychological. What

we must now consider are problems of a more detailed or

logical character. They are prominently these two; (1) the

distinction between chance arrangement and \_causal\_ arrangement

in physical phenomena; and (2) the distinction between

chance arrangement and \_designed\_ arrangement where

we are supposed to be contemplating rational agency as

acting on one side at least.

II. The first of these questions raises the antithesis

between chance and causation, not as a general characteristic

pervading all phenomena, but in reference to some specified

occurrence:--Is this a case of chance or not? The most

strenuous supporters of the universal prevalence of causation

and order admit that the question is a relevant one, and

they must therefore be supposed to have some rule for

testing the answers to it.

Suppose, for instance, a man is seized with a fit in a

house where he has gone to dine, and dies there; and some

one remarks that that was the very house in which he was

born. We begin to wonder if this was an odd coincidence

and nothing more. But if our informant goes on to tell us

that the house was an old family one, and was occupied by

the brother of the deceased, we should feel at once that

these facts put the matter in a rather different light. Or

again, as Cournot suggests, if we hear that two brothers

have been killed in battle on the same day, it makes a great

difference in our estimation of the case whether they were

killed fighting in the same engagement or whether one fell

in the north of France and the other in the south. The

latter we should at once class with mere coincidences, whereas

the former might admit of explanation.

10. The problem, as thus conceived, seems to be one

rather of Inductive Logic than of Probability, because there

is not the slightest attempt to calculate chances. But it

deserves some notice here. Of course no accurate thinker

who was under the sway of modern physical notions would

for a moment doubt that each of the two elements in question

had its own 'cause' behind it, from which (assuming perfect

knowledge) it might have been confidently inferred. No

more would he doubt, I apprehend, that if we could take a

sufficiently minute and comprehensive view, and penetrate

sufficiently far back into the past, we should reach a stage at

which (again assuming perfect knowledge) the co-existence of

the two events could equally have been foreseen. The

employment of the word \_casual\_ therefore does not imply any

rejection of a cause; but it does nevertheless correspond to a

distinction of some practical importance. We call a coincidence

casual, I apprehend, when we mean to imply that no

knowledge of one of the two elements, which we can suppose

to be practically attainable, would enable us to expect the

other. We know of no generalization which covers them

both, except of course such as are taken for granted to be

inoperative. In such an application it seems that the word

'casual' is not used in antithesis to 'causal' or to 'designed',

but rather to that broader conception of order or regularity

to which I should apply the term Uniformity. The casual

coincidence is one which we cannot bring under any special

generalization; certain, probable, or even plausible.

A slightly different way of expressing this distinction is

to regard these 'mere coincidences' as being simply cases in

point of \_independent\_ events, in the sense in which independence

was described in a former chapter. We saw

that any two events, A and B, were so described when each

happens with precisely the same relative statistical frequency

whether the other happens or not. This state of things

seems to hold good of the successions of heads and tails in

tossing coins, as in that of male and female births in a

town, or that of the digits in many mathematical tables.

Thus we suppose that when men are picked up in the street

and taken into a house to die, there will not be in the long

run any preferential selection for or against the house in

which they were born. And all that we necessarily mean to

claim when we deny of such an occurrence, in any particular

case, that it is a mere coincidence, is that that particular

case must be taken out of the common list and transferred

to one in which there \_is\_ some such preferential selection.

11. III. The next problem is a somewhat more intricate

one, and will therefore require rather careful subdivision.

It involves the antithesis between Chance and

Design. That is, we are not now (as in the preceding case)

considering objects in their physical aspect alone, and taking

account only of the relative frequency of their co-existence or

sequence; but we are considering the agency by which they

are produced, and we are enquiring whether that agency

trusted to what we call chance, or whether it employed what

we call design.

The reader must clearly understand that we are not now

discussing the mere question of fact whether a certain

assigned arrangement \_is\_ what we call a chance one. This,

as was fully pointed out in the fourth chapter, can be settled

by mere inspection, provided the materials are extensive

enough. What we are now proposing to do is to carry on

the enquiry from the point at which we then had to leave it

off, by solving the question, Given a certain arrangement, is

it more likely that this was \_produced\_ by design, or by some

of the methods commonly called chance methods? The distinction

will be obvious if we revert to the succession of

figures which constitute the ratio π. As I have said, this

arrangement, regarded as a mere succession of digits, appears

to fulfil perfectly the characteristics of a chance arrangement.

If we were to omit the first four or five digits,

which are familiar to most of us, we might safely defy any

one to whom it was shown to say that it was not got at by

simply drawing figures from a bag. He might look at it for

his whole life without detecting that it was anything but the

result of such a chance selection. And rightly so, because

regarded as a mere arrangement it \_is\_ a chance one: it fulfils

all the requirements of such an arrangement.[2] The question

we are now proceeding to discuss is this: Given any such

arrangement how are we to determine the process by which

it was arrived at?

We are supposed to have some event before us which

might have been produced in either of two alternative

ways, i.e. by chance or by some kind of deliberate design;

and we are asked to determine the odds in favour of one

or other of these alternatives. It is therefore a problem in

Inverse Probability and is liable to all the difficulties to

which problems of this class are apt to be exposed.

12. For the theoretic solution of such a question we

require the two following data:--

(1) The relative frequency of the two classes of agencies,

viz. that which is to act in a chance way and that which

is to act designedly.

(2) The probability that each of these agencies, if it

were the really operative one, would produce the event in

question.

The latter of these data can generally be secured without

any difficulty. The determination of the various contingencies

on the chance hypothesis ought not, if the example

were a suitable one, to offer any other than arithmetical

difficulties. And as regards the design alternative, it is

generally taken for granted that if this had been operative

it would certainly have produced the result aimed at. For

instance, if ten pence are found on a table, all with head

uppermost, and it be asked whether chance or design had

been at work here; we feel no difficulty up to a certain

point. Had the pence been tossed we should have got ten

heads only once in 1024 throws; but had they been placed

designedly the result would have been achieved with certainty.

But the other postulate, viz. that of the relative prevalence

of these two classes of agencies, opens up a far more

serious class of difficulties. Cases can be found no doubt,

though they are not very frequent, in which this question

can be answered approximately, and then there is no further

trouble. For instance, if in a school class-list I were to see

the four names Brown, Jones, Robinson, Smith, standing

in this order, it might occur to me to enquire whether this

arrangement were alphabetical or one of merit. In our

enlarged sense of the terms this is equivalent to chance

and design as the alternatives; for, since the initial letter of

a boy's name has no known connection with his attainments,

the successive arrangement of these letters on any other

than the alphabetical plan will display the random features,

just as we found to be the case with the digits of an incommensurable

magnitude. The odds are 23 to 1 against 4 names

coming undesignedly in alphabetical order; they are

equivalent to certainty in favour of their doing so if this

order had been designed. As regards the relative frequency

of the two kinds of orders in school examinations I do not

know that statistics are at hand, though they could easily

be procured if necessary, but it is pretty certain that the

majority adopt the order of merit. Put for hypothesis the

proportion as high as 9 to 1, and it would still be found more

likely than not that in the case in question the order was

really an alphabetical one.

13. But in the vast majority of cases we have no

such statistics at hand, and then we find ourselves exposed

to very serious ambiguities. These may be divided into

two distinct classes, the nature of which will best be seen

by the discussion of examples.

In the first place we are especially liable to the drawback

already described in a former chapter as rendering

mere statistics so untrustworthy, which consists in the fact

that the proportions are so apt to be disturbed almost from

moment to moment by the possession of fresh hints or information.

We saw for instance why it was that statistics of

mortality were so very unserviceable in the midst of a

disease or in the crisis of a battle. Suppose now that on

coming into a room I see on the table ten coins lying face

uppermost, and am asked what was the likelihood that the

arrangement was brought about by design. Everything

turns upon special knowledge of the circumstances of the

case. Who had been in the room? Were they children,

or coin-collectors, or persons who might have been supposed

to have indulged in tossing for sport or for gambling purposes?

Were the coins new or old ones? a distinction of

this kind would be very pertinent when we were considering

the existence of any motive for arranging them the same

way uppermost. And so on; we feel that our statistics are

at the mercy of any momentary fragment of information.

14. But there is another consideration besides this.

Not only should we be thus influenced by what may be

called external circumstances of a general kind, such as the

character and position of the agents, we should also be influenced

by what we supposed to be the conventional[3] estimate

with which this or that particular chance arrangement was

then regarded. Thus from time to time as new games of

cards become popular new combinations acquire significance;

and therefore when the question of design takes the form of

possible cheating a knowledge of the current estimate of

such combinations becomes exceedingly important.

15. The full significance of these difficulties will best

be apprehended by the discussion of a case which is not

fictitious or invented for the purpose, but which has actually

given rise to serious dispute. Some years ago Prof. Piazzi

Smyth published a work[4] upon the great pyramid of Ghizeh,

the general object of which was to show that that building

contained, in its magnitude, proportions and contents, a

number of almost imperishable natural standards of length,

volume, &c. Amongst other things it was determined that

the value of π was accurately (the degree of accuracy is not,

I think, assigned) indicated by the ratio of the sides to the

height. The contention was that this result could not be

accidental but must have been designed.

As regards the estimation of the value of the chance

hypothesis the calculation is not quite so clear as in the

case of dice or cards. We cannot indeed suppose that,

for a given length of base, \_any\_ height can be equally possible.

We must limit ourselves to a certain range here; for if too

high the building would be insecure, and if too low it would

be ridiculous. Again, we must decide to how close an

approximation the measurements are made. If they are

guaranteed to the hundredth of an inch the coincidence

would be of a quite different order from one where the

guarantee extended only to an inch. Suppose that this

has been decided, and that we have ascertained that out

of 10,000 possible heights for a pyramid of given base just

that one has been selected which would most nearly yield

the ratio of the radius to the circumference of a circle.

The remaining consideration would be the relative frequency

of the 'design' alternative,--what is called its \_à priori\_

probability,--that is, the relative frequency with

which such builders can be supposed to have aimed at that

ratio; with the obvious implied assumption that if they did

aim at it they would certainly secure it. Considering our

extreme ignorance of the attainments of the builders it is

obvious that no attempt at numerical appreciation is here

possible. If indeed the 'design' was interpreted to mean

conscious resolve to produce that ratio, instead of mere resolve

to employ some method which happened to produce

it, few persons would feel much hesitation. Not only do

we feel tolerably certain that the builders did not know the

value of π, except in the rude way in which all artificers

must know it; but we can see no rational motive, if they

did know it, which should induce them to perpetuate it

in their building. If, however, to adopt an ingenious suggestion,[5]

we suppose that the builder may have proceeded

in the following fashion, the matter assumes a different

aspect. Suppose that having decided on the height of his

pyramid he drew a circle with that as radius: that, laying

down a cord along the line of this circle, he drew this cord

out into a square, which square marked the base of the

building. Hardly any simpler means could be devised in

a comparatively rude age; and it is obvious that the circumference

of the base, being equal to the length of the

cord, would bear exactly the admitted ratio to the height.

In other words, the exact attainment of a geometric value

does not imply a knowledge of that ratio, but merely of some

method which involves and displays it. A teredo can bore,

as well as any of us, a hole which displays the geometric properties

of a circle, but we do not credit it with corresponding

knowledge.

As before said, all numerical appreciation of the likelihood

of the design alternative is out of the question. But,

\_if\_ the precision is equal to what Mr Smyth claimed, I suppose

that most persons (with the above suggestion before

them) will think it somewhat more likely that the coincidence

was not a chance one.

16. There still remains a serious, and highly interesting

speculative consideration. In the above argument we took

it for granted, in calculating the chance alternative, that

only \_one\_ of the 10,000 possible values was favourable; that

is, we took it for granted that the ratio π was the only one

whose claims, so to say, were before the court. But it is

clear that if we had obtained just double this ratio the result

would have been of similar significance, for it would have

been simply the ratio of the circumference to the diameter.

In fact, Mr Smyth's selected ratio,--the height to twice the

breadth of the base as compared with the diameter to the

circumference,--is obviously only one of a plurality of ratios.

Again; if the measured results had shown that the ratio of

the height to one side of the base was 1 : sqrt{2} (i.e. that of a

side to a diagonal of a square) or 1 : sqrt{3} (i.e. that of a side

to a diagonal of a cube) would not such results equally show

evidence of design? Proceeding in this way, we might

suggest one known mathematical ratio after another until

most of the 10,000 supposed possible values had been taken

into account. We might then argue thus: since almost

every possible height of the pyramid would correspond to

\_some\_ mathematical ratio, a builder, ignorant of them all

alike, would be not at all unlikely to stumble upon one or

other of them: why then attribute design to him in one case

rather than another?

17. The answer to this objection has been already

hinted at. Everything turns upon the \_conventional\_ estimate

of one result as compared with another. Revert, for simplicity

to the coins. Ten heads is just as likely as alternate

heads and tails, or five heads followed by five tails; or, in

fact, as any one of the remaining 1023 possible cases. But

universal convention has picked out a run of ten as being

remarkable. Here, of course, the convention seems a very

natural and indeed inevitable one, but in other cases it is

wholly arbitrary. For instance, in cards, "queen of spades

and knave of diamonds" is exactly as uncommon as any

other such pair: moreover, till \_bezique\_ was introduced it

offered presumably no superior interest over any other

specified pair. But during the time when that game was

very popular this combination was brought into the category

of coincidences in which interest was felt; and, given dishonesty

amongst the players, its chance of being designed

stood at once on a much better footing.[6]

Returning then to the pyramid, we see that in balancing

the claims of chance and design we must, in fairness to the

latter, reckon to its account several other values as well as

that of π, e.g. sqrt{2} and sqrt{3}, and a few more such simple and

familiar ratios, as well as some of their multiples. But

though the number of such values which \_might\_ be reckoned,

on the ground that they are actually known to us, is infinite,

yet the number that \_ought\_ to be reckoned, on the ground

that they could have been familiar to the builders of a

pyramid, are very few. The order of probability for or against

the existence of design will not therefore be seriously altered

here by such considerations.[7]

18. Up to this point it will be observed that what we

have been balancing against each other are two forms of

agency,--of human agency, that is,--one acting through

chance, and the other by direct design. In this case we

know where we are, for we can thoroughly understand agency

of this kind. The problem is indeed but seldom numerically

soluble, and in most cases not soluble at all, but it is at any

rate capable of being clearly stated. We know the kind of

answer to be expected and the reasons which would serve to

determine it, if they were attainable.

The next stage in the enquiry would be that of balancing

ordinary human chance agency against,--I will not call it

direct spiritualist agency, for that would be narrowing the

hypothesis unnecessarily,--but against all other possible

causes. Some of the investigations of the Society for

Psychical Research will furnish an admirable illustration of

what is intended by this statement. There is a full discussion

of these applications in a recent essay by Mr F. Y.

Edgeworth;[8] but as his account of the matter is connected

with other calculations and diagrams I can only quote it in

part. But I am in substantial agreement with him.

"It is recorded that 1833 guesses were made by a 'percipient'

as to the suit of cards which the 'agent' had fixed

upon. The number of successful guesses was 510, considerably

above 458, the number which, as being the quarter of 1833,

would, on the supposition of pure chance, be more

likely than any other number. Now, by the Law of Error,

we are able approximately to determine the probability of

such an excess occurring by chance. It is equal to the extremity

of the tail of a probability-curve such as [those we

have already had occasion to examine].... The proportion

of this extremity of the tail to the whole body is 0.003 to 1.

That fraction, then, is the probability of a chance shot

striking that extremity of the tail; the probability that, if

the guessing were governed by pure chance, a number of

successful guesses equal or greater than 510 would occur":

odds, that is, of about 332 to 1 against such occurrence.

19. Mr Edgeworth holds, as strongly as I do, that for purposes

of calculation, in any strict sense of the word, we ought

to have some determination of the data on the non-chance side

of the hypothesis. We ought to know its relative frequency

of occurrence, and the relative frequency with which it

attains its aims. I am also in agreement with him that

"what that other cause may be,--whether some trick, or

unconscious illusion, or \_thought-transference\_ of the sort which

is vindicated by the investigators--it is for common-sense

and ordinary Logic to consider."

I am in agreement therefore with those who think that

though we cannot form a quantitative opinion we can in

certain cases form a tolerably decisive one. Of course if we

allow the last word to the supporters of the chance hypothesis

we can never reach proof, for it will always be open to

them to revise and re-fix the antecedent probability of the

counter hypothesis. What we may fairly require is that

those who deny the chance explanation should assign some

sort of minimum value to the probability of occurrence on

the other supposition, and we can then try to surmount this

by increasing the rarity of the actually produced phenomenon

on the chance hypothesis. If, for instance, they declare that

in their estimation the odds against any other than the chance

agency being at work are greater than 332 to 1, we must

try to secure a yet uncommoner occurrence than that in

question. If the supporters of thought-transference have

the courage of their convictions,--as they most assuredly

have,--they would not shrink from accepting this test. I

am inclined to think that even at present, on such evidence

as that above, the probability that the results were got at by

ordinary guessing is very small.

20. The problems discussed in the preceding sections

are at least intelligible even if they are not always resolvable.

But before finishing this chapter we must take notice

of some speculations upon this part of the subject which do not

seem to keep quite within the limits of what is intelligible.

Take for instance the question discussed by Arbuthnott (in

a paper in the \_Phil. Transactions\_, Vol. XXVII.) under the

title "An Argument for Divine Providence, taken from the

constant Regularity observed in the birth of both sexes." Had

his argument been of the ordinary teleological kind; that

is, had he simply maintained that the existent ratio of approximate

equality, with a six per cent. surplusage of males,

was a beneficent one, there would have been nothing here to

object against. But what he contemplated was just such a

balance of alternate hypotheses between chance and design

as we are here considering. His conclusion in his own words

is, "it is art, not chance, that governs."

It is difficult to render such an argument precise without

rendering it simply ridiculous. Strictly understood it can

surely bear only one of two interpretations. On the one

hand we may be personifying Chance: regarding it as an

agent which must be reckoned with as being quite capable

of having produced man, or at any rate having arranged the

proportion of the sexes. And then the decision must be

drawn, as between this agent and the Creator, which of the

two produced the existent arrangement. If so, and Chance

be defined as any agent which produces a chance or random

arrangement, I am afraid there can be little doubt that it

was this agent that was at work in the case in question.

The arrangement of male and female births presents, so far

as we can see, one of the most perfect examples of chance:

there is ultimate uniformity emerging out of individual

irregularity: all the 'runs' or successions of each alternative

are duly represented: the fact of, say, five sons having been

already born in a family does not seem to have any certain

effect in diminishing the likelihood of the next being a son,

and so on. Such a nearly perfect instance of 'independent

events' is comparatively very rare in physical phenomena.

It is all that we can claim from a chance arrangement.[9] The

only other interpretation I can see is to suggest that there

was but one agent who might, like any one of us, have either

tossed up or designed, and we have to ascertain which course

he probably adopted in the case in question. Here too, if

we are to judge of his mode of action by the tests we should

apply to any work of our own, it would certainly look very

much as if he had adopted some scheme of tossing.

21. The simple fact is that any rational attempt to

decide between chance and design as agencies must be confined

to the case of finite intelligences. One of the important

determining elements here, as we have seen, is the

state of knowledge of the agent, and the conventional estimate

entertained about this or that particular arrangement;

and these can be appreciated only when we are dealing with

beings like ourselves.

For instance, to return to that much debated question

about the arrangement of the stars, there can hardly be any

doubt that what Mitchell,--who started the discussion,--had

in view was the decision between Chance and Design. He

says (\_Trans. Roy. Soc.\_ 1767) "The argument I intend to

make use of... is of that kind which infers either design or

some general law from a general analogy and from the greatness

of the odds against things having been in the present

situation if it was not owing to some such cause." And he

concludes that had the stars "been scattered by mere chance

as it might happen" there would be "odds of near 500,000

to 1 that no six stars out of that number [1500], scattered at

random in the whole heavens, would be within so small a

distance from each other as the Pleiades are." Under any

such interpretation the controversy seems to me to be idle.

I do not for a moment dispute that there is some force in

the ordinary teleological argument which seeks to trace signs

of goodness and wisdom in the general tendency of things.

But what do we possibly understand about the nature of

creation, or the designs of the Creator, which should enable

us to decide about the likelihood of his putting the stars in

one shape rather than in another, or which should allow any

significance to "mere chance" as contrasted with his supposed

all-pervading agency?

22. Reduced to intelligible terms the two following

questions seem to me to emerge from the controversy:--

(I.) The stars being distributed through space, some of

them would of course be nearly in a straight line behind

others when looked at from our planet. Supposing that

they were tolerably uniformly distributed, we could calculate

about how many of them would thus be seen in apparent

close proximity to one another. The question is then put,

Are there more of them near to each other, two and two,

than such calculation would account for? The answer is that

there are many more. So far as I can see the only direct

inference that can be drawn from this is that they are \_not\_

uniformly distributed, but have a tendency to go in pairs.

This, however, is a perfectly sound and reasonable application

of the theory. Any further conclusions, such as that

these pairs of stars will form systems, as it were, to themselves,

revolving about one another, and for all practical purposes

unaffected by the rest of the sidereal system, are of

course derived from astronomical considerations.[10] Probability

confines itself to the simple answer that the distribution is

not uniform; it cannot pretend to say whether, and by what

physical process, these binary systems of stars have been

'caused'.[11]

23. (II.) The second question is this, Does the distribution

of the stars, after allowing for the case of the binary

stars just mentioned, resemble that which would be produced

by human agency sprinkling things 'at random'?

(We are speaking, of course, of their distribution as it appears

to us, on the visible heavens, for this is nearly all that

we can observe; but if they extend beyond the telescopic

range in every direction, this would lead to practically much

the same discussion as if we considered their actual arrangement

in space.) We have fully discussed, in a former chapter,

the meaning of 'randomness.' Applying it to the case

before us, the question becomes this, Is the distribution

tolerably uniform on the whole, but with innumerable individual

deflections? That is, when we compare large areas,

are the ratios of the number of stars in each equal area

approximately equal, whilst, as we compare smaller and

smaller areas, do the relative numbers become more and

more irregular? With certain exceptions, such as that of

the Milky Way and other nebular clusters, this seems to be

pretty much the case, at any rate as regards the bulk of the

stars.[12]

All further questions: the decision, for instance, for or

against any form of the Nebular Hypothesis: or, admitting

this, the decision whether such and such parts of the visible

heavens have sprung from the same nebula, must be left to

Astronomy to adjudicate.

NOTE ON THE PROPORTIONS OF THE SEXES.

The following remarks were rather too long for convenient insertion on

p. 259, and are therefore appended here.

The 'random' character of male and female births has generally been

rested almost entirely on statistics of place and time. But what is

more wanted, surely, is the proportion displayed when we compare a

number of \_families\_. This seems so obvious that I cannot but suppose

that the investigation must have been already made somewhere, though I

have not found any trace of it in the most likely quarters. Thus

Prof. Lexis (\_Massenerscheinungen\_) when supporting his view that the

proportion between the sexes at birth is almost the only instance

known to him, in natural phenomena, of true normal dispersion about a

mean, rests his conclusions on the ordinary statistics of the

registers of different countries.

It certainly needs proof that the same characteristics will hold good

when the family is taken as the unit, especially as some theories (e.g. that

of Sadler) would imply that 'runs' of boys or girls would be proportionally

commoner than pure chance would assign. Lexis has shown that this is

most markedly the case with \_twins\_: i.e., to use an obviously intelligible

notation, (M for male, F for female), that M.M. and F.F. are very much

commoner in proportion than M.F.

I have collected statistics including over 13,000 male and female

births, arranged in families of four and upwards. They were taken from

the pedigrees in the Herald's Visitations, and therefore represent as

a rule a somewhat select class, viz. the families of the eldest sons

of English country gentlemen in the sixteenth century. They are not

sufficiently extensive yet for publication, but I give a summary of

the results to indicate their tendency so far. The upper line of

figures in each case gives the \_observed\_ results: i.e. in the case

of a family of four, the numbers which had four male, three male and

one female, two male and two female, and so on. The lower line gives

the \_calculated\_ results; i.e. the corresponding numbers which would

have been obtained had batches of M.s and F.s been drawn from a bag in

which they were mixed in the ratio assigned by the total observed

numbers for those families.

512 families of 4; | m^4 m^3 f m^2 f^2 mf^3 f^4

yielding | 81 + 148 + 161 + 98 + 24 (observed.)

1188 M. : 860 F. | 57 + 168 + 184 + 88 + 15 (calculated.)

512 families of 5; | m^5 m^4 f m^3 f^2 m^2 f^3 mf^4 f^5

yielding | 50 + 82 + 161 + 143 + 61 + 15 (obs.)

1402 M. : 1158 F. | 25 + 103 + 172 + 143 + 59 + 10 (calc.)

512 families of 6; | m^6 m^5 f m^4 f^2 m^3 f^3 m^2 f^4 mf^5 f^6

yielding | 30 + 48 + 115 + 146 + 126 + 40 + 7 (obs.)

1612 M. : 1460 F. | 10 + 56 + 133 + 159 + 108 + 41 + 5 (calc.)

The numbers for the larger families are as yet too small to be worth

giving, but they show the same tendency. It will be seen that in every case

the observed central values are less than the calculated; and that the

observed extreme values are much greater than the calculated. The results

seem to suggest (so far) that a family cannot be likened to a chance drawing

of the requisite number from \_one\_ bag. A better analogy would be to suppose

two bags, one with M.s in excess and the other with F.s in less excess, and

that some persons draw from one and some from the other. But fuller

statistics are needed.

It will be observed that the total excess of male births is large. This

\_may\_ arise from undue omission of females; but I have carefully confined

myself to the two or three last generations, in each pedigree, for greater

security.

1. \_Essay on Probabilities\_, p. 114.

2. Doubts have been expressed about the truly random character of the

digits in this case (v. De Morgan, \_Budget of Paradoxes\_, p. 291),

and Jevons has gone so far as to ask (\_Principles of Science\_,

p. 529), "Why should the value of π, when expressed to a great

number of figures, contain the digit 7 much less frequently than any

other digit!" I do not quite understand what this means. If such a

question were asked in relation to any unusual divergence from the

\_à priori\_ chance in a case of throwing dice, say, we should

probably substitute for it the following, as being more appropriate

to our science:--Assign the degree of improbability of the event in

question; i.e. its statistical rarity. And we should then proceed

to judge, in the way indicated in the text, whether this

improbability gave rise to any grounds of suspicion.

The calculation is simple. The actual number of 7's, in the 708

digits, is 53: whilst the fair average would be 71. The question is,

What is the chance of such a departure from the average in 708

turns? By the usual methods of calculation (v. Galloway on

\_Probability\_) the chances against an excess or defect of 18 are

about 44 : 1, in respect of any specified digit. But of course what

we want to decide are the chances against \_some one of the ten\_

showing this divergence. This I estimate as being approximately

determined by the fraction (44/45)^{10}, viz. 0.8. This represents

odds of only about 4 : 1 against such an occurrence, which is

nothing remarkable. As a matter of fact several digits in the two

other magnitudes which Mr Shanks had calculated to the same length,

viz. Tan^{-1} 1/5 and Tan^{-1} 1/239, show the same divergencies

(v. \_Proc. Roy. Soc.\_ xxi. 319).

I may call attention here to a point which should have been noticed

in the chapter on Randomness. We must be cautious when we decide

upon the random character by mere inspection. It is very instructive

here to compare the digits in π with those within the 'period' of a

circulating decimal of very long period. That of 1 ÷ 7699, which

yields the full period of 7698 figures, was calculated some years

ago by two Cambridge graduates (Mr Lunn and Mr Suffield), and

privately printed. If we confine our examination to a portion of the

succession the random character seems plausible; i.e. the digits,

and their various combinations, come out in nearly, but not exactly,

equal numbers. So if we take batches of 10; the averages hover

nicely about 45. But if we took the whole period which

'circulates,' we should find these characteristics overdone, and the

random character would disappear. That is, instead of a merely

ultimate approximation to equality we should have (as far as this is

possible) an absolute attainment of it.

3. Of course this conventional estimate is nothing different in kind

from that which may attach to \_any\_ order or succession. Ten heads

in succession is intrinsically or objectively indistinguishable in

character from alternate heads and tails, or seven heads and three

tails, &c. Its distinction only consists in its almost universal

acceptance as remarkable.

4. \_Our Inheritance in the Great Pyramid\_, Ed. III. 1877.

5. Made in \_Nature\_ (Jan. 24, 1878) by Mr J. G. Jackson. It must be

remarked that Mr Smyth's alternative statement of his case leads up

to that explanation:--"The vertical height of the great pyramid is

the radius of a theoretical circle the length of whose curved

circumference is exactly equal to the sum of the lengths of the four

straight sides of the actual and practical square base." As regards

the alternatives of chance and design, here, it must be remembered

in justice to Mr Smyth's argument that the antithesis he admits to

chance is not human, but divine design.

6. See Cournot, \_Essai sur les fondements de nos connaissances\_.

Vol. I. p. 71.

7. It deserves notice that considerations of this kind have found

their way into the Law Courts though of course without any attempt

at numerical valuation. Thus, in the celebrated De Ros trial, in so

far as the evidence was indirect, one main ground of suspicion seems

to have been that Lord De Ros, when dealing at whist, obtained far

more court cards than chance could be expected to assign him; and

that in consequence his average gains for several years in

succession were unusually large. The counsel for the defence urged

that still larger gains had been secured by other players without

suspicion of unfairness,--(I cannot find that it was explained over

how large an area of experience these instances had been sought; nor

how far the magnitude of the stakes, as distinguished from the

number of successes, accounted for that of the actual gains),--and

that large allowance must be made for skill where the actual gains

were computed. (See the \_Times\_' report, Feb. 11, 1837.)

8. \_Metretike\_. At the end of this volume will be found a useful list

of a number of other publications by the same author on allied

topics.

9. That is, if we look simply to statistical results, as Arbuthnott

did, and as we should do if we were examining the tosses of a

penny. If the remarkable theory of Dr Düsing (\_Die Regulierung des

Geschlechts-verhältnisses...\_ Jena, 1884) be confirmed, the matter

would assume a somewhat different aspect. He attempts to show, both

on physiological grounds, and by analysis of statistics referring to

men and animals, that there is a decidedly \_compensatory\_ process at

work. That is, if for any cause either sex attains a preponderance,

agencies are at once set in motion which tend to redress the

balance. This is a modification and improvement of the older theory,

that the relative age of the parents has something to do with the

sex of the offspring.

Quetelet (\_Letters\_, p. 61) has attempted to \_prove\_ a proposition

about the succession of male and female births by certain

experiments supposed to be tried upon an urn with black and white

balls in it. But this is going too far. (See the note at the end of

this chapter.)

10. It is precisely analogous to the conclusion that the \_flowers\_ of

the daisies (as distinguished from the \_plants\_, v. p. 109) are not

distributed at random, but have a tendency to go in groups of two or

more. Mere observation shows this: and then, from our knowledge of

the growth of plants we may infer that these little groups spring

from the same root.

11. In this discussion, writers often speak of the probability of a

"\_physical connection\_" between these double stars. The phrase seems

misleading, for on the usual hypothesis of universal gravitation

\_all\_ stars are physically connected, by gravitation. It is

therefore better, as above, to make it simply a question of relative

proximity, and to leave it to astronomy to infer what follows from

unusual proximity.

12. Professor Forbes in the paper in the Philosophical Magazine

already referred to (Ch. VII. §18) gave several diagrams to show

what were the actual arrangements of a random distribution. He

scattered peas over a chess-board, and then counted the number which

rested on each square. His figures seem to show that the general

appearance of the stars is much the same as that produced by such a

plan of scattering.

Some recent investigations by Mr R. A. Proctor seem to show,

however, that there are at least two exceptions to this tolerably

uniform distribution. (1) He has ascertained that the stars are

decidedly more thickly aggregated in the Milky Way than

elsewhere. So far as this is to be relied on the argument is the

same as in the case of the double stars; it tends to prove that the

proximity of the stars in the Milky Way is not merely apparent, but

actual. (2) He has ascertained that there are two large areas, in

the North and South hemispheres, in which the stars are much more

thickly aggregated than elsewhere. Here, it seems to me,

Probability proves nothing: we are simply denying that the

distribution is uniform. What may follow in the way of inferences

as to the physical process of causation by which the stars have been

disposed is a question for the Astronomer. See Mr Proctor's \_Essays

on Astronomy\_, p. 297. Also a series of Essays in \_The Universe and

the coming Transits\_.

CHAPTER XI.

\_ON CERTAIN CONSEQUENCES OF THE OBJECTIVE TREATMENT OF A SCIENCE OF

INFERENCE.\_[1]

1. In the previous edition a large part of this chapter was devoted to

the general consideration of the distinction between a Material and

a Conceptualist view of Logic. I have omitted most of this here, as

also a large part of a chapter devoted to the detailed discussion of

the Law of Causation, as I hope before very long to express my

opinions on these subjects more fully, and more appropriately, in a

treatise on the general principles of Inductive Logic.

1. Students of Logic are familiar with that broad

distinction between the two methods of treatment to which

the names of Material and Conceptualist may be applied.

The distinction was one which had been gradually growing

up under other names before it was emphasized, and treated

as a distinction within the field of Logic proper, by the publication

of Mill's well known work. No one, for instance,

can read Whewell's treatises on Induction, or Herschel's

Discourse, without seeing that they are treating of much the

same subject-matter, and regarding it in much the same

way, as that which Mill discussed under the name of Logic,

though they were not disposed to give it that name. That

is, these writers throughout took it for granted that what

they had to do was to systematise the facts of nature in

their objective form, and under their widest possible treatment,

and to expound the principal modes of inference and

the principal practical aids in the investigation of these

modes of inference, which reason could suggest and which

experience could justify. What Mill did was to bring these

methods into close relation with such portions of the old

scholastic Logic as he felt able to retain, to work them out

into much fuller detail, to systematize them by giving them

a certain philosophical and psychological foundation,--and

to entitle the result \_Logic\_.

The practical treatment of a science will seldom correspond

closely to the ideal which its supporters propose to

themselves, and still seldomer to that which its antagonists

insist upon demanding from the supporters. If we were to

take our account of the distinction between the two views of

Logic expounded respectively by Hamilton and by Mill,

from Mill and Hamilton respectively, we should certainly

not find it easy to bring them under one common definition.

By such a test, the material Logic would be regarded as

nothing more than a somewhat arbitrary selection from the

domain of Physical Science in general, and the conceptualist

Logic nothing more than a somewhat arbitrary selection from

the domain of Psychology. The former would omit all consideration

of the laws of thought and the latter all consideration

of the truth or falsehood of our conclusions.

Of course, in practice, such extremes as these are soon

seen to be avoidable, and in spite of all controversial exaggerations

the expounders of the opposite views do contrive to

retain a large area of speculation in common. I do not propose

here to examine in detail the restrictions by which this

accommodation is brought about, or the very real and important

distinctions of method, aim, tests, and limits which

in spite of all approach to agreement are still found to subsist.

To attempt this would be to open up rather too wide an

enquiry to be suitable in a treatise on one subdivision only

of the general science of Inference.

2. One subdivision of this enquiry is however really

forced upon our notice. It does become important to consider

the restrictions to which the ultra-material account of

the province of Logic has to be subjected, because we shall

thus have our attention drawn to an aspect of the subject

which, slight and fleeting as it is within the region of Induction

becomes very prominent and comparatively permanent

in that of Probability. According to this ultra-material view,

Inductive Logic would generally be considered to have nothing

to do with anything but objective facts: its duty is to

start from facts and to confine itself to such methods as will

yield nothing but facts. What is doubtful it either establishes

or it lets alone for the present, what is unattainable

it rejects, and in this way it proceeds to build up by slow

accretion a vast fabric of certain knowledge.

But of course all this is supposed to be done by human

minds, and therefore if we enquire whether notions or concepts,--call

them what we will,--have no place in such a

scheme it must necessarily be admitted that they \_have\_ some

place. The facts which form our starting point must be

grasped by an intelligent being before inference can be built

upon them; and the 'facts' which form the conclusion have

often, at any rate for some time, no place anywhere else than

in the mind of man. But no one can read Mill's treatise, for

instance, without noticing how slight is his reference to this

aspect of the question. He remarks, in almost contemptuous

indifference, that the man who digs must of course have a

notion of the ground he digs and of the spade he puts into

it, but he evidently considers that these 'notions' need not

much more occupy the attention of the speculative logician,

in so far as his mere inferences are concerned, than they

occupy that of the husbandman.

3. It must be admitted that there is some warrant

for this omission of all reference to the subjective side of

inference so long as we are dealing with Inductive Logic.

The inductive \_discoverer\_ is of course in a very different

position. If he is worthy of the name his mind at every

moment will be teeming with notions which he would be as

far as any one from calling facts: he is busy making them

such to the best of his power. But the logician who follows

in his steps, and whose business it is to explain and justify

what his leader has discovered, is rather apt to overlook this

mental or uncertain stage. What he mostly deals in are the

'complete inductions' and 'well-grounded generalizations'

and so forth, or the exploded errors which contradict them:

the prisoners and the corpses respectively, which the real

discoverer leaves on the field behind him whilst he presses

on to complete his victory. The whole method of science,--expository

as contrasted with militant,--is to emphasize the

distinction between fact and non-fact, and to treat of little

else but these two. In other words a treatise on Inductive

Logic \_can\_ be written without any occasion being found to

define what is meant by a notion or concept, or even to employ

such terms.

4. And yet, when we come to look more closely, signs

may be detected even within the field of Inductive Logic,

of an occasional breaking down of the sharp distinction in

question; we may meet now and then with entities (to use

the widest term attainable) in reference to which it would

be hard to say that they are either facts or conceptions.

For instance, Inductive Logic has often occasion to make use

of Hypotheses: to which of the above two classes are these

to be referred? They do not seem in strictness to belong to

either; nor are they, as will presently be pointed out, by any

means a solitary instance of the kind.

It is true that within the province of Inductive Logic

these hypotheses do not give much trouble on this score.

However vague may be the form in which they first present

themselves to the philosopher's mind, they have not much

business to come before us in our capacity of logicians until

they are well on their way, so to say, towards becoming

facts: until they are beginning to harden into that firm

tangible shape in which they will eventually appear. We

generally have some such recommendations given to us as

that our hypotheses shall be well-grounded and reasonable.

This seems only another way of telling us that however

freely the philosopher may make his guesses in the privacy

of his own study, he had better not bring them out into

public until they can with fair propriety be termed facts,

even though the name be given with some qualification, as

by terming them 'probable facts.' The reason, therefore,

why we do not take much account of this intermediate state

in the hypothesis, when we are dealing with the inductive

processes, is that here at any rate it plays only a temporary

part; its appearance in that guise is but very fugitive. If

the hypothesis be a sound one, it will soon take its place as

an admitted fact; if not, it will soon be rejected altogether.

Its state as a hypothesis is not a normal one, and therefore

we have not much occasion to scrutinize its characteristics.

In so saying, it must of course be understood that we are

speaking as inductive logicians; the philosopher in his workshop

ought, as already remarked, to be familiar enough with

the hypothesis in every stage of its existence from its origin;

but the logician's duty is different, dealing as he does with

proof rather than with the processes of original investigation

and discovery.

We might indeed even go further, and say that in many

cases the hypothesis does not present itself to the reader,

that is to the recipient of the knowledge, until it has ceased

to deserve that name at all. It may be first suggested to

him along with the proof which establishes it, he not having

had occasion to think of it before. It thus comes at a single

step out of the obscurity of the unknown into the full possession

of its rights as a fact, skipping practically the intermediate

or hypothetical stage altogether. The original investigator

himself may have long pondered over it, and kept

it present to his mind, in this its dubious stage, but finally

have given it to the world with that amount of evidence

which raises it at once in the minds of others to the level of

commonly accepted facts.

Still this doubtful stage exists in every hypothesis,

though for logical purposes, and to most minds, it exists

in a very fugitive way only. When attention has been

directed to it, it may be also detected elsewhere in Logic.

Take the case, for instance, of the reference of names.

Mill gives the examples of the sun, and a battle, as distinguished

from the ideas of them which we, or children,

may entertain. Here the distinction is plain and obvious

enough. But if, on the other hand, we take the case of

things whose existence is doubtful or disputed, the difficulty

above mentioned begins to show itself. The case of merely

extinct things, or such as have not yet come into existence,

offers indeed no trouble, since of course actually \_present\_

existence is not necessary to constitute a fact. The usual

distinction may even be retained also in the case of mythical

existences. Centaur and Griffin have as universally recognised

a significance amongst the poets, painters, and heralds

as lion and leopard have. Hence we may claim, even here,

that our conceptions shall be 'truthful,' 'consistent with

fact,' and so on, by which we mean that they are to be in

accordance with universal convention upon such subjects.

Necessary and universal accordance is sometimes claimed

to be all that is meant by 'objective,' and since universal

accordance is attainable in the case of the notoriously fictitious,

our fundamental distinction between fact and conception,

and our determination that our terms shall refer to

what is objective rather than to what is subjective, may with

some degree of strain be still conceived to be tenable even here.

5. But when we come to the case of disputed phenomena

the difficulty re-emerges. A supposed planet or

new mineral, a doubtful fact in history, a disputed theological

doctrine, are but a few examples out of many that might be

offered. What some persons strenuously assert, others as

strenuously deny, and whatever hope there may be of speedy

agreement in the case of physical phenomena, experience

shows that there is not much prospect of this in the case of

those which are moral and historical, to say nothing of theological.

So long as those who are in agreement confine their

intercourse to themselves, their 'facts' are accepted as such,

but as soon as they come to communicate with others all

distinction between fact and conception is lost at once, the

'facts' of one party being mere groundless 'conceptions' to

their opponents. There is therefore, I think, in these cases

a real difficulty in carrying out distinctly and consistently

the account which the Materialist logician offers as to the

reference of names. It need hardly be pointed out that

what thus applies to names or terms applies equally to

propositions in which particular or general statements are

made involving names.

6. But when we step into Probability, and treat this

from the same material or Phenomenal point of view, we can

no longer neglect the question which is thus presented to us.

The difficulty cannot here be rejected, as referring to what is

merely temporary or occasional. The intermediate condition

between conjecture and fact, so far from being temporary

or occasional only, is here normal. It is just the condition

which is specially characteristic of Probability. Hence it

follows that however decidedly we may reject the Conceptualist

theory we cannot altogether reject the use of

Conceptualist language. If we can prove that a given man

will die next year, or attain sufficiently near to proof to

leave us practically certain on the point, we may speak of

his death as a (future) fact. But if we merely contemplate

his death as probable? This is the sort of inference, or

substitute for inference, with which Probability is specially

concerned. We may, if we so please, speak of 'probable

facts,' but if we examine the meaning of the words we

may find them not merely obscure, but self-contradictory.

Doubtless there are facts here, in the fullest sense of the

term, namely the statistics upon which our opinion is ultimately

based, for these are known and admitted by all who

have looked into the matter. The same language may also

be applied to that extension of these statistics by induction

which is involved in the assertion that similar statistics

will be found to prevail elsewhere, for these also may rightfully

claim universal acceptance. But these statements, as

was abundantly shown in the earlier chapters, stand on a

very different footing from a statement concerning the individual

event; the establishment and discussion of the

former belong by rights to Induction, and only the latter

to Probability.

7. It is true that for want of appropriate terms to

express such things we are often induced, indeed compelled,

to apply the same name of 'facts' to such individual contingencies.

We should not, for instance, hesitate to speak of

the fact of the man dying being probable, possible, unlikely,

or whatever it might be. But I cannot help regarding such

expressions as a strictly incorrect usage arising out of a

deficiency of appropriate technical terms. It is doubtless

certain that one or other of the two alternatives must happen,

but this alternative certainty is not the subject of our contemplation;

what we have before us is the \_single\_ alternative,

which is notoriously uncertain. It is this, and this only,

which is at present under notice, and whose occurrence has

to be estimated. We have surely no right to dignify this

with the name of a fact, under any qualifications, when the

opposite alternative has claims, not perhaps actually equal to,

but at any rate not much inferior to its own. Such language,

as already remarked, may be quite right in Inductive logic,

where we are only concerned with conjectures of such a high

degree of likelihood that their non-occurrence need not be

taken into practical account, and which are moreover regarded

as merely temporary. But in Probability the conjecture

may have any degree of likelihood about it; it may be just

as likely as the other alternative, nay it may be much less

likely. In these latter cases, for instance, if the chances are

very much against the man's death, it is surely an abuse of

language to speak of the 'fact' of his dying, even though we

qualify it by declaring it to be highly improbable. The

subject-matter essential to Probability being the uncertain,

we can never with propriety employ upon it language which

in its original and correct application is only appropriate to

what is actually or approximately certain.

8. It should be remembered also that this state of

things, thus characteristic of Probability, is \_permanent\_ there.

So long as they remain under the treatment of that science

our conjectures, or whatever we like to call them, never

develop into facts. I calculate, for instance, the chance that

a die will give ace, or that a man will live beyond a certain

age. Such an approximation to knowledge as is thus acquired

is as much as we can ever afterwards hope to get,

unless we resort to other methods of enquiry. We do not, as

in Induction, feel ourselves on the brink of some experimental

or other proof which at any moment may raise it

into certainty. It is nothing but a conjecture of a certain

degree of strength, and such it will ever remain, so long as

Probability is left to deal with it. If anything more is ever

to be made out of it we must appeal to direct experience, or

to some kind of inductive proof. As we have so often said,

individual facts can never be determined here, but merely

ultimate tendencies and averages of many events. I may,

indeed, by a second appeal to Probability improve the

character of my conjecture, through being able to refer it to

a narrower and better class of statistics; but its essential

nature remains throughout what it was.

It appears to me therefore that the account of the Materialist

view of logic indicated at the commencement of this

chapter, though substantially sound, needs some slight reconsideration

and re-statement. It answers admirably so far as

ordinary Induction is concerned, but needs some revision

if it is to be equally applicable to that wider view of

the nature and processes of acquiring knowledge wherein

the science of logic is considered to involve Probability

also as well as Induction.

9. Briefly then it is this. We regard the scientific

thinker, whether he be the original investigator who discovers,

or the logician who analyses and describes the proofs

that may be offered, as surrounded by a world of objective

phenomena extending indefinitely both ways in time, and in

every direction in space. Most of them are, and always will

remain, unknown. If we speak of them as facts we mean

that they are potential objects of human knowledge, that

under appropriate circumstances men could come to determinate

and final agreement about them. The scientific or

material logician has to superintend the process of converting

as much as possible of these unknown phenomena into what

are known, of aggregating them, as we have said above, about

the nucleus of certain data which experience and observation

had to start with. In so doing his principal resources are

the Methods of Induction, of which something has been

said in a former chapter; another resource is found in the

Theory of Probability, and another in Deduction.

Now, however such language may be objected to as

savouring of Conceptualism, I can see no better compendious

way of describing these processes than by saying that we are

engaged in getting at conceptions of these external phenomena,

and as far as possible converting these conceptions

into facts. What is the natural history of 'facts' if we trace

them back to their origin? They first come into being as

mere guesses or conjectures, as contemplated possibilities

whose correspondence with reality is either altogether disbelieved

or regarded as entirely doubtful. In this stage, of

course, their contrast with facts is sharp enough. \_How\_ they

arise it does not belong to Logic but to Psychology to say.

Logic indeed has little or nothing to do with them whilst

they are in this form. Everyone is busy all his life in entertaining

such guesses upon various subjects, the superiority of

the philosopher over the common man being mainly found in

the quality of his guesses, and in the skill and persistence

with which he sifts and examines them. In the next stage

they mostly go by the name of theories or hypotheses, when

they are comprehensive in their scope, or are in any way on

a scale of grandeur and importance: when however they are

of a trivial kind, or refer to details, we really have no distinctive

or appropriate name for them, and must be content

therefore to call them 'conceptions.' Through this stage

they flit with great rapidity in Inductive Logic; often the

logician keeps them back until their evidence is so strong

that they come before the world at once in the full dignity

of facts. Hence, as already remarked, this stage of their

career is not much dwelt upon in Logic. But the whole

business of Probability is to discuss and estimate them at

this point. Consequently, so far as this science is concerned,

the explanation of the Material logician as to the reference

of names and propositions has to be modified.

10. The best way therefore of describing our position

in Probability is as follows:--We are \_entertaining a conception\_

of some event, past, present, or future. From the nature

of the case this conception is all that can be actually entertained

by the mind. In its present condition it would be

incorrect to call it a fact, though we would willingly, if we

could, convert it into such by making certain of it one way

or the other. But so long as our conclusions are to be

effected by considerations of Probability only, we cannot do

this. The utmost we can do is to \_estimate\_ or \_evaluate\_ it.

The whole function of Probability is to give rules for so

doing. By means of reference to statistics or by direct

deduction, as the case may be, we are enabled to say how

much this conception is to be believed, that is in what proportion

out of the total number of cases we shall be right

in so doing. Our position, therefore, in these cases seems

distinctly that of entertaining a conception, and the process

of inference is that of ascertaining to what extent we are

justified in adding this conception to the already received

body of truth and fact.

So long, then, as we are confined to Probability these

conceptions remain such. But if we turn to Induction we

see that they are meant to go a step further. Their final

stage is not reached until they have ripened into facts, and

so taken their place amongst uncontested truths. This is

their final destination in Logic, and our task is not accomplished

until they have reached it.

11. Such language as this in which we speak of our

position in Probability as being that of entertaining a conception,

and being occupied in determining what degree of

belief is to be assigned to it, may savour of Conceptualism,

but is in spirit perfectly different from it. Our ultimate

reference is always to facts. We start from them as our data,

and reach them again eventually in our results whenever it

is possible. In Probability, of course, we cannot do this in

the individual result, but even then (as shown in Ch. VI.) we

always \_justify\_ our conclusions by appeal to facts, viz. to what

happens in the long run.

The discussion which has been thus given to this part of

the subject may seem somewhat tedious, but it was so obviously

forced upon us when considering the distinction

between the two main views of Logic, that it was impossible

to pass it over without fear of misapprehension and confusion.

Moreover, as will be seen in the course of the next

chapter, several important conclusions could not have been

properly explained and justified without first taking pains

to make this part of our ground perfectly plain and satisfactory.

CHAPTER XII.

\_CONSEQUENCES OF THE FOREGOING DISTINCTIONS.\_

1. We are now in a position to explain and justify some

important conclusions which, if not direct consequences of

the distinctions laid down in the last chapter, will at any

rate be more readily appreciated and accepted after that

exposition.

In the first place, it will be seen that in Probability \_time\_

has nothing to do with the question; in other words, it does

not matter whether the event, whose probability we are discussing,

be past, present, or future. The problem before us,

in its simplest form, is this:--Statistics (extended by Induction,

and practically often gained by Deduction) inform us

that a certain event has happened, does happen, or will

happen, in a certain way in a certain proportion of cases.

We form a conception of that event, and regard it as possible;

but we want to do more; we want to know \_how much\_ we

ought to expect it (under the explanations given in a former

chapter about quantity of belief). There is therefore a

sort of relative futurity about the event, inasmuch as our

knowledge of the fact, and therefore our justification or

otherwise of the correctness of our surmise, almost necessarily

comes after the surmise was formed; but the futurity

is only relative. The evidence by which the question is to

be settled may not be forthcoming yet, or we may have it by

us but only consult it afterwards. It is from the fact of the

futurity being, as above described, only relative, that I have

preferred to speak of the conception of the event rather than

of the anticipation of it. The latter term, which in some respects

would have seemed more intelligible and appropriate,

is open to the objection, that it does rather, in popular estimation,

convey the notion of an absolute as opposed to a

relative futurity.

2. For example; a die is thrown. Once in six times

it gives ace; if therefore we assume, without examination,

that the throw is ace, we shall be right once in six times.

In so doing we may, according to the usual plan, go \_forwards\_

in time; that is, form our opinion about the throw beforehand,

when no one can tell what it will be. Or we might go

\_backwards\_; that is, form an opinion about dice that had

been cast on some occasion in time past, and then correct

our opinion by the testimony of some one who had been a

witness of the throws. In either case the mental operation

is precisely the same; an opinion formed merely on statistical

grounds is afterwards corrected by specific evidence. The

opinion may have been formed upon a past, present, or future

event; the evidence which corrects it afterwards may be our

own eyesight, or the testimony of others, or any kind of inference;

by the evidence is merely meant such subsequent

examination of the case as is assumed to set the matter at

rest. It is quite possible, of course, that this specific evidence

should never be forthcoming; the conception in that

case remains as a conception, and never obtains that degree

of conviction which qualifies it to be regarded as a 'fact.'

This is clearly the case with all past throws of dice the results

of which do not happen to have been recorded.

In discussing games of chance there are obvious advantages

in confining ourselves to what is really, as well as

relatively, future, for in that case direct information concerning

the contemplated result being impossible, all persons are

on precisely the same footing of comparative ignorance, and

must form their opinion entirely from the known or inferred

frequency of occurrence of the event in question. On the

other hand, if the event be passed, there is almost always evidence

of some kind and of some value, however slight, to

inform us what the event really was; if this evidence is not

actually at hand, we can generally, by waiting a little, obtain

something that shall be at least of some use to us in forming

our opinion. Practically therefore we generally confine ourselves,

in anticipations of this kind, to what is really future,

and so in popular estimation futurity becomes indissolubly

associated with probability.

3. There is however an error closely connected with

the above view of the subject, or at least an inaccuracy of

expression which is constantly liable to lead to error, which

has found wide acceptance, and has been sanctioned by

writers of the greatest authority. For instance, both Butler,

in his \_Analogy\_, and Mill, have drawn attention, under one

form of expression or another, to the distinction between improbability

before the event and improbability after the

event, which they consider to be perfectly different things.

That this phraseology indicates a distinction of importance

cannot be denied, but it seems to me that the language in

which it is often expressed requires to be amended.

Butler's remarks on this subject occur in his \_Analogy\_, in

the chapter on miracles. Admitting that there is a strong

presumption against miracles (his equivalent for the ordinary

expression, an 'improbability before the event') he

strives to obtain assent for them by showing that other

events, which also have a strong presumption against them,

are received on what is in reality very slight evidence. He

says, "There is a very strong presumption against common

speculative truths, and against the most ordinary facts, before

the proof of them; which yet is overcome by almost any

proof. There is a presumption of millions to one against the

story of Cæsar, or of any other man. For, suppose a number

of common facts so and so circumstanced, of which one had

no kind of proof, should happen to come into one's thoughts,

every one would without any possible doubt conclude them

to be false. And the like may be said of a single common

fact."

4. These remarks have been a good deal criticized,

and they certainly seem to me misleading and obscure in

their reference. If one may judge by the context, and by

another passage in which the same argument is afterwards

referred to,[1] it would certainly appear that Butler drew no

distinction between miraculous accounts, and other accounts

which, to use any of the various expressions in common use,

are unlikely or improbable or have a presumption against

them; and concluded that since some of the latter were instantly

accepted upon somewhat mediocre testimony, it was

altogether irrational to reject the former when similarly or

better supported.[2] This subject will come again under our

notice, and demand fuller discussion, in the chapter on the

Credibility of extraordinary stories. It will suffice here to

remark that, however satisfactory such a view of the matter

might be to some theologians, no antagonist of miracles

would for a moment accept it. He would naturally object

that, instead of the miraculous element being (as Butler

considers) "a small additional presumption" against the

narrative, it involved the events in a totally distinct class of

incredibility; that it multiplied, rather than merely added

to, the difficulties and objections in the way of accepting

the account.

Mill's remarks (\_Logic\_, Bk. III. ch. XXV. §4) are of a different

character. Discussing the grounds of disbelief he

speaks of people making the mistake of "overlooking the

distinction between (what may be called) improbability

before the fact, and improbability after it, two different

properties, the latter of which is always a ground of disbelief,

the former not always." He instances the throwing

of a die. It is improbable beforehand that it should turn

up ace, and yet afterwards, "there is no reason for disbelieving

it if any credible witness asserts it." So again, "the

chances are greatly against A. B.'s dying, yet if any one tells

us that he died yesterday we believe it."

5. That there is some difficulty about such problems

as these must be admitted. The fact that so many people

find them a source of perplexity, and that such various

explanations are offered to solve the perplexity, are a sufficient

proof of this.[3] The considerations of the last chapter,

however, over-technical and even scholastic as some of the

language in which it was expressed may have seemed to the

reader, will I hope guide us to a more satisfactory way of

regarding the matter.

When we speak of an improbable event, it must be

remembered that, objectively considered, an event can only

be more or less \_rare\_; the extreme degree of rarity being of

course that in which the event does not occur at all. Now,

as was shown in the last chapter, our position, when forming

judgments of the time in question, is that of entertaining

a conception or conjecture (call it what we will), and assigning

a certain weight of trustworthiness to it. The real

distinction, therefore, between the two classes of examples

respectively, which are adduced both by Butler and by Mill,

consists in the way in which those conceptions are obtained;

they being obtained in one case by the process of guessing,

and in the other by that of giving heed to the reports of

witnesses.

6. Take Butler's instance first. In the 'presumption

before the proof' we have represented to us a man thinking

of the story of Cæsar, that is, making a guess about certain

historical events without any definite grounds for it, and

then speculating as to what value is to be attached to the

probability of its truth. Such a guess is of course, as he

says, concluded to be false. But what does he understand

by the 'presumption after the proof'? That a story not

adopted at random, but actually suggested and supported by

witnesses, should be true. The latter might be accepted,

whilst the former would undoubtedly be rejected; but all

that this proves, or rather illustrates, is that the testimony

of almost any witness is in most cases vastly better than

a mere guess.[4] We may in both cases alike speak of 'the

event' if we will; in fact, as was admitted in the last

chapter, common language will not readily lend itself to any

other way of speaking. But it should be clearly understood

that, phrase it how we will, what is really present to

the man's mind, and what is to have its probable value

assigned to it, is the conception of an event, in the sense in

which that expression has already been explained. And

surely no two conceptions can have a much more important

distinction put between them than that which is involved in

supposing one to rest on a mere guess, and the other on the

report of a witness. Precisely the same remarks apply to

the example given by Mill. Before A. B.'s death our

opinion upon the subject was nothing but a guess of our

own founded upon life statistics; after his death it was

founded upon the evidence of some one who presumably

had tolerable opportunities of knowing what the facts really

were.

7. That the distinction before us has no essential connection

whatever with time is indeed obvious on a moment's

consideration. Conceive for a moment that some one had

opportunities of knowing whether A. B. would die or not.

If he told us that A. B. would die to-morrow, we should in

that case be just as ready to believe him as when he tells us

that A. B. \_has\_ died. If we continued to feel any doubt

about the statement (supposing always that we had full

confidence about his veracity in matters into which he had

duly enquired), it would be because we thought that in his

case, as in ours, it was equivalent to a guess, and nothing

more. So with the event when past, the fact of its being

past makes no difference whatever; until the credible witness

informs us of what he knows to have occurred, we

should doubt it if it happened to come into our minds, just

as much as if it were future.

The distinction, therefore, between probability before the

event and probability after the event seems to resolve itself

simply into this;--before the event we often have no better

means of information than to appeal to statistics in some

form or other, and so to guess amongst the various possible

alternatives; after the event the guess may most commonly

be improved or superseded by appeal to specific evidence,

in the shape of testimony or observation. Hence, naturally,

our estimate in the latter case is commonly of much more

value. But if these characteristics were anyhow inverted;

if, that is, we were to confine ourselves to guessing about the

past, and if we could find any additional evidence about the

future, the respective values of the different estimates would

also be inverted. The difference between these values has

no necessary connection with time, but depends entirely

upon the different grounds upon which our conception or

conjecture about the event in question rests.

8. The following imaginary example will serve to

bring out the point indicated above. Conceive a people with

very short memories, and who preserved no kind of record to

perpetuate their hold upon the events which happened

amongst them.[5] The whole region of the past would then be

to them what much of the future is to us; viz. a region of

guesses and conjectures, one in reference to which they

could only judge upon general considerations of probability,

rather than by direct and specific evidence. But conceive

also that they had amongst them a race of prophets who

could succeed in foretelling the future with as near an

approach to accuracy and trustworthiness as our various

histories, and biographies, and recollections, can attain in

respect to the past. The present and usual functions of

direct evidence or testimony, and of probability, would then

be simply inverted; and so in consequence would the present

accidental characteristics of improbability before and

after the event. It would then be the latter which would

by comparison be regarded as 'not always a ground of disbelief,'

whereas in the case of the former we should then

have it maintained that it always was so.

9. The origin of the mistake just discussed is worth

enquiring into. I take it to be as follows. It is often the

case, as above remarked, when we are speculating about

a future event, and almost always the case when that future

event is taken from a game of chance, that all persons are in

precisely the same condition of ignorance in respect to it.

The limit of available information is confined to statistics,

and amounts to the knowledge that the unknown event

must assume some one of various alternative forms. The

conjecture, therefore, of any one man about it is as valuable

as that of any other. But in regard to the past the case is

very different. Here we are not in the habit of relying

upon statistical information. Hence the conjectures of different

men are of extremely different values; in the case of

many they amount to what we call positive knowledge.

This puts a broad distinction, in popular estimation, between

what may be called the objective certainty of the past and of

the future, a distinction, however, which from the standing-point

of a science of inference ought to have no existence.

In consequence of this, when we apply to the past and

the future respectively the somewhat ambiguous expression

'the chance of the event,' it commonly comes to bear very

different significations. Applied to the future it bears its

proper meaning, namely, the value to be assigned to a conjecture

upon statistical grounds. It does so, because in this

case hardly any one has more to judge by than such conjectures.

But applied to the past it shifts its meaning,

owing to the fact that whereas some men have conjectures

only, others have positive knowledge. By the chance of the

event is now often meant, not the value to be assigned to a

conjecture founded on statistics, but to such a conjecture

derived from and enforced by any body else's conjecture, that

is by his knowledge and his testimony.

10. There is a class of cases in apparent opposition to

some of the statements in this chapter, but which will be

found, when examined closely, decidedly to confirm them.

I am walking, say, in a remote part of the country, and suddenly

meet with a friend. At this I am naturally surprised.

Yet if the view be correct that we cannot properly speak

about events in themselves being probable or improbable,

but only say this of our conjectures about them, how do we

explain this? We had formed no conjecture beforehand,

for we were not thinking about anything of the kind, but

yet few would fail to feel surprise at such an incident.

The reply might fairly be made that we \_had\_ formed

such anticipations tacitly. On any such occasion every

one unconsciously divides things into those which are known

to him and those which are not. During a considerable

previous period a countless number of persons had met us,

and all fallen into the list of the unknown to us. There

was nothing to remind us of having formed the anticipation

or distinction at all, until it was suddenly called out

into vivid consciousness by the exceptional event. The

words which we should instinctively use in our surprise seem

to show this:--'Who would have thought of seeing you

here?' viz. Who would have given any weight to the latent

thought if it had been called out into consciousness beforehand?

We put our words into the past tense, showing that

we have had the distinction lurking in our minds all the

time. We always have a multitude of such ready-made

classes of events in our minds, and when a thing happens to

fall into one of those classes which are very small we cannot

help noticing the fact.

Or suppose I am one of a regiment into which a shot

flies, and it strikes me, and me only. At this I am surprised,

and why? Our common language will guide us to the

reason. 'How strange that it should just have hit \_me\_ of all

men!' We are thinking of the very natural two-fold division

of mankind into, ourselves, and everybody else; our surprise

is again, as it were, retrospective, and in reference to this

division. No anticipation was distinctly formed, because

we did not think beforehand of the event, but the event,

when it has happened, is at once assigned to its appropriate

class.

11. This view is confirmed by the following considerations.

Tell the story to a friend, and he will be a little

surprised, but less so than we were, \_his\_ division in this

particular case being,--his friends (of whom we are but one),

and the rest of mankind. It is not a necessary division, but

it is the one which will be most likely suggested to him.

Tell it again to a perfect stranger, and his division being

different (viz. we falling into the majority) we shall fail to

make him perceive that there is anything at all remarkable

in the event.

It is not of course attempted in these remarks to justify

our surprise in every case in which it exists. Different

persons might be differently affected in the cases supposed,

and the examples are therefore given mainly for illustration.

Still on principles already discussed (Ch. VI. §32) we might

expect to find something like a general justification of the

amount of surprise.

12. The answer commonly given in these cases is

confined to attempting to show that the surprise should not

arise, rather than to explaining how it does arise. It takes

the following form,--'You have no right to be surprised, for

nothing remarkable has really occurred. If this particular

thing had not happened something equally improbable

must. If the shot had not hit you or your friend, it must

have hit some one else who was \_à priori\_ as unlikely to be

hit.'

For one thing this answer does not explain the fact

that almost every one \_is\_ surprised in such cases, and surprised

somewhat in the different proportions mentioned

above. Moreover it has the inherent unsatisfactoriness

of admitting that something improbable has really happened,

but getting over the difficulty by saying that all the

other alternatives were equally improbable. A natural inference

from this is that there is a class of things, in themselves

really improbable, which can yet be established upon

very slight evidence. Butler accepted this inference, and

worked it out to the strange conclusion given above. Mill

attempts to avoid it by the consideration of the very different

values to be assigned to improbability before and after

the event. Some further discussion of this point will be

found in the chapter on Fallacies, and in that on the Credibility

of Extraordinary Stories.

13. In connection with the subject at present under

discussion we will now take notice of a distinction which we

shall often find insisted on in works on Probability, but to

which apparently needless importance has been attached.

It is frequently said that probability is \_relative\_, in the sense

that it has a different value to different persons according

to their respective information upon the subject in question.

For example, two persons, A and B, are going to draw

a ball from a bag containing 4 balls: A knows that the

balls are black and white, but does not know more;

B knows that three are black and one white. It would

be said that the probability of a white ball to A is 1/2, and

to B 1/4.

When however we regard the subject from the material

standing point, there really does not seem to me much more

in this than the principle, equally true in every other science,

that our inferences will vary according to the data we assume.

We might on logical grounds with almost equal

propriety speak of the area of a field or the height of a

mountain being relative, and therefore having one value to

one person and another to another. The real meaning of the

example cited above is this: A supposes that he is choosing

white at random out of a series which in the long run would

give white and black equally often; B supposes that he

is choosing white out of a series which in the long run would

give three black to one white. By the application, therefore,

of a precisely similar rule they draw different conclusions;

but so they would under the same circumstances in

any other science. If two men are measuring the height of

a mountain, and one supposes his base to be 1000 feet,

whilst the other takes it to be 1001, they would of course

form different opinions about the height. The science of

mensuration is not supposed to have anything to do with

the truth of the data, but assumes them to have been correctly

taken; why should not this be equally the case with

Probability, making of course due allowance for the peculiar

character of the data with which it is concerned?

14. This view of the relativeness of probability is

connected, as it appears to me, with the subjective view of

the science, and is indeed characteristic of it. It seems a

fair illustration of the weak side of that view, that it should

lead us to lay any stress on such an expression. As was

fully explained in the last chapter, in proportion as we work

out the Conceptualist principle we are led away from the

fundamental question of the material logic, viz. Is our belief

actually correct, or not? and, if the former, to what extent

and degree is it correct? We are directed rather to ask,

What belief does any one as a matter of fact hold? And,

since the belief thus entertained naturally varies according

to the circumstances and other sources of information of the

person in question, its relativeness comes to be admitted as

inevitable, or at least it is not to be wondered at if such

should be the case.

On our view of Probability, therefore, its 'relativeness' in

any given case is a misleading expression, and it will be

found much preferable to speak of the effect produced by

variations in the nature and amount of the data which we

have before us. Now it must be admitted that there are

frequently cases in our science in which such variations are

peculiarly likely to be found. For instance, I am expecting

a friend who is a passenger in an ocean steamer. There are

a hundred passengers on board, and the crew also numbers

a hundred. I read in the papers that one person was lost by

falling overboard; my anticipation that it was my friend who

was lost is but small, of course. On turning to another

paper, I see that the man who was lost was a passenger, not

one of the crew; my slight anxiety is at once doubled. But

another account adds that it was an Englishman, and on

that line at that season the English passengers are known

to be few; I at once begin to entertain decided fears. And

so on, every trifling bit of information instantly affecting my

expectations.

15. Now since it is peculiarly characteristic of Probability,

as distinguished from Induction, to be thus at the

mercy, so to say, of every little fact that may be floating

about when we are in the act of forming our opinion, what

can be the harm (it may be urged) of expressing this state

of things by terming our state of expectation \_relative\_?

There seem to me to be two objections. In the first place,

as just mentioned, we are induced to reject such an expression

on grounds of consistency. It is inconsistent with the

general spirit and treatment of the subject hitherto adopted,

and tends to divorce Probability from Inductive logic instead

of regarding them as cognate sciences. We are aiming at

truth, as far as that goal can be reached by our road, and

therefore we dislike to regard our conclusions as relative in

any other sense than that in which truth itself may be said

to be relative.

In the second place, this condition of unstable assent,

this constant liability to have our judgment affected, to any

degree and at any moment, by the accession of new knowledge,

though doubtless characteristic of Probability, does

not seem to me characteristic of it in its sounder and more

legitimate applications. It seems rather appropriate to a

precipitate judgment formed in accordance with the rules,

than a strict example of their natural employment. Such

precipitate judgments may occur in the case of ordinary deductive

conclusions. In the practical exigencies of life we

are constantly in the habit of forming a hasty opinion with

nearly full confidence, at any rate temporarily, upon the

strength of evidence which we must well know at the time

cannot be final. We wait a short time, and something else

turns up which induces us to alter our opinion, perhaps to

reverse it. Here our conclusions may have been perfectly

sound under the given circumstances, that is, they may be

such as every one else would have drawn who was bound to

make up his mind upon the data before us, and they are

unquestionably 'relative' judgments in the sense now under

discussion. And yet, I think, every one would shrink from

so terming them who wished systematically to carry out the

view that Logic was to be regarded as an organon of truth.

16. In the examples of Probability which we have

hitherto employed, we have for the most part assumed that

there was a certain body of statistics set before us on which

our conclusion was to rest. It was assumed, on the one

hand, that no direct specific evidence could be got, so that

the judgment was really to be one of Probability, and to rest

on these statistics; in other words, that nothing better than

them was available for us. But it was equally assumed, on

the other hand, that these statistics were open to the observation

of every one, so that we need not have to put up with

anything inferior to them in forming our opinion. In other

words, we have been assuming that here, as in the case of

most other sciences, those who have to draw a conclusion

start from the same footing of opportunity and information.

This, for instance, clearly is or ought to be the case when

we are concerned with games of chance; ignorance or misapprehension

of the common data is never contemplated

there. So with the statistics of life, or other insurance: so

long as our judgment is to be accurate (after its fashion) or

justifiable, the common tables of mortality are all that any

one has to go by.

17. It is true that in the case of a man's prospect of

death we should each qualify our judgment by what we

knew or reasonably supposed as to his health, habits, profession,

and so on, and should thus arrive at varying estimates.

But no one could \_justify\_ his own estimate without

appealing explicitly or implicitly to the statistical grounds on

which he had relied, and if these were not previously available

to other persons, he must now set them before their

notice. In other words, the judgments we entertain, here as

elsewhere, are only relative so long as we rest them on

grounds peculiar to ourselves. The process of justification,

which I consider to be essential to logic, has a tendency to

correct such individualities of judgment, and to set all observers

on the same basis as regards their data.

It is better therefore to regard the conclusions of Probability

as being absolute and objective, in the same sense as,

though doubtless in a far less degree than, they are in Induction.

Fully admitting that our conclusions will in many

cases vary exceedingly from time to time by fresh accessions

of knowledge, it is preferable to regard such fluctuations of

assent as partaking of the nature of precipitate judgments,

founded on special statistics, instead of depending only on

those which are common to all observers. In calling such

judgments precipitate it is not implied that there is any

blame in entertaining them, but simply that, for one reason

or another, we have been induced to form them without

waiting for the possession of the full amount of evidence,

statistical or otherwise, which might ultimately be looked

for. This explanation will suit the facts equally well, and is

more consistent with the general philosophical position maintained

in this work.

1. "Is it not self-evident that internal improbabilities of all kinds

weaken external proof? Doubtless, but to what practical purpose can

this be alleged here, when it has been proved before, that real

internal improbabilities, which rise even to moral certainty, are

overcome by the most ordinary testimony." Part II. ch. III.

2. "Miracles must not be compared to common natural events; or to

events which, though uncommon, are similar to what we daily

experience; but to the extraordinary phenomena of nature. And then

the comparison will be between the presumption against miracles, and

the presumption against such uncommon appearances, suppose as

comets,".... Part II. ch. II.

3. For instance, Sir J. F. Stephen explains it by drawing a

distinction between chances and probabilities, which he says that

Butler has confused together; "the objection that very ordinary

proof will overcome a presumption of millions to one is based upon a

confusion between probabilities and chances. The probability of an

event is its capability of being proved. Its chance is the numerical

proportion between the number of possible cases--supposed to be

equally favourable--favourable to its occurrence; and the number of

possible cases unfavourable to its occurrence" (\_General view of the

Criminal Law of England\_, p. 255). Donkin, again (\_Phil. Magazine\_,

June, 1851), employs the terms improbability and incredibility to

mark the same distinction.

4. In the extreme case of the witness himself merely guessing, or

being as untrustworthy as if he merely guessed, the two stories will

of course stand on precisely the same footing. This case will be

noticed again in Chapter XVII. It may be remarked that there are

several subtleties here which cannot be adequately noticed without

some previous investigation into the question of the credibility of

witnesses.

5. According to Dante, something resembling this prevailed amongst the

occupants of the \_Inferno\_. The cardinals and others whom he there

meets are able to \_give\_ information about many events which were

yet to happen upon earth, but they had to \_ask\_ it for many events

which actually had happened.

CHAPTER XIII.

\_ON THE CONCEPTION AND TREATMENT OF MODALITY.\_

1. The reader who knows anything of the scholastic

Logic will have perceived before now that we have been

touching in a variety of places upon that most thorny and

repulsive of districts in the logical territory;--modality. It

will be advisable, however, to put together, somewhat more

definitely, what has to be said upon the subject. I propose,

therefore, to devote this chapter to a brief account of the

principal varieties of treatment which the modals have received

at the hands of professed logicians.

It must be remarked at the outset that the sense in

which modality and modal propositions have been at various

times understood, is by no means fixed and invariably the

same. This diversity of view has arisen partly from corresponding

differences in the view taken of the province and

nature of logic, and partly from differences in the philosophical

and scientific opinions entertained as to the constitution

and order of nature. In later times, moreover,

another very powerful agent in bringing about a change in

the treatment of the subject must be recognized in the

gradual and steady growth of the theory of Probability, as

worked out by the mathematicians from their own point of

view.

2. In spite, however, of these differences of treatment,

there has always been some community of subject-matter in

the discussions upon this topic. There has almost always

been some reference to quantity of belief; enough perhaps

to justify De Morgan's[1] remark, that Probability was "the

unknown God whom the schoolmen ignorantly worshipped

when they so dealt with this species of enunciation, that it

was said to be beyond human determination whether they

most tortured the modals, or the modals them." But this

reference to quantity of belief has sometimes been direct and

immediate, sometimes indirect and arising out of the nature

of the subject-matter of the proposition. The fact is, that

that distinction between the purely subjective and purely objective

views of logic, which I have endeavoured to bring out

into prominence in the eleventh chapter, was not by any

means clearly recognized in early times, nor indeed before

the time of Kant, and the view to be taken of modality

naturally shared in the consequent confusion. This will, I

hope, be made clear in the course of the following chapter,

which is intended to give a brief sketch of the principal

different ways in which the modality of propositions has

been treated in logic. As it is not proposed to give anything

like a regular history of the subject, there will be no

necessity to adhere to any strict sequence of time, or to

discuss the opinions of any writers, except those who may be

taken as representative of tolerably distinct views. The

outcome of such investigation will be, I hope, to convince

the reader (if, indeed, he had not come to that conviction

before), that the logicians, after having had a long and fair

trial, have failed to make anything satisfactory out of this

subject of the modals by their methods of enquiry and treatment;

and that it ought, therefore, to be banished entirely

from that science, and relegated to Probability.

3. From the earliest study of the syllogistic process

it was seen that, complete as that process is within its own

domain, the domain, at any rate under its simplest treatment,

is a very limited one. Propositions of the pure form,--All

(or some) A is (or is not) B,--are found in practice to form

but a small portion even of our categorical statements. We

are perpetually meeting with others which express the relation

of B to A with various degrees of necessity or probability;

e.g. A must be B, A may be B; or the effect of

such facts upon our judgment, e.g. I am perfectly certain

that A is B, I think that A may be B; with many others of

a more or less similar type. The question at once arises,

How are such propositions to be treated? It does not seem

to have occurred to the old logicians, as to some of their successors

in modern times, simply to reject all consideration of

this topic. Their faith in the truth and completeness of

their system of inference was far too firm for them to suppose

it possible that forms of proposition universally recognized

as significant in popular speech, and forms of inference

universally recognized there as valid, were to be omitted

because they were inconvenient or complicated.

4. One very simple plan suggests itself, and has indeed

been repeatedly advocated, viz. just to transfer all that

is characteristic of such propositions into that convenient receptacle

for what is troublesome elsewhere, the predicate.[2]

Has not another so-called modality been thus got rid of?[3]

and has it not been attempted by the same device to abolish

the distinctive characteristic of negative propositions, viz. by

shifting the negative particle into the predicate? It must

be admitted that, up to a certain point, something may be

done in this way. Given the reasoning, 'Those who take

arsenic will probably die; A has taken it, therefore he will

probably die;' it is easy to convert this into an ordinary

syllogism of the pure type, by simply wording the major,

'Those who take arsenic are people-who-will-probably-die,'

when the conclusion follows in the same form, 'A is one

who-will-probably-die.' But this device will only carry us

a very little way. Suppose that the minor premise also is

of the same modal description, e.g. 'A has probably taken

arsenic,' and it will be seen that we cannot relegate the

modality here also to the predicate without being brought to

a stop by finding that there are four terms in the syllogism.

But even if there were not this particular objection, it

does not appear that anything is to be gained in the way of

intelligibility or method by such a device as the above. For

what is meant by a modal predicate, by the predicate

'probably mortal,' for instance, in the proposition 'All poisonings

by arsenic are probably mortal'? If the analogy with

ordinary pure propositions is to hold good, it must be a

predicate referring to the \_whole\_ of the subject, for the subject

is distributed. But then we are at once launched into

the difficulties discussed in a former chapter (Ch. VI. §§19-25),

when we attempt to justify or verify the application of

the predicate. We have to enquire (at least on the view

adopted in this work) whether the application of the predicate

'probably mortal' to the \_whole\_ of the subject, really

means at bottom anything else than that the predicate

'mortal' is to be applied to a \_portion\_ (more than half) of the

members denoted by the subject. When the transference of

the modality to the predicate raises such intricate questions

as to the sense in which the predicate is to be interpreted,

there is surely nothing gained by the step.

5. A second, and more summary way of shelving all

difficulties of the subject, so far at least as logic, or the

writers upon logic, are concerned, is found by simply denying

that modality has any connection whatever with logic. This

is the course adopted by many modern writers, for instance,

by Hamilton and Mansel, in reference to whom one cannot

help remarking that an unduly large portion of their logical

writings seems occupied with telling us what does \_not\_ belong

to logic. They justify their rejection on the ground that

the mode belongs to the matter, and must be determined by

a consideration of the matter, and therefore is extralogical.

To a certain extent I agree with their grounds of rejection,

for (as explained in Chapter VI.) it is not easy to see how

the degree of modality of any proposition, whether premise

or conclusion, can be justified without appeal to the matter.

But then questions of justification, in any adequate sense of

the term, belong to a range of considerations somewhat alien

to Hamilton's and Mansel's way of regarding the science.

The complete justification of our inferences is a matter

which involves their truth or falsehood, a point with which

these writers do not much concern themselves, being only

occupied with the consistency of our reasonings, not with

their conformity with fact. Were I speaking as a Hamiltonian

I should say that modality \_is\_ formal rather than

material, for though we cannot justify the degree of our

belief of a proposition without appeal to the matter, we can

to a moderate degree of accuracy estimate it without any

such appeal; and this would seem to be quite enough to

warrant its being regarded as formal.

It must be admitted that Hamilton's account of the

matter when he is recommending the rejection of the modals,

is not by any means clear and consistent. He not only fails,

as already remarked, to distinguish between the formal and

the material (in other words, the true and the false) modality;

but when treating of the former he fails to distinguish

between the extremely diverse aspects of modality when

viewed from the Aristotelian and the Kantian stand-points.

Of the amount and significance of this difference we shall

speak presently, but it may be just pointed out here that

Hamilton begins (Vol. I. p. 257) by rejecting the modals on

the ground that the distinctions between the necessary, the

contingent, the possible, and the impossible, must be wholly

rested on an appeal to the matter of the propositions, in

which he is, I think, quite correct. But then a little further

on (p. 260), in explaining 'the meaning of three terms which

are used in relation to pure and modal propositions,' he gives

the widely different Kantian, or three-fold division into the

apodeictic, the assertory, and the problematic. He does not

take the precaution of pointing out to his hearers the very

different general views of logic from which these two accounts

of modality spring.[4]

6. There is one kind of modal syllogism which it

would seem unreasonable to reject on the ground of its not

being formal, and which we may notice in passing. The

premise 'Any A is probably B,' is equivalent to 'Most A are B.'

Now it is obvious that from two such premises as 'Most A

are B,' 'Most A are C,' we can deduce the consequence,

'Some C are B.' Since this holds good whatever may be

the nature of A, B, and C, it is, according to ordinary usage

of the term, a formal syllogism. Mansel, however, refuses to

admit that any such syllogisms belong to formal logic. His

reasons are given in a rather elaborate review[5] and criticism

of some of the logical works of De Morgan, to whom the

introduction of 'numerically definite syllogisms' is mainly

due. Mansel does not take the particular example given

above, as he is discussing a somewhat more comprehensive

algebraic form. He examines it in a special numerical

example:[6]--18 out of 21 Ys are X; 15 out of 21 Ys are Z;

the conclusion that 12 Zs are X is rejected from formal logic

on the ground that the arithmetical judgment involved is

synthetical, not analytical, and rests upon an intuition of

quantity. We cannot enter upon any examination of these

reasons here; but it may merely be remarked that his

criticism demands the acceptance of the Kantian doctrines

as to the nature of arithmetical judgments, and that it would

be better to base the rejection not on the ground that the

syllogism is not \_formal\_, but on the ground that it is not

\_analytical\_.

7. There is another and practical way of getting rid

of the perplexities of modal reasoning which must be noticed

here. It is the resource of ordinary reasoners rather than the

decision of professed logicians,[7] and, like the first method of

evasion already pointed out in this chapter, is of very partial

application. It consists in treating the premises, during the

process of reasoning, as if they were pure, and then reintroducing

the modality into the conclusion, as a sort of

qualification of its full certainty. When each of the premises

is nearly certain, or when from any cause we are not

concerned with the extent of their departure from full certainty,

this rough expedient will answer well enough. It is,

I apprehend, the process which passes through the minds of

most persons in such cases, in so far as they reason consciously.

They would, presumably, in such an example as that previously

given (§4), proceed as if the premises that 'those

who take arsenic will die,' and that 'the man in question

has taken it,' were quite true, instead of being only probably

true, and they would consequently draw the conclusion that

'he would die.' But bearing in mind that the premises are

not certain, they would remember that the conclusion was

only to be held with a qualified assent. This they would

express quite correctly, if the mere nature and not the

degree of that assent is taken into account, by saying that

'he is likely to die.' In this case the modality is rejected

temporarily from the premises to be reintroduced into the

conclusion.

It is obvious that such a process as this is of a very

rough and imperfect kind. It does, in fact, omit from accurate

consideration just the one point now under discussion.

It takes no account of the varying shades of expression by

which the degree of departure from perfect conviction is

indicated, which is of course the very thing with which

modality is intended to occupy itself. At best, therefore, it

could only claim to be an extremely rude way of deciding

questions, the accurate and scientific methods of treating

which are demanded of us.

8. In any employment of applied logic we have of

course to go through such a process as that just mentioned.

Outside of pure mathematics it can hardly ever be the case

that the premises from which we reason are held with absolute

conviction. Hence there must be a lapse from absolute

conviction in the conclusion. But we reason on the hypothesis

that the premises are true, and any trifling defection

from certainty, of which we may be conscious, is mentally

reserved as a qualification to the conclusion. But such considerations

as these belong rather to ordinary applied logic;

they amount to nothing more than a caution or hint to be

borne in mind when the rules of the syllogism, or of induction,

are applied in practice. When, however, we are

treating of modality, the extent of the defection from full

certainty is supposed to be sufficiently great for our language

to indicate and appreciate it. What we then want is of

course a scientific discussion of the principles in accordance

with which this departure is to be measured and expressed,

both in our premises and in our conclusion. Such a plan

therefore for treating modality, as the one under discussion,

is just as much a banishment of it from the field of real

logical enquiry, as if we had determined avowedly to reject it

from consideration.

9. Before proceeding to a discussion of the various

ways in which modality may be treated by those who admit

it into logic, something must be said to clear up a possible

source of confusion in this part of the subject. In the

cases with which we have hitherto been mostly concerned,

in the earlier chapters of this work, the characteristic of

modality (for in this chapter we may with propriety use this

logical term) has generally been found in singular and particular

propositions. It presented itself when we had to

judge of individual cases from a knowledge of the average,

and was an expression of the fact that the proposition relating

to these individuals referred to a portion only of the

whole class from which the average was taken. Given that

of men of fifty-five, three out of five will die in the course of

twenty years, we have had to do with propositions of the

vague form, 'It is probable that AB (of that age) will die,'

or of the more precise form, 'It is three to two that AB will

die,' within the specified time. Here the modal proposition

naturally presents itself in the form of a singular or particular

proposition.

10. But when we turn to ordinary logic we may find

\_universal\_ propositions spoken of as modal. This must mostly

be the case with those which are termed necessary or impossible,

but it may also be the case with the probable. We

may meet with the form 'All X is probably Y.' Adopting

the same explanation here as has been throughout adopted in

analogous cases, we must say that what is meant by the

modality of such a proposition is the proportional number of

times in which the universal proposition would be correctly

made. And in this there is, so far, no difficulty. The only

difference is that whereas the justification of the former, viz.

the particular or individual kind of modal, was obtainable

within the limits of the universal proposition which included

it, the justification of the modality of a universal proposition

has to be sought in a group or succession of other propositions.

The proposition has to be referred to some group of

similar ones and we have to consider the proportion of cases

in which it will be true. But this distinction is not at all

fundamental.

It is quite true that universal propositions from their

nature are much less likely than individual ones to be justified,

in practice, by such appeal. But, as has been already

frequently pointed out, we are not concerned with the way

in which our propositions are practically obtained, nor with

the way in which men might find it most natural to test

them; but with that ultimate justification to which we appeal

in the last resort, and which has been abundantly shown

to be of a statistical character. When, therefore, we say that

'it is probable that all X is Y,' what we mean is, that in

more than half the cases we come across we should be right

in so judging, and in less than half the cases we should be

wrong.

11. It is at this step that the possible ambiguity is

encountered. When we talk of the chance that All X is Y,

we contemplate or imply the complementary chance that it is

not so. Now this latter alternative is not free from ambiguity.

It might happen, for instance, in the cases of failure,

that no X is Y, or it might happen that some X, only, is not Y;

for both of these suppositions contradict the original proposition,

and are therefore instances of its failure. In practice,

no doubt, we should have various recognized rules and

inductions to fall back upon in order to decide between these

alternatives, though, of course, the appeal to them would be

in strictness extralogical. But the mere existence of such an

ambiguity, and the fact that it can only be cleared up by

appeal to the subject-matter, are in themselves no real

difficulty in the application of the conception of modality to

universal propositions as well as to individual ones.

12. Having noticed some of the ways in which the

introduction of modality into logic has been evaded or rejected,

we must now enter into a brief account of its treatment

by those who have more or less deliberately admitted

its claims to acceptance.

The first enquiry will be, What opinions have been held

as to the nature of modality? that is, Is it primarily an affection

of the matter of the proposition, and, if not, what is it

exactly? In reference to this enquiry it appears to me, as

already remarked, that amongst the earlier logicians no such

clear and consistent distinction between the subjective and

objective views of logic as is now commonly maintained, can

be detected.[8] The result of this appears in their treatment

of modality. This always had some reference to the subjective

side of the proposition, viz. in this case to the nature

or quantity of the belief with which it was entertained; but

it is equally clear that this characteristic was not estimated

at first hand, so to say, and in itself, but rather from a consideration

of the matter determining what it should be. The

commonly accepted scholastic or Aristotelian division, for

instance, is into the necessary, the contingent, the possible,

and the impossible. This is clearly a division according to

the matter almost entirely, for on the purely mental side the

necessary and the impossible would be just the same; one

implying full conviction of the truth of a proposition, and the

other of that of its contradictory. So too, on the same side, it

would not be easy to distinguish between the contingent and

the possible. On the view in question, therefore, the modality

of a proposition was determined by a reference to the nature

of the subject-matter. In some propositions the nature of

the subject-matter decided that the predicate was necessarily

joined to the subject; in others that it was impossible that

they should be joined; and so on.

13. The artificial character of such a four-fold division

will be too obvious to modern minds for it to be necessary to

criticize it. A very slight study of nature and consequent

appreciation of inductive evidence suffice to convince us that

those uniformities upon which all connections of phenomena,

whether called necessary or contingent, depend, demand extremely

profound and extensive enquiry; that they admit of

no such simple division into clearly marked groups; and

that, therefore, the pure logician had better not meddle with

them.[9]

The following extract from Grote's \_Aristotle\_ (Vol. I. p. 192)

will serve to show the origin of this four-fold division,

its conformity with the science of the day, and consequently

its utter want of conformity with that of our own time:--"The

distinction of Problematical and Necessary Propositions

corresponds, in the mind of Aristotle, to that capital and

characteristic doctrine of his Ontology and Physics, already

touched on in this chapter. He thought, as we have seen,

that in the vast circumferential region of the Kosmos, from

the outer sidereal sphere down to the lunar sphere, celestial

substance was a necessary existence and energy, sempiternal

and uniform in its rotations and influence; and that through

its beneficent influence, pervading the concavity between the

lunar sphere and the terrestrial centre (which included the

four elements with their compounds) there prevailed a regularizing

tendency called Nature; modified, however, and

partly counteracted by independent and irregular forces called

Spontaneity and Chance, essentially unknowable and unpredictable.

The irregular sequences thus named by Aristotle

were the objective correlate of the Problematical Proposition

in Logic. In these sublunary sequences, as to future time,

\_may or may not\_, was all that could be attained, even by the

highest knowledge; certainty, either of affirmation or negation,

was out of the question. On the other hand, the necessary

and uniform energies of the celestial substance, formed

the objective correlate of the Necessary Proposition in Logic;

this substance was not merely an existence, but an existence

necessary and unchangeable... he considers the Problematical

Proposition in Logic to be not purely subjective, as an

expression of the speaker's ignorance, but something more,

namely, to correlate with an objective essentially unknowable

to all."

14. Even after this philosophy began to pass away,

the divisions of modality originally founded upon it might

have proved, as De Morgan has remarked,[10] of considerable

service in mediæval times. As he says, people were much

more frequently required to decide in one way or the other

upon a single testimony, without there being a sufficiency of

specific knowledge to test the statements made. The old

logician "did not know but that any day of the week might

bring from Cathay or Tartary an account of men who ran on

four wheels of flesh and blood, or grew planted in the ground,

like Polydorus in the Æneid, as well evidenced as a great

many nearly as marvellous stories." Hence, in default of

better inductions, it might have been convenient to make

rough classifications of the facts which were and which were

not to be accepted on testimony (the necessary, the impossible, &c.),

and to employ these provisional inductions (which

is all we should now regard them) as testing the stories

which reached him. Propositions belonging to the class of

the impossible might be regarded as having an antecedent

presumption against them so great as to prevail over almost

any testimony worth taking account of, and so on.

15. But this old four-fold division of modals continued

to be accepted and perpetuated by the logicians long

after all philosophical justification for it had passed away.

So far as I have been able to ascertain, scarcely any logician

of repute or popularity before Kant, was bold enough to

make any important change in the way of regarding them.[11]

Even the Port-Royal Logic, founded as it is on Cartesianism,

repeats the traditional statements, though with extreme

brevity. This adherence to the old forms led, it need not be

remarked, to considerable inconsistency and confusion in

many cases. These forms were founded, as we have seen, on

an objective view of the province of logic, and this view was

by no means rigidly carried out in many cases. In fact it

was beginning to be abandoned, to an extent and in directions

which we have not opportunity here to discuss, before

the influence of Kant was felt. Many, for instance, added to

the list of the four, by including the true and the false;

occasionally also the probable, the supposed, and the certain

were added. This seems to show some tendency towards

abandoning the objective for the subjective view, or at least

indicates a hesitation between them.

16. With Kant's view of modality almost every one is

familiar. He divides judgments, under this head, into the

apodeictic, the assertory, and the problematic. We shall have

to say something about the number and mutual relations of

these divisions presently; we are now only concerned with

the general view which they carry out. In this respect it will

be obvious at once what a complete change of position has

been reached. The 'necessary' and the 'impossible' demanded

an appeal to the matter of a proposition in order to

recognize them; the 'apodeictic' and the 'assertory', on the

other hand, may be true of almost any matter, for they

demand nothing but an appeal to our consciousness in order

to distinguish between them. Moreover, the distinction

between the assertory and the problematic is so entirely

subjective and personal, that it may vary not only between

one person and another, but in the case of the same person

at different times. What one man knows to be true, another

may happen to be in doubt about. The apodeictic judgment

is one which we not only accept, but which we find ourselves

unable to reverse in thought; the assertory is simply accepted;

the problematic is one about which we feel in

doubt.

This way of looking at the matter is the necessary outcome

of the conceptualist or Kantian view of logic. It has

been followed by many logicians, not only by those who may

be called followers of Kant, but by almost all who have felt

his influence. Ueberweg, for instance, who is altogether at

issue with Kant on some fundamental points, adopts it.

17. The next question to be discussed is, How many

subdivisions of modality are to be recognized? The Aristotelian

or scholastic logicians, as we have seen, adopted a four-fold

division. The exact relations of some of these to each

other, especially the possible and the contingent, is an extremely

obscure point, and one about which the commentators

are by no means agreed. As, however, it seems tolerably

clear that it was not consciously intended by the use of these

four terms to exhibit a graduated scale of intensity of conviction,

their correspondence with the province of modern

probability is but slight, and the discussion of them, therefore,

becomes rather a matter of special or antiquarian interest.

De Morgan, indeed (\_Formal Logic\_, p. 232), says that

the schoolmen understood by contingent more likely than

not, and by possible less likely than not. I do not know

on what authority this statement rests, but it credits them

with a much nearer approach to the modern views of probability

than one would have expected, and decidedly nearer than

that of most of their successors.[12] The general conclusion at

which I have arrived, after a reasonable amount of investigation,

is that there were two prevalent views on the subject.

Some (e.g. Burgersdyck, Bk. I. ch. 32) admitted that there

were at bottom only two kinds of modality; the contingent

and the possible being equipollent, as also the necessary and

the impossible, provided the one asserts and the other

denies. This is the view to which those would naturally

be led who looked mainly to the nature of the subject-matter.

On the other hand, those who looked mainly at the form of

expression, would be led by the analogy of the four forms of

proposition, and the necessity that each of them should stand

in definite opposition to each other, to insist upon a distinction

between the four modals.[13] They, therefore, endeavoured

to introduce a distinction by maintaining (e.g. Crackanthorpe,

Bk. III. ch. 11) that the contingent is that which now is but

may not be, and the possible that which now is not but may

be. A few appear to have made the distinction correspondent

to that between the physically and the logically

possible.

18. When we get to the Kantian division we have

reached much clearer ground. The meaning of each of these

terms is quite explicit, and it is also beyond doubt that they

have a more definite tendency in the direction of assigning a

graduated scale of conviction. So long as they are regarded

from a metaphysical rather than a logical standing point,

there is much to be said in their favour. If we use introspection

merely, confining ourselves to a study of the judgments

themselves, to the exclusion of the grounds on which

they rest, there certainly does seem a clear and well-marked

distinction between judgments which we cannot even conceive

to be reversed in thought; those which we could

reverse, but which we accept as true; and those which we

merely entertain as possible.

Regarded, however, as a logical division, Kant's arrangement

seems to me of very little service. For such logical

purposes indeed, as we are now concerned with, it really

seems to resolve itself into a \_two\_-fold division. The distinction

between the apodeictic and the assertory will be

admitted, I presume, even by those who accept the metaphysical

or psychological theory upon which it rests, to be a

difference which concerns, not the quantity of belief with

which the judgments are entertained, but rather the violence

which would have to be done to the mind by the attempt to

upset them. Each is fully believed, but the one can, and

the other cannot, be controverted. The belief with which an

assertory judgment is entertained is full belief, else it would

not differ from the problematic; and therefore in regard to

the quantity of belief, as distinguished from the quality or

character of it, there is no difference between it and the apodeictic.

It is as though, to offer an illustration, the index

had been already moved to the top of the scale in the assertory

judgment, and all that was done to convert this into

an apodeictic one, was to \_clamp\_ it there. The only logical

difference which then remains is that between problematic

and assertory, the former comprehending all the judgments

as to the truth of which we have any degree of doubt, and

the latter those of which we have no doubt. The whole

range of the former, therefore, with which Probability is

appropriately occupied, is thrown undivided into a single

compartment. We can hardly speak of a 'division' where

one class includes everything up to the boundary line, and

the other is confined to that boundary line. Practically,

therefore, on this view, modality, as the mathematical student

of Probability would expect to find it, as completely

disappears as if it were intended to reject it.

19. By less consistent and systematic thinkers, and

by those in whom ingenuity was an over prominent feature,

a variety of other arrangements have been accepted or proposed.

There is, of course, some justification for such attempts

in the laudable desire to bring our logical forms into better

harmony with ordinary thought and language. In practice,

as was pointed out in an earlier chapter, every one recognizes

a great variety of modal forms, such as 'likely,' 'very

likely,' 'almost certainly,' and so on almost without limit in

each direction. It was doubtless supposed that, by neglecting

to make use of technical equivalents for some of these

forms, we should lose our logical control over certain possible

kinds of inference, and so far fall short even of the precision

of ordinary thought.

With regard to such additional forms, it appears to me

that all those which have been introduced by writers who

were uninfluenced by the Theory of Probability, have done

little else than create additional confusion, as such writers do

not attempt to marshal their terms in order, or to ascertain

their mutual relations. Omitting, of course, forms obviously

of material modality, we have already mentioned the true

and the false; the probable, the supposed, and the certain.

These subdivisions seem to have reached their climax at a

very early stage in Occam (Prantl, III. 380), who held that a

proposition might be modally affected by being 'vera, scita,

falsa, ignota, scripta, prolata, concepta, credita, opinata, dubitata.'

20. Since the growth of the science of Probability,

logicians have had better opportunities of knowing what

they had to aim at; and, though it cannot be said that their

attempts have been really successful, these are at any rate a

decided improvement upon those of their predecessors. Dr

Thomson,[14] for instance, gives a nine-fold division. He says

that, arranging the degrees of modality in an ascending

scale, we find that a judgment may be either possible,

doubtful, probable, morally certain for the thinker himself,

morally certain for a class or school, morally certain for all,

physically certain with a limit, physically certain without

limitation, and mathematically certain. Many other divisions

might doubtless be mentioned, but, as every mathematician

will recognize, the attempt to secure any general

agreement in such a matter of arrangement is quite hopeless.

It is here that the beneficial influence of the mathematical

theory of Probability is to be gratefully acknowledged. As

soon as this came to be studied it must have been perceived

that in attempting to mark off clearly from one another

certain gradations of belief, we should be seeking for breaches

in a continuous magnitude. In the advance from a slight

presumption to a strong presumption, and from that to moral

certainty, we are making a gradual ascent, in the course of

which there are no natural halting-places. The proof of this

continuity need not be entered upon here, for the materials

for it will have been gathered from almost every chapter of

this work. The reader need merely be reminded that the

grounds of our belief, in all cases which admit of number and

measurement, are clearly seen to be of this description; and

that therefore unless the belief itself is to be divorced from

the grounds on which it rests, what thus holds as to their

characteristics must hold also as to its own.

It follows, therefore, that modality in the old sense of the

word, wherein an attempt was made to obtain certain natural

divisions in the scale of conviction, must be finally abandoned.

All that it endeavoured to do can now be done incomparably

better by the theory of Probability, with its numerical scale

which admits of indefinite subdivision. None of the old systems

of division can be regarded as a really natural one;

those which admit but few divisions being found to leave the

whole range of the probable in one unbroken class, and those

which adopt many divisions lapsing into unavoidable vagueness

and uncertainty.

21. Corresponding to the distinction between pure

and modal propositions, but even more complicated and

unsatisfactory in its treatment, was that between pure and

modal syllogisms. The thing discussed in the case of the

latter was, of course, the effect produced upon the conclusion

in respect of modality, by the modal affection of one or both

premises. It is only when we reach such considerations as

these that we are at all getting on to the ground appropriate

to Probability; but it is obvious that very little could be

done with such rude materials, and the inherent clumsiness

and complication of the whole modal system come out very

clearly here. It was in reference probably to this complication

that some of the bitter sayings[15] of the schoolmen and

others which have been recorded, were uttered.

Aristotle has given an intricate investigation of this subject,

and his followers naturally were led along a similar

track. It would be quite foreign to my purpose in the slight

sketch in this chapter to attempt to give any account of

these enquiries, even were I competent to do so; for, as has

been pointed out, the connection between the Aristotelian

modals and the modern view of the nature of Probability,

though real, is exceedingly slight. It need only be remarked

that what was complicated enough with four modals to be

taken account of, grows intricate beyond all endurance when

such as the 'probable' and the 'true' and the 'false' have

also to be assigned a place in the list. The following examples[16]

will show the kind of discussions with which the logicians

exercised themselves. 'Whether, with one premise

certain, and the other probable, a certain conclusion may be

inferred': 'Whether, from the impossible, the necessary can

be inferred'; 'Whether, with one premise necessary and the

other \_de inesse\_, the conclusion is necessary', and so on,

endlessly.

22. On the Kantian view of modality the discussion

of such kinds of syllogisms becomes at once decidedly more

simple (for here but three modes are recognized), and also

somewhat more closely connected with strict Probability, (for

the modes are more nearly of the nature of gradations of

conviction). But, on the other hand, there is less justification

for their introduction, as logicians might really be expected

to know that what they are aiming to effect by their clumsy

contrivances is the very thing which Probability can carry

out to the highest desired degree of accuracy. The former

methods are as coarse and inaccurate, compared with the

latter, as were the roughest measurements of Babylonian

night-watchers compared with the refined calculations of the

modern astronomer. It is indeed only some of the general

adherents of the Kantian Logic who enter upon any such

considerations as these; some, such as Hamilton and Mansel,

entirely reject them, as we have seen. By those who do

treat of the subject, such conclusions as the following are laid

down; that when both premises are apodeictic the conclusion

will be the same; so when both are assertory or problematic.

If one is apodeictic and the other assertory, the latter, or

'weaker,' is all that is to be admitted for the conclusion;

and so on. The English reader will find some account of

these rules in Ueberweg's \_Logic\_.[17]

23. But although those modals, regarded as instruments

of accurate thought, have been thus superseded by the

precise arithmetical expressions of Probability, the question

still remains whether what may be termed our popular modal

expressions could not be improved and adapted to more

accurate use. It is true that the attempt to separate them

from one another by any fundamental distinctions is futile,

for the magnitude of which they take cognizance is, as we

have remarked, continuous; but considering the enormous

importance of accurate terminology, and of recognizing

numerical distinctions wherever possible, it would be a real

advance if any agreement could be arrived at with regard to

the use of modal expressions. We have already noticed (Ch. II. §16)

some suggestions by Mr Galton as to the possibility

of a natural system of classification, resting upon the regularity

with which most kinds of magnitudes tend to group

themselves about a mean. It might be proposed, for instance,

that we should agree to apply the term 'good'

to the first quarter, measuring from the best downwards;

'indifferent' to the middle half, and 'bad' to the last quarter.

There seems no reason why a similarly improved terminology

should not some day be introduced into the ordinary modal

language of common life. It might be agreed, for instance,

that 'very improbable' should as far as possible be confined

to those events which had odds of (say) more than 99 to 1

against them; and so on, with other similar expressions.

There would, no doubt, be difficulties in the way, for in all

applications of classification we have to surmount the two-fold

obstacles which lie in the way, firstly (to use Kant's

expression) of the faculty of making rules, and secondly of

that of subsumption under rules. That is to say, even if we

had agreed upon our classes, there would still be much doubt

and dispute, in the case of things which did not readily lend

themselves to be counted or measured, as to whether the

odds were more or less than the assigned quantity.

It is true that when we know the odds for or against an

event, we can always state them explicitly without the necessity

of first agreeing as to the usage of terms which shall

imply them. But there would often be circumlocution and

pedantry in so doing, and as long as modal terms are in

practical use it would seem that there could be no harm, and

might be great good, in arriving at some agreement as to the

degree of probability which they should be generally understood

to indicate. Bentham, as is well known, in despair of

ever obtaining anything accurate out of the language of common

life on this subject, was in favour of a direct appeal to

the numerical standard. He proposed the employment, in

judicial trials, of an instrument, graduated from 0 to 10, on

which scale the witness was to be asked to indicate the degree

of his belief of the facts to which he testified: similarly

the judge might express the force with which he held his

conclusion. The use of such a numerical scale, however, was

to be optional only, not compulsory, as Bentham admitted

that many persons might feel at a loss thus to measure the

degree of their belief. (\_Rationale of Judicial Evidence\_,

Bk. I., Ch. VI.)

24. Throughout this chapter we have regarded the

modals as the nearest counterpart to modern Probability

which was afforded by the old systems of logic. The reason

for so regarding them is, that they represented some slight

attempt, rude as it was, to recognize and measure certain

gradations in the degree of our conviction, and to examine

the bearing of such considerations upon our logical inferences.

But although it is amongst the modals that the germs of

the methods of Probability are thus to be sought; the true

subject-matter of our science, that is, the classes of objects

with which it is most appropriately concerned, are rather

represented by another part of the scholastic logic. This

was the branch commonly called Dialectic, in the old sense

of that term. Dialectic, according to Aristotle, seems to

have been a sort of sister art to Rhetoric. It was concerned

with syllogisms differing in no way from demonstrative syllogisms,

except that their premises were probable instead of

certain. Premises of this kind he termed topics, and the

syllogisms which dealt with them enthymemes. They were

said to start from 'signs and likelihoods' rather than from

axioms.[18]

25. The terms in which such reasonings are commonly

described sound very much like those applicable to

Probability, as we now understand it. When we hear of

likelihood, and of probable syllogisms, our first impression

might be that the inferences involved would be of a similar

character.[19] This, however, would be erroneous. In the

first place the province of this Dialectic was much too wide,

for it covered in addition the whole field of what we should

now term Scientific or Material Induction. The distinctive

characteristic of the dialectic premises was their want of

certainty, and of such uncertain premises Probability (as I

have frequently insisted) takes account of one class only,

Induction concerning itself with another class. Again, not the

slightest attempt was made to enter upon the enquiry, \_How\_

uncertain are the premises? It is only when this is attempted

that we can be considered to enter upon the field of

Probability, and it is because, after a rude fashion, the modals

attempted to grapple with this problem, that we have regarded

them as in any way occupied with our special subject-matter.

26. Amongst the older logics with which I have made

any acquaintance, that of Crackanthorpe gives the fullest discussion

upon this subject. He divides his treatment of the

syllogism into two parts, occupied respectively with the 'demonstrative'

and the 'probable' syllogism. To the latter a

whole book is devoted. In this the nature and consequences

of thirteen different 'loci'[20] are investigated, though it is not

very clear in what sense they can every one of them be regarded

as being 'probable.'

It is doubtless true, that if the old logicians had been

in possession of such premises as modern Probability is concerned

with, and had adhered to their own way of treating

them, they would have had to place them amongst such \_loci\_,

and thus to make the consideration of them a part of their

Dialectic. But inasmuch as there does not seem to have

been the slightest attempt on their part to do more here

than recognize the \_fact\_ of the premises being probable; that

is, since it was not attempted to \_measure\_ their probability

and that of the conclusion, I cannot but regard this part of

Logic as having only the very slightest relation to Probability

as now conceived. It seems to me little more than

one of the ways (described at the commencement of this

chapter) by which the problem of Modality is not indeed rejected,

but practically evaded.

27. As Logic is not the only science which is directly

and prominently occupied with questions about belief and

evidence, so the difficulties which have arisen there have

been by no means unknown elsewhere. In respect of the

modals, this seems to have been manifestly the case in Jurisprudence.

Some remarks, therefore, may be conveniently

made here upon this application of the subject, though of

course with the brevity suitable on the part of a layman who

has to touch upon professional topics.

Recall for a moment what are the essentials of modality.

These I understand to be the attempt to mark off from one

another, without any resort to numerical notation, varying

degrees of conviction or belief, and to determine the consequent

effect of premises, thus affected, upon our conclusions.

Moreover, as we cannot construct or retain a scale of any

kind without employing a standard from and by which to

measure it, the attainment and recognition of a standard of

certainty, or of one of the other degrees of conviction, is

almost inseparably involved in the same enquiry. In this

sense of the term, modal difficulties have certainly shown

themselves in the department of Law. There have been

similar attempts here, encountered by similar difficulties, to

come to some definite agreement as to a scale of arrangement

of the degrees of our assent. It is of course much

more practicable to secure such agreement in the case of a

special science, confined more or less to the experts, than in

subjects into which all classes of outsiders have almost equal

right of entry. The range of application under the former

circumstances is narrower, and the professional experts have

acquired habits and traditions by which the standards may

be retained in considerable integrity. It does not appear,

however, according to all accounts, as if any very striking

success had been attained in this direction by the lawyers.

28. The difficulty in its scientific, or strictly jurisprudential

shape, seems to have shown itself principally in the

attempt to arrange legal evidence into classes in respect of

the degree of its cogency. This, I understand, was the case

in the Roman law, and in some of the continental systems of

jurisprudence which took their rise from the Roman law.

"The direct evidence of so many witnesses was \_plena probatio\_.

Then came \_minus plena probatio\_, then \_semiplenâ

major\_ and \_semiplenâ minor\_; and by adding together a

certain number of half-proofs--for instance, by the production

of a tradesman's account-books, \_plus\_ his supplementary

oath--full proof might be made out. It was on

this principle that torture was employed to obtain a confession.

The confession was evidence suppletory to the circumstances

which were held to justify its employment."[21]

According to Bentham,[22] the corresponding scale in the

English school was:--Positive proof, Violent presumption.

Probable presumption, Light or Rash presumption. Though

admitted by Blackstone and others, I understand that these

divisions are not at all generally accepted at the present

day.

29. In the above we are reminded rather of modal

syllogisms. The principal practical form in which the difficulty

underlying the simple modal propositions presents

itself, is in the attempt to obtain some criterion of judicial

certainty. By 'certainty' here we mean, of course, not what

the metaphysicians term apodeictic,[23] for that can seldom or

never be secured in practical affairs, but such a degree of

conviction, short of this, as every reasonable person will feel

to be sufficient for all his wants. Here again, one would

think, the quest must appear, to accurate thinkers, an utterly

hopeless one; an effort to discover natural breaks in a continuous

magnitude. There cannot indeed be the least doubt

that, amongst limited classes of keen and practised intellects,

a standard of certainty, as of everything else, might be retained

and handed down with considerable accuracy: this is

possible in matters of taste and opinion where personal peculiarities

of judgment are far more liable to cause disagreement

and confusion. But then such a consensus is almost entirely

an affair of tact and custom; whereas what is wanted in the

case in question is some criterion to which the comparatively

uninitiated may be able to appeal. The standard, therefore,

must not merely be retained by recollection, but be generally

recognizable by its characteristics. If such a criterion could

be secured, its importance could hardly be overrated. But

so far as one may judge from the speeches of counsel, the

charges of judges, and the verdicts of juries, nothing really

deserving the name is ever attained.

30. The nearest approach, perhaps, to a recognized

standard is to be found in the frequent assurance that juries

are not bound to convict only in case they have \_no\_ doubt of

the guilt of the accused; for the absolute exclusion of all

doubt, the utter impossibility of suggesting any counter

hypothesis which this assumes, is unattainable in human

affairs. But, it is frequently said, they are to convict if

they have no 'reasonable doubt,' no such doubt, that is,

as would be 'a hindrance to acting in the important affairs

of life.' As a caution against seeking after unattainable

certainty, such advice may be very useful; but it need

hardly be remarked that the certainty upon which we act in

the important affairs of life is no fixed standard, but varies

exceedingly according to the nature of those affairs. The

greater the reward at stake, the greater the risk we are

prepared to run, and conversely. Hardly any degree of certainty

can exist, upon the security of which we should not

be prepared to act under appropriate circumstances.[24]

Some writers indeed altogether deny that any standard,

in the common sense of the word, either is, or ought to be,

aimed at in legal proceedings. For instance, Sir J. F.

Stephen, in his work on English Criminal Law,[25] after

noticing and rejecting such standards as that last indicated,

comes to the conclusion that the only standard recognized

by our law is that which induces juries to convict:--"What

is judicial proof? That which being permitted by

law to be given in evidence, induces twelve men, chosen

according to the Jury Act, to say that, having heard it, their

minds are satisfied of the truth of the proposition which it

affirms. They may be prejudiced, they may be timid, they

may be rash, they may be ignorant; but the oath, the

number, and the property qualification, are intended, as far

as possible, to neutralize these disadvantages, and answer

precisely to the conditions imposed upon standards of value

or length." (p. 263.)

To admit this is much about the same thing as to abandon

such a standard as unattainable. Evidence which induces

a jury to convict may doubtless be a standard to me and

others of what we ought to consider 'reasonably certain,'

provided of course that the various juries are tolerably uniform

in their conclusions. But it clearly cannot be proposed

as a standard to the juries themselves; if their decisions are

to be consistent and uniform, they want some external indication

to guide them. When a man is asking, \_How\_ certain

ought I to feel? to give such an answer as the above is,

surely, merely telling him that he is to be as certain as

he is. If, indeed, juries composed a close profession, they

might, as was said above, retain a traditional standard. But

being, as they are, a selection from the ordinary lay public,

their own decisions in the past can hardly be held up to

them as a direction what they are to do in future.

31. It would appear therefore that we may fairly say

that the English law, at any rate, definitely rejects the main

assumption upon which the logical doctrine of modality and

its legal counterpart are based: the assumption, namely, that

different grades of conviction can be marked off from one

another with sufficient accuracy for us to be able to refer

individual cases to their corresponding classes. And that

with regard to the collateral question of fixing a standard of

certainty, it will go no further than pronouncing, or implying,

that we are to be content with nothing short of, but

need not go beyond, 'reasonable certainty.'

This is a statement of the standard, with which the

logician and scientific man can easily quarrel; and they

may with much reason maintain that it has not the slightest

claim to accuracy, even if it had one to strict intelligibility.

If a man wishes to know whether his present degree of certainty

\_is\_ reasonable, whither is he to appeal? He can

scarcely compare his mental state with that which is experienced

in 'the important affairs of life,' for these, as

already remarked, would indicate no fixed value. At the

same time, one cannot suppose that such an expression is

destitute of all signification. People would not continue to

use language, especially in matters of paramount importance

and interest, without meaning something by it. We are

driven therefore to conclude that 'reasonable certainty' does

in a rude sort of way represent a traditional standard to

which it is attempted to adhere. As already remarked, this

is perfectly practicable in the case of any class of professional

men, and therefore not altogether impossible in the case of

those who are often and closely brought into connection with

such a class. Though it is hard to believe that any such

expressions, when used for purposes of ordinary life, attain

at all near enough to any conventional standard to be worth

discussion; yet in the special case of a jury, acting under

the direct influence of a judge, it seems quite possible that

their deliberate assertion that they are 'fully convinced'

may reach somewhat more nearly to a tolerably fixed standard

than ordinary outsiders would at first think likely.

32. Are there then any means by which we could

ascertain what this standard is; in other words, by which we

could determine what is the real worth, in respect of accuracy,

of this 'reasonable certainty' which the juries are supposed

to secure? In the absence of authoritative declarations

upon the subject, the student of Logic and Probability

would naturally resort to two means, with a momentary

notice of which we will conclude this enquiry.

The first of these would aim at determining the standard

of judicial certainty indirectly, by simply determining the

statistical frequency with which the decisions (say) of a jury

were found to be correct. This may seem to be a hopeless

task; and so indeed it is, but not so much on any theoretic

insufficiency of the determining elements as on account of

the numerous arbitrary assumptions which attach to most

of the problems which deal with the probability of testimony

and judgments. It is not necessary for this purpose that we

should have an infallible superior court which revised the

decisions of the one under consideration;[26] it is sufficient if a

large number of ordinary representative cases are submitted

to a court consisting even of exactly similar materials to the

one whose decisions we wish to test. Provided always that we

make the monstrous assumption that the judgments of men

about matters which deeply affect them are 'independent'

in the sense in which the tosses of pence are independent,

then the statistics of mere agreement and disagreement will

serve our purpose. We might be able to say, for instance,

that a jury of a given number, deciding by a given majority,

were right nine times out of ten in their verdict. Conclusions

of this kind, in reference to the French courts, are

what Poisson has attempted at the end of his great work on

the Probability of Judgments; though I do not suppose that

he attached much numerical accuracy to his results.

A scarcely more hopeful means would be found by a

reference to certain cases of legal 'presumptions.' A 'conclusive

presumption' is defined as follows:--"Conclusive, or

as they are elsewhere termed imperative or absolute presumptions

of law, are rules determining the quantity of evidence

requisite for the support of any particular averment

which is not permitted to be overcome by any proof that the

fact is otherwise."[27] A large number of such presumptions

will be found described in the text-books, but they seem to

refer to matters far too vague, for the most part, to admit of

any reduction to statistical frequency of occurrence. It is

indeed maintained by some authorities that any assignment

of degree of Probability is not their present object, but that

they are simply meant to exclude the troublesome delays

that would ensue if everything were considered open to

doubt and question. Moreover, even if they did assign a

degree of certainty this would rather be an indication of

what legislators or judges thought reasonable than of what

was so considered by the juries themselves.

There are indeed presumptions as to the time after which

a man, if not heard of, is supposed to be dead (capable of

disproof, of course, by his reappearance). If this time varied

with the age of the man in question, we should at once have

some such standard as we desire, for a reference to the Life

tables would fix his probable duration of life, and so determine

indirectly the measure of probability which satisfied

the law. But this is not the case; the period chosen is

entirely irrespective of age. The nearest case in point (and

that does not amount to much) which I have been able to

ascertain is that of the age after which it has been presumed

that a woman was incapable of bearing children.

This was the age of 53. A certain approach to a statistical

assignment of the chances in this case is to be found in

Quetelet's \_Physique Sociale\_ (Vol. I. p. 184, note). According

to the authorities which he there quotes it would seem that

in about one birth in 5500 the mother was of the age of 50

or upwards. This does not quite assign the degree of what

may be called the \_à priori\_ chance against the occurrence of a

birth at that age, because the fact of having commenced a

family at an early age represents some diminution of the

probability of continuing it into later life. But it serves to

give some indication of what may be called the odds against

such an event.

It need not be remarked that any such clues as these to

the measure of judicial certainty are far too slight to be of

any real value. They only deserve passing notice as a possible

logical solution of the problem in question, or rather as

an indication of the mode in which, in theory, such a solution

would have to be sought, were the English law, on those

subjects, a perfectly consistent scheme of scientific evidence.

This is the mode in which one would, under those circumstances,

attempt to extract from its proceedings an admission

of the exact measure of that standard of certainty which it

adopted, but which it declined openly to enunciate.

1. \_Formal Logic\_, p. 232.

2. This appears to be the purport of some statements in a very

confused passage in Whately's \_Logic\_ (Bk. II., ch. IV. §1). "A

modal proposition may be stated as a pure one by attaching the mode

to one of the terms, and the proposition will in all respects fall

under the foregoing rules;... 'It is probable that all knowledge is

useful;' 'probably useful' is here the predicate." He draws

apparently no such distinction as that between the true and false

modality referred to in the next note. What is really surprising is

that even Hamilton puts the two (the true and the false modality)

upon the same footing. "In regard to these [the former] the case is

precisely the same; the mode is merely a part of the predicate."

\_Logic\_, I. 257.

3. I allude of course to such examples as 'A killed B unjustly,' in

which the killing of B by A was sometimes said to be asserted not

simply but with a modification. (Hamilton's \_Logic\_, I. 256.) It is

obvious that the modification in such cases is by rights merely a

part of the predicate, there being no formal distinction between 'A

is the killer of B' and 'A is the unjust killer of B.' Indeed some

logicians who were too conservative to reject the generic name of

modality in this application adopted the common expedient of

introducing a specific distinction which did away with its meaning,

terming the spurious kind 'material modality' and the genuine kind

'formal modality'. The former included all the cases in which the

modification belonged by right either to the predicate or to the

subject; the latter was reserved for the cases in which the

modification affected the real conjunction of the predicate with the

subject. (Keckermann, \_Systema Logicæ\_, Lib. II. ch. 3.) It was, I

believe, a common scholastic distinction.

For some account of the dispute as to whether the negative particle

was to be considered to belong to the copula or to the predicate,

see Hamilton's \_Logic\_, I. 253.

4. He has also given a short discussion of the subject elsewhere

(\_Discussions\_, Ed. II. p. 702), in which a somewhat different view

is taken. The modes are indeed here admitted into logic, but only

in so far as they fall by subdivision under the relation of genus

and species, which is of course tantamount to their entire

rejection; for they then differ in no essential way from any other

examples of that relation.

5. \_Letters, Lectures and Reviews\_, p. 61. Elsewhere in the review

(p. 45) he gives what appears to me a somewhat different decision.

6. It must be remembered that this is not one of the proportional

propositions with which we have been concerned in previous chapters:

it is meant that there are \_exactly\_ 21 Ys, of which just 18 are X,

not that on the average 18 out of 21 may be so regarded.

7. I consider however, as I have said further on (p. 320), that the

treatment in the older logics of Probable syllogisms, and Dialectic

syllogisms, came to somewhat the same thing as this, though they

looked at the matter from a different point of view, and expressed

it in very different language.

8. The distinction is however by no means entirely neglected. Thus

Smiglecius, when discussing the modal affections of certainty and

necessity, says, "certitudo ad cognitionem spectat: necessitas vero

est in re" (\_Disputationes;\_ Disp. XIII., Quæst. XII.).

9. It may be remarked that Whately (\_Logic\_, Bk. II. ch. II. §2)

speaks of necessary, impossible and contingent matter, without any

apparent suspicion that they belong entirely to an obsolete point of

view.

10. \_Formal Logic\_, p. 233.

11. The subject was sometimes altogether omitted, as by Wolf. He says

a good deal however about probable propositions and syllogisms, and,

like Leibnitz before him, looked forward to a "logica probabilium"

as something new and desirable. I imagine that he had been

influenced by the writers on Chances, as of the few who had already

treated that subject nearly all the most important are referred to

in one passage (\_Philosophia Rationalis sive Logica\_, §593).

Lambert stands quite apart. In this respect, as in most others where

mathematical conceptions and symbols are involved, his logical

attitude is thoroughly unconventional. See, for instance, his

chapter 'Von dem Wahrscheinlichen', in his \_Neues Organon\_.

12. I cannot find the slightest authority for the statement in the

elaborate history of Logic by Prantl.

13. "Hi quatuor modi magnam censeri solent analogiam habere cum

quadruplici propositionum in quantitate et qualitate varietate"

(Wallis's \_Instit. Logic.\_ Bk. II. ch. 8).

14. \_Laws of Thought\_, §118.

15. "Haud scio magis ne doctrinam modalium scholastici exercuerint,

quam ea illos vexarit. Certe usque adeo sudatum hic fuit, ut

dicterio locus sit datus; \_De modalibus non gustabit asinus\_."

Keckermann, \_Syst. Log.\_ Bk. II. ch. 3.

16. \_Smiglecii Disputationes\_, Ingolstadt, 1618.

See also Prantl's \_Geschichte der Logik\_ (under \_Occam\_ and

\_Buridan\_) for accounts of the excessive complication which the

subtlety of those learned schoolmen evolved out of such suitable

materials.

17. Translation by T. M. Lindsay, p. 439.

18. "The εἰκòς and σημεῖον themselves are propositions; the former

stating a \_general probability\_, the latter a fact, which is known

to be an indication, more or less certain, of the truth of some

further statement, whether of a single fact, or of a general

belief. The former is a general proposition, nearly, though not

quite, universal; as 'most men who envy hate'; the latter is a

\_singular\_ proposition, which however is not regarded as a sign,

except relatively to some other proposition, which it is supposed

may by inferred from it." (Mansel's Aldrich; Appendix F, where an

account will be found of the Aristotelian enthymeme, and dialectic

syllogism. Also, of course, Grote's Aristotle, \_Topics\_ and

elsewhere.)

19. "Nam in hoc etiam differt demonstratio, sen demonstrativa

argumentatio, à probabili, quia in illâ tam conclusio quam præmissæ

necessariæ sunt; in probabili autem argumentatione sicut conclusio

ut probabilis infertur ita præmissæ ut probabiles afferuntur"

(Crackanthorpe, Bk. V., Ch. 1); almost the words with which De

Morgan distinguishes between logic and probability in a passage

already cited (see Ch. VI. §3).

Perhaps it was a development of some such view as this that Leibnitz

looked forward to. "J'ai dit plus d'une fois qu'il faudrait une

nouvelle espèce de Logique, qui traiteroit des degrés de

Probabilité, puisqu'Aristote dans ses Topiques n'a rien moins fait

que cela" (\_Nouveaux essais\_, Lib. IV. ch. XVI). It is possible,

indeed, that he had in his mind more what we now understand by the

mathematical theory of Probability, but in the infancy of a science

it is of course hard to say whether any particular subject is

definitely contemplated or not. Leibnitz (as Todhunter has shown in

his history) took the greatest interest in such chance problems as

had yet been discussed.

20. By \_loci\_ were understood certain general classes of

premises. They stood, in fact, to the major premise in somewhat the

same relation that the Category or Predicament did to the

term. Crackanthorpe says of them, "sed duci a \_loco probabiliter

arguendi\_, hoc vere proprium est Argumentationis probabilis; et in

hoc a Demonstratione differt, quia Demonstrator utitur solummodo

quatuor Locis eisque necessariis.... Præter hos autem, ex quibus

quoque probabiliter arguere licet, sunt multo plures Loci arguendi

probabiliter; ut a Genere, a Specie, ab Adjuncto, ab Oppositis, et

similia" (\_Logica\_, Lib. V., ch. II.).

21. Stephen's \_General View of the Criminal Law of England\_, p. 241.

22. \_Rationale of Judicial Evidence\_; Bk. I. ch. VI.

23. Though this is claimed by some Kantian logicians;--Nie darf an

einem angeblichen Verbrecher die gesetzliche Strafe vollzogen

werden, bevor er nicht selbst das Verbrechen eingestanden. Denn wenn

auch alle Zeugnisse und die übrigen Anzeigen wider ihn wären, so

bleibt doch das Gegentheil immer möglich" (Krug, \_Denklehre\_, §131).

24. As Mr C. J. Monro puts it: "Suppose that a man is suspected of

murdering his daughter. Evidence which would not convict him before

an ordinary jury might make a grand jury find a true bill; evidence

which would not do this might make a coroner's jury bring in a

verdict against him; evidence which would not do this would very

often prevent a Chancery judge from appointing the man guardian to a

ward of the court; evidence which would not affect the judge's mind

might make a father think twice on his death-bed before he appointed

the man guardian to \_his\_ daughter."

25. The portions of this work which treat of the nature of proof in

general, and of judicial proof in particular, are well worth reading

by every logical student. It appears to me, however, that the author

goes much too far in the direction of regarding proof as subjective,

that is as what \_does\_ satisfy people, rather than as what \_should\_

satisfy them. He compares the legislative standard of certainty with

that of value; this latter is declared to be a certain weight of

gold, irrespective of the rarity or commonness of that metal. So

with certainty; if people grow more credulous the intrinsic value of

the standard will vary.

26. The question will be more fully discussed in a future chapter, but

a few words may be inserted here by way of indication. Reduce the

case to the simplest possible elements by supposing only two judges

or courts, of the same average correctness of decision. Let this be

indicated by x. Then the chance of their agreeing is

x^{2} + (1 - x)^{2}, for they agree if both are right or both wrong.

If the statistical frequency of this agreement is known, that is, the

frequency with which the first judgment is confirmed by the second,

we have the means of determining x.

27. Taylor on Evidence: the latter part of the extract does not seem

very clear.

CHAPTER XIV.

\_FALLACIES.\_

1. In works on Logic a chapter is generally devoted

to the discussion of Fallacies, that is, to the description

and classification of the different ways in which the rules

of Logic may be transgressed. The analogy of Probability

to Logic is sufficiently close to make it advisable to adopt

the same plan here. In describing his own opinions an

author is, of course, perpetually obliged to describe and

criticise those of others which he considers erroneous. But

some of the most widely spread errors find no supporters

worth mentioning, and exist only in vague popular misapprehension.

It will be found the best arrangement, therefore,

at the risk of occasional repetition, to collect a few of

the errors that occur most frequently, and as far as possible

to trace them to their sources; but it will hardly be worth

the trouble to attempt any regular system of arrangement

and classification. We shall mainly confine ourselves, in

accordance with the special province of this work, to problems

which involve questions of logical interest, or to those

which refer to the application of Probability to moral and

social science. We shall avoid the discussion of isolated

problems in games of chance and skill except when some

error of principle seems to be involved in them.

2. (I.) One of the most fertile sources of error and

confusion upon the subject has been already several times

alluded to, and in part discussed in a previous chapter.

This consists in choosing the class to which to refer an

event, and therefore judging of the rarity of the event and

the consequent improbability of foretelling it, \_after it has

happened\_, and then transferring the impressions we experience

to a supposed contemplation of the event beforehand.

The process in itself is perfectly legitimate (however unnecessary

it may be), since time does not in strictness enter

at all into questions of Probability. No error therefore

need arise in this way, if we were careful as to the class

which we thus selected; but such carefulness is often neglected.

An illustration may afford help here. A man once

pointed to a small target chalked upon a door, the target

having a bullet hole through the centre of it, and surprised

some spectators by declaring that he had fired that shot

from an old fowling-piece at a distance of a hundred yards,

His statement was true enough, but he suppressed a rather

important fact. The shot had really been aimed in a general

way at the barn-door, and had hit it; the target was afterwards

chalked round the spot where the bullet struck. A

deception analogous to this is, I think, often practised unconsciously

in other matters. We judge of events on a similar

principle, feeling and expressing surprise in an equally unreasonable

way, and deciding as to their occurrence on

grounds which are really merely a subsequent adjunct of our

own. Butler's remarks about 'the story of Cæsar,' discussed

already in the twelfth chapter, are of this character. He

selects a series of events from history, and then imagines a

person guessing them correctly who at the time had not the

history before him. As I have already pointed out, it is one

thing to be unlikely to guess an event rightly without

specific evidence; it is another and very different thing to

appreciate the truth of a story which is founded partly or

entirely upon evidence. But it is a great mistake to transfer

to one of these ways of viewing the matter the mental impressions

which properly belong to the other. It is like

drawing the target afterwards, and then being surprised to

find that the shot lies in the centre of it.

3. One aspect of this fallacy has been already discussed,

but it will serve to clear up difficulties which are

often felt upon the subject if we reexamine the question

under a somewhat more general form.

In the class of examples under discussion we are generally

presented with an individual which is not indeed definitely

referred to a class, but in regard to which we have no great

difficulty in choosing the appropriate class. Now suppose

we were contemplating such an event as the throwing of

sixes with a pair of dice four times running. Such a throw

would be termed a very unlikely event, as the odds against

its happening would be 36 × 36 × 36 × 36 - 1 to 1 or 1679615

to 1. The meaning of these phrases, as has been abundantly

pointed out, is simply that the event in question occurs very

rarely; that, stated with numerical accuracy, it occurs once in

1679616 times.

4. But now let us make the assumption that the

throw has actually occurred; let us put ourselves into the

position of contemplating sixes four times running when it is

known or reported that this throw has happened. The same

phrase, namely that the event is a very unlikely one, will

often be used in relation to it, but we shall find that this

phrase may be employed to indicate, on one occasion or

another, extremely different meanings.

(1) There is, firstly, the most correct meaning. The

event, it is true, has happened, and we know what it is, and

therefore, we have not really any occasion to resort to the

rules of Probability; but we can nevertheless conceive ourselves

as being in the position of a person who does not

know, and who has only Probability to appeal to. By calling

the chances 1679615 to 1 against the throw we then mean

to imply the fact, that inasmuch as such a throw occurs only

once in 1679616 times, our guess, were we to guess, would

be correct only once in the same number of times; provided,

that is, that it is a fair guess, based simply on these statistical

grounds.

5. (2) But there is a second and very different conception

sometimes introduced, especially when the event in

question is supposed to be known, not as above by the evidence

of our experience, but by the report of a witness. We

may then mean by the 'chances against the event' (as was

pointed out in Chapter XII.) not the proportional number of

times we should be right in guessing the event, but the

proportional number of times the witness will be right in

reporting it. The bases of our inference are here shifted

on to new ground. In the former case the statistics were

the throws and their respective frequency, now they are the

witnesses' statements and their respective truthfulness.

6. (3) But there is yet another meaning sometimes

intended to be conveyed when persons talk of the chances

against such an event as the throw in question. They may

mean--not, Here is an event, how often should I have

guessed it?--nor, Here is a report, how often will it be

correct?--but something different from either, namely, Here

is an event, how often will it be found to be produced by

some one particular kind of cause?

When, for example, a man hears of dice giving the same

throw several times running, and speaks of this as very

extraordinary, we shall often find that he is not merely

thinking of the improbability of his guess being right, or of

the report being true, but, that along with this, he is introducing

the question of the throw having been produced by

fair dice. There is, of course, no reason whatever why such

a question as this should not also be referred to Probability,

provided always that we could find the appropriate statistics

by which to judge. These statistics would be composed, not

of throws of the particular dice, nor of reports of the particular

witness, but of the occasions on which such a throw as

the one in question respectively had, and had not, been

produced fairly. The objection to entering upon this view

of the question would be that no such statistics are obtainable,

and that if they were, we should prefer to form our

opinion (on principles to be described in Chapter XVI.) from

the special circumstances of the case rather than from an

appeal to the average.

7. The reader will easily be able to supply examples

in illustration of the distinctions just given; we will briefly

examine but one. I hide a banknote in a certain book in a

large library, and leave the room. A person tells me that,

after I went out, a stranger came in, walked straight up to

that particular book, and took it away with him. Many

people on hearing this account would reply, How extremely

improbable! On analysing the phrase, I think we shall

find that certainly two, and possibly all three, of the above

meanings might be involved in this exclamation. (1) What

may be meant is this,--Assuming that the report is true,

and the stranger innocent, a rare event has occurred. Many

books might have been thus taken without that particular

one being selected. I should not therefore have expected

the event, and when it has happened I am surprised. Now

a man has a perfect right to be surprised, but he has no

logical right (so long as we confine ourselves to this view) to

make his surprise a ground for disbelieving the event. To

do this is to fall into the fallacy described at the commencement

of this chapter. The fact of my not having been likely

to have guessed a thing beforehand is no reason in itself for

doubting it when I am informed of it. (2) Or I may stop

short of the events reported, and apply the rules of Probability

to the report itself. If so, what I mean is that such a

story as this now before me is of a kind very generally false,

and that I cannot therefore attach much credit to it now.

(3) Or I may accept the truth of the report, but doubt the

fact of the stranger having taken the book at random. If

so, what I mean is, that of men who take books in the way

described, only a small proportion will be found to have

taken them really at random; the majority will do so because

they had by some means ascertained, or come to suspect,

what there was inside the book.

Each of the above three meanings is a possible and a

legitimate meaning. The only requisite is that we should be

careful to ascertain which of them is present to the mind, so

as to select the appropriate statistics. The first makes in

itself the most legitimate use of Probability; the drawback

being that at the time in question the functions of Probability

are superseded by the event being otherwise known.

The second or third, therefore, is the more likely meaning to

be present to the mind, for in these cases Probability, if it

could be practically made use of, would, at the time in

question, be a means of drawing really important inferences.

The drawbacks are the difficulty of finding such statistics,

and the extreme disturbing influence upon these statistics of

the circumstances of the special case.

8. (II.) Closely connected with the tendency just

mentioned is that which prompts us to confound a true

chance selection with one which is more or less picked.

When we are dealing with familiar objects in a concrete

way, especially when the greater rarity corresponds to superiority

of quality, almost every one has learnt to recognize

the distinction. No one, for instance, on observing a fine

body of troops in a foreign town, but would be prompted to

ask whether they came from an average regiment or from

one that was picked. When however the distinction refers

to unfamiliar objects, and especially when only comparative

rarity seems to be involved, the fallacy may assume a rather

subtle and misleading form, and seems to deserve special

notice by the consideration of a few examples.

Sometimes the result is not so much an actual fallacy as

a slight misreckoning of the order of probability of the event

under consideration. For instance, in the Pyramid question,

we saw that it made some difference whether we considered

that π alone was to be taken into account or whether we

put this constant into a class with a small number of other

similar ones. In deciding, however, whether or not there is

anything remarkable in the actual falling short of the

representation of the number 7 in the evaluation of π

(v. p. 248) the whole question turns upon considerations of

this kind. The only enquiry raised is whether there is anything

remarkable in this departure from the mean, and the

answer depends upon whether we suppose that we are referring

to a predetermined digit, or to whatever digit of the

ten happens to be most above or below the average. Or,

take the case raised by Cournot (\_Exposition de la Théorie

des Chances\_, §§102, 114), that a certain deviation from the

mean in the case of Departmental returns of the proportion

between male and female births is significant and indicative

of a difference in kind, provided that we select at random a

single French Department; but that the same deviation may

be accidental if it is the maximum of the respective returns

for several Departments.[1] The answer may be given one

way or the other according as we bear this consideration in

mind.

9. We are peculiarly liable to be misled in this way

when we are endeavouring to determine the cause of some

phenomenon, by mere statistics, in entire ignorance as to the

direction in which the cause should be expected. In such

cases an ingenious person who chooses to look about over a

large field can never fail to hit upon an explanation which is

plausible in the sense that it fits in with the hitherto observed

facts. With a tithe of the trouble which Mr Piazzi

Smyth expended upon the measurement of the great pyramid,

I think I would undertake to find plausible intimations of

several of the important constants and standards which he

discovered there, in the dimensions of the desk at which I am

writing. The oddest instance of this sort of conclusion is

perhaps to be found in the researches of a writer who has

discovered[2] that there is a connection of a striking kind

between the respective successes of the Oxford and the

Cambridge boat in the annual race, and the greater and less

frequency of sun-spots.

Of course our usual practical resource in such cases is to

make appeal to our previous knowledge of the subject in

question, which enables us to reject as absurd a great number

of hypotheses which can nevertheless make a fair show when

they are allowed to rest upon a limited amount of adroitly

selected instances. But it must be remembered that if any

theory chooses to appeal to statistics, to statistics it must be

suffered to go for judgment. Even the boat race theory

could be established (if sound) on this ground alone. That

is, if it really could be shown that experience in the long run

confirmed the preponderance of successes on one side or the

other according to the relative frequency of the sun-spots,

we should have to accept the fact that the two classes of

events were not really independent. One of the two, whichever

it may be, must be suspected of causing or influencing

the other; or both must be caused or influenced by some

common circumstances.

10. (III.) The fallacy described at the commencement

of this chapter arose from determining to judge of an observed

or reported event by the rules of Probability, but

employing a wrong set of statistics in the process of judging.

Another fallacy, closely connected with this, arises from the

practice of taking some only of the characteristics of such an

event, and arbitrarily confining to these the appeal to Probability.

Suppose I toss up twelve pence and find that eleven

of them give heads. Many persons on witnessing such an occurrence

would experience a feeling which they would express by

the remark, How near that was to getting all heads! And if

any thing very important were staked on the throw they

would be much excited at the occurrence. But in what

sense were we near to twelve? There is a not uncommon

error, I apprehend, which consists in unconsciously regarding

the eleven heads as a thing which is already somehow

secured, so that one might as it were keep them, and then

take our chance for securing the remaining one. The eleven

are mentally set aside, looked upon as certain (for they have

already happened), and we then introduce the notion of

chance merely for the twelfth. But this twelfth, having

also happened, has no better claim to such a distinction than

any of the others. If we will introduce the notion of chance

in the case of the one that gave tail we must do the same in

the case of all the others as well. In other words, if the

tosser be dissatisfied at the appearance of the one tail, and

wish to cancel it and try his luck again, he must toss up the

whole lot of pence again fairly together. In this case, of

course, so far from his having a better prospect for the next

throw he may think himself in very good luck if he makes

again as good a throw as the one he rejected. What he is

doing is confounding this case with that in which the throws

are really \_successive\_. If eleven heads have been tossed up in

turn, we are of course within an even chance of getting a

twelfth; but the circumstances are quite different in the

instance proposed.

11. In the above example the error is transparent.

But in forming a judgment upon matters of greater complexity

than dice and pence, especially in the case of what are

called 'narrow escapes,' a mistake of an analogous kind is, I

apprehend, far from uncommon. A person, for example, who

has just experienced a narrow escape will often be filled

with surprise and anxiety amounting almost to terror. The

event being past, these feelings are, at the time, in strictness

inappropriate. If, as is quite possible, they are merely instinctive,

or the result of association, they do not fall within

the province of any kind of Logic. If, however, as seems

more likely, they partially arise from a supposed transference

of ourselves into that point of past time at which the event

was just about to happen, and the production by imagination

of the feelings we should then expect to experience,

this process partakes of the nature of an inference, and can

be right or wrong. In other words, the alarm may be proportionate

or disproportionate to the amount of danger that

might fairly have been reckoned upon in such a hypothetical

anticipation. If the supposed transfer were completely

carried out, there would be no fallacy; but it is often very

incompletely done, some of the component parts of the event

being supposed to be determined or 'arranged' (to use a

sporting phrase) in the form in which we now know that

they actually have happened, and only the remaining ones

being fairly contemplated as future chances.

A man, for example, is out with a friend, whose rifle goes

off by accident, and the bullet passes through his hat. He

trembles with anxiety at thinking what might have happened,

and perhaps remarks, 'How very near I was to being

killed!' Now we may safely assume that he means something

more than that a shot passed very close to him. He

has some vague idea that, as he would probably say, 'his

chance of being killed then was very great.' His surprise

and terror may be in great part physical and instinctive,

arising simply from the knowledge that the shot had passed

very near him. But his mental state may be analysed, and

we shall then most likely find, at bottom, a fallacy of the

kind described above. To speak or think of chance in connection

with the incident, is to refer the particular incident

to a class of incidents of a similar character, and then to consider

the comparative frequency with which the contemplated

result ensues. Now the series which we may suppose

to be most naturally selected in this case is one composed of

shooting excursions with his friend; up to this point the

proceedings are assumed to be designed, beyond it only,

in the subsequent event, was there accident. Once in a

thousand times perhaps on such occasions the gun will go

off accidentally; one in a thousand only of those discharges

will be directed near his friend's head. If we will make the

accident a matter of Probability, we ought by rights in this

way (to adopt the language of the first example), to 'toss up

again' fairly. But we do not do this; we seem to assume for

certain that the shot goes within an inch of our heads, detach

that from the notion of chance at all, and then begin to

introduce this notion again for possible deflections from that

saving inch.

12. (IV.) We will now notice a fallacy connected with

the subjects of betting and gambling. Many or most of the

popular misapprehensions on this subject imply such utter

ignorance and confusion as to the foundations of the science

that it would be needless to discuss them here. The following

however is of a far more plausible kind, and has been a

source of perplexity to persons of considerable acuteness.

The case, put into the simplest form, is as follows.[3]

Suppose that a person A is playing against B, B being

either another individual or a group of individuals, say a

gambling bank. They begin by tossing for a shilling, and

A maintains that he is in possession of a device which will

insure his winning. If he does win on the first occasion he

has clearly gained his point so far. If he loses, he stakes

next time two shillings instead of one. The result of course

is that if he wins on the second occasion he replaces his

former loss, and is left with one shilling profit as well. So

he goes on, doubling his stake after every loss, with the

obvious result that on the first occasion of success he makes

good all his previous losses, and is left with a shilling over.

But such an occasion must come sooner or later, by the

assumptions of chance on which the game is founded. Hence

it follows that he can insure, sooner or later, being left a

final winner. Moreover he may win to any amount; firstly

from the obvious consideration that he might make his

initial stake as large as he pleased, a hundred pounds, for

instance, instead of a shilling; and secondly, because what

he has done once he may do again. He may put his shilling

by, and have a second spell of play, long or short as the case

may be, with the same termination to it. Accordingly by

mere persistency he may accumulate any sum of money he

pleases, in apparent defiance of all that is meant by luck.

13. I have classed this opinion among fallacies, as the

present is the most convenient opportunity of discussing it,

though in strictness it should rather be termed a paradox,

since the conclusion is perfectly sound. The only fallacy

consists in regarding such a way of obtaining the result as

mysterious. On the contrary, there is nothing more easy

than to insure ultimate success under the given conditions.

The point is worth enquiry, from the principles it involves,

and because the answers commonly given do not quite meet

the difficulty. It is sometimes urged, for instance, that no

bank would or does allow the speculator to choose at will the

amount of his stake, but puts a limit to the amount for

which it will consent to play. This is quite true, but is of

course no answer to the hypothetical enquiry before us, which

assumes that such a state of things \_is\_ allowed. Again, it has

been urged that the possibility in question turns entirely

upon the fact that credit must be supposed to be given, for

otherwise the fortune of the player may not hold out until

his turn of luck arrives:--that, in fact, sooner or later, if he

goes on long enough, his fortune will not hold out long

enough, and all his gains will be swept away. It is quite

true that credit is a \_condition\_ of success, but it is in no sense

the cause. We may suppose both parties to agree at the

outset that there shall be no payments until the game be

ended, A having the right to decide when it shall be considered

to be ended. It still remains true that whereas in

ordinary gambling, i.e. with fixed or haphazard stakes, A could

not ensure winning eventually to any extent, he can

do so if he adopt such a scheme as the one in question. And

this is the state of things which seems to call for explanation.

14. What causes perplexity here is the supposed fact

that in some mysterious way certainty has been conjured out

of uncertainty; that in a game where the detailed events are

utterly inscrutable, and where the average, by supposition,

shows no preference for either side, one party is nevertheless

succeeding somehow in steadily drawing the luck his own

way. It looks as if it were a parallel case with that of a

man who should succeed by some device in permanently

securing more than half of the tosses with a penny which

was nevertheless to be regarded as a perfectly fair one.

This is quite a mistake. The real fact is that A does

not expose his gains to chance at all; all that he so exposes

is the number of times he has to wait until he gains. Put

such a case as this. I offer to give a man any sum of money

he chooses to mention provided he will at once give it back

again to me with one pound more. It does not need much

acuteness to see that it is a matter of indifference to me

whether he chooses to mention one pound, or ten, or a

hundred. Now suppose that instead of leaving it to his

choice which of these sums is to be selected each time, the

two parties agree to leave it to chance. Let them, for

instance, draw a number out of a bag each time, and let that

be the sum which A gives to B under the prescribed conditions.

The case is not altered. A still gains his pound each

time, for the introduction of the element of chance has not

in any way touched this. All that it does is to make this

pound the result of an uncertain subtraction, sometimes 10

\_minus\_ 9, sometimes 50 \_minus\_ 49, and so on. It is these

numbers only, not their difference, which he submits to luck,

and this is of no consequence whatever.

To suggest to any individual or company that they

should consent to go on playing upon such terms as these

would be too barefaced a proposal. And yet the case in

question is identical in principle, and almost identical in

form, with this. To offer to give a man any sum he likes to

name provided he gives you back again that same sum \_plus\_

one, and to offer him any number of terms he pleases of the

series 1, 2, 4, 8, 16, &c., provided you have the next term of

the set, are equivalent. The only difference is that in the

latter case the result is attained with somewhat more of

arithmetical parade. Similarly equivalent are the processes

in case we prefer to leave it to chance, instead of to choice,

to decide what sum or what number of terms shall be fixed

upon. This latter is what is really done in the case in

question. A man who consents to go on doubling his stake

every time he wins, is leaving nothing else to chance than

the determination of the particular number of terms of such

a geometrical series which shall be allowed to pass before he

stops.

15. It may be added that there is no special virtue in

the particular series in question, viz. that in accordance with

which the stake is doubled each time. All that is needed is

that the last term of the series should more than balance all

the preceding ones. Any other series which increased faster

than this geometrical one, would answer the purpose as well

or better. Nor is it necessary, again, that the game should

be an even or 'fair' one. Chance, be it remembered, affects

nothing here but the number of terms to which the series

attains on each occasion, its final result being always arithmetically

fixed. When a penny is tossed up it is only on

one of every two occasions that the series runs to more than

two terms, and so his fixed gains come in pretty regularly.

But unless he was playing for a limited time only, it would

not affect him if the series ran to two hundred terms; it

would merely take him somewhat longer to win his stakes.

A man might safely, for instance, continue to lay an \_even\_

bet that he would get the single prize in a lottery of a thousand

tickets, provided he thus doubled, or more than doubled,

his stake each time, and unlimited credit was given.

16. So regarded, the problem is simple enough, but

there are two points in it to which attention may conveniently

be directed.

In the first place, it serves very pointedly to remind us of

the distinction between a series of events (in this case the

tosses of the penny) which really are subjects of chance, and

our conduct founded upon these events, which may or may

not be so subject.[4] It is quite possible that this latter may

be so contrived as to be in many respects a matter of absolute

certainty,--a consideration, I presume, familiar enough

to professional betting men. Why is the ordinary way of

betting on the throws of a penny fair to both parties? Because

a 'fair' series is 'fairly' treated. The heads and tails

occur at random, but on an average equally often, and the

stakes are either fixed or also arranged at random. If a man

backs heads every time for the same amount, he will of

course in the long run neither win nor lose. Neither will he

if he varies the stake every time, provided he does not vary

it in such a way as to make its amount dependent on the

fact of his having won or lost the time before. But he

may, if he pleases, and the other party consents, so arrange

his stakes (as in the case in question) that Chance, if one

might so express it, does not get a fair chance. Here the

human elements of choice and design have been so brought

to bear upon a series of events which, regarded by themselves,

exhibit nothing but the physical characteristics of

chance, that the latter elements disappear, and we get a

result which is arithmetically certain. Other analogous

instances might be suggested, but the one before us has the

merit of most ingeniously disguising the actual process.

17. The meaning of the remark just made will be

better seen by a comparison with the following case. It has

been attempted[5] to explain the preponderance of male births

over female by assuming that the chances of the two are

equal, but that the general desire to have a male heir tends

to induce many unions to persist until the occurrence of this

event, and no longer. It is supposed that in this way there

would be a slight preponderance of families which consisted

of one son only, or of two sons and one daughter, and so forth.

This is quite fallacious (as had been noticed by Laplace,

in his \_Essai\_); and there could not be a better instance

chosen than this to show just what we can do and what we

cannot do in the way of altering the luck in a real chance-succession

of events. To suppose that the number of actual

births could be influenced in the way in question is exactly

the same thing as to suppose that a number of gamblers

could increase the ratio of heads to tails, to something over

one-half, by each handing the coin to his neighbour as soon

as he had thrown a head: that they have only to leave off as

soon as head has appeared; an absurdity which we need not

pause to explain at this stage. The essential point about

the 'Martingale' is that, whereas the occurrence of the

events on which the stakes are laid is unaffected, the stakes

themselves can be so adjusted as to make the luck swing one

way.

18. In the second place, this example brings before us

what has had to be so often mentioned already, namely, that

the series of Probability are in strictness supposed to be

interminable. If therefore we allow either party to call

upon us to stop, especially at a point which just happens to

suit him, we may get results decidedly opposed to the

integrity of the theory. In the case before us it is a necessary

stipulation for A that he may be allowed to leave off

when he wishes, that is at one of the points at which the

throw is in his favour. Without this stipulation he may be

left a loser to any amount.

Introduce the supposition that one party may arbitrarily

call for a stoppage when it suits him and refuse to permit it

sooner, and almost any system of what would be otherwise fair

play may be converted into a very one-sided arrangement.

Indeed, in the case in question, A need not adopt this device

of doubling the stakes every time he loses. He may play

with a fixed stake, and nevertheless insure that \_one\_ party

shall win any assigned sum, assuming that the game is even

and that he is permitted to play on credit.

19. (V.) A common mistake is to assume that a very

unlikely thing will not happen at all. It is a mistake which,

when thus stated in words, is too obvious to be committed,

for the meaning of an unlikely thing is one that happens at

rare intervals; if it were not assumed that the event would

happen sometimes it would not be called unlikely, but impossible.

This is an error which could scarcely occur except

in vague popular misapprehension, and is so abundantly refuted

in works on Probability, that it need only be touched

upon briefly here. It follows of course, from our definition

of Probability, that to speak of a very rare combination of

events as one that is 'sure never to happen,' is to use language

incorrectly. Such a phrase may pass current as a

loose popular exaggeration, but in strictness it involves a

contradiction. The truth about such rare events cannot be

better described than in the following quotation from De Morgan:[6]--

"It is said that no person ever \_does\_ arrive at such extremely

improbable cases as the one just cited [drawing the

same ball five times running out of a bag containing twenty

balls]. That a given individual should never throw an ace

twelve times running on a single die, is by far the most

likely; indeed, so remote are the chances of such an event

in any twelve trials (more than 2,000,000,000 to 1 against it)

that it is unlikely the experience of any given country, in

any given century, should furnish it. But let us stop for a

moment, and ask ourselves to what this argument applies.

A person who rarely touches dice will hardly believe that

doublets sometimes occur three times running; one who handles

them frequently knows that such is sometimes the fact.

Every very practised user of those implements has seen still

rarer sequences. Now suppose that a society of persons had

thrown the dice so often as to secure a run of six aces observed

and recorded, the preceding argument would still be

used against twelve. And if another society had practised

long enough to see twelve aces following each other, they

might still employ the same method of doubting as to a run

of twenty-four; and so on, \_ad infinitum\_. The power of imagining

cases which contain long combinations so much exceeds

that of exhibiting and arranging them, that it is easy

to assign a telegraph which should make a separate signal

for every grain of sand in a globe as large as the visible universe,

upon the hypothesis of the most space-penetrating

astronomer. The fallacy of the preceding objection lies in

supposing events in number beyond our experience, composed

entirely of sequences such as fall within our experience. It

makes the past necessarily contain the whole, as to the quality

of its components; and judges by samples. Now the

least cautious buyer of grain requires to examine a handful

before he judges of a bushel, and a bushel before he judges

of a load. But relatively to such enormous numbers of combinations

as are frequently proposed, our experience does not

deserve the title of a handful as compared with a bushel, or

even of a single grain."

20. The origin of this inveterate mistake is not difficult

to be accounted for. It arises, no doubt, from the exigencies

of our practical life. No man can bear in mind

every contingency to which he may be exposed. If therefore

we are ever to do anything at all in the world, a large number

of the rarer contingencies must be left entirely out of

account. And the necessity of this oblivion is strengthened

by the shortness of our life. Mathematically speaking, it

would be said to be certain that any one who lives long

enough will be bitten by a mad dog, for the event is not an

impossible, but only an improbable one, and must therefore

come to pass in time. But this and an indefinite number

of other disagreeable contingencies have on most occasions

to be entirely ignored in practice, and thence they come

almost necessarily to drop equally out of our thought and

expectation. And when the event is one in itself of no importance,

like a rare throw of the dice, a great effort of

imagination may be required, on the part of persons not accustomed

to abstract mathematical calculation, to enable

them to realize the throw as being even possible.

Attempts have sometimes been made to estimate what

extremity of unlikelihood ought to be considered as equivalent

to this practical zero point of belief. In so far as

such attempts are carried out by logicians, or by those who

are unwilling to resort to mathematical valuation of chances,

they must be regarded as merely a special form of the modal

difficulties discussed in the last chapter, and need not therefore

be reconsidered here; but a word or two may be added

concerning the views of some who have looked at the matter

from the mathematician's point of view.

The principal of these is perhaps Buffon. He has arrived

at the estimate (\_Arithmétique Morale\_ §VIII.) that this

practical zero is equivalent to a chance of 1/10,000. The

grounds for selecting this fraction are found in the fact that,

according to the tables of mortality accessible to him, it

represents the chance of a man of 56 dying in the course of

the next day. But since no man under common circumstances

takes the chance into the slightest consideration, it

follows that it is practically estimated as having no value.

It is obvious that this result is almost entirely arbitrary,

and in fact his reasons cannot be regarded as anything more

than a slender justification from experience for adopting a

conveniently simple fraction; a justification however which

would apparently have been equally available in the case of

any other fractions lying within wide limits of the one

selected.[7]

21. There is one particular form of this error, which,

from the importance occasionally attached to it, deserves

perhaps more special examination. As stated above, there

can be no doubt that, however unlikely an event may be, if

we (loosely speaking) vary the circumstances sufficiently, or

if, in other words, we keep on trying long enough, we shall

meet with such an event at last. If we toss up a pair of

dice a few times we shall get doublets; if we try longer with

three we shall get triplets, and so on. However unusual the

event may be, even were it sixes a thousand times running,

it will come some time or other if we have only patience and

vitality enough. Now apply this result to the letters of the

alphabet. Suppose that one letter at a time is drawn from

a bag which contains them all, and is then replaced. If the

letters were written down one after another as they occurred,

it would commonly be expected that they would be found to

make mere nonsense, and would never arrange themselves

into the words of any language known to men. No more

they would in general, but it is a commonly accepted result

of the theory, and one which we may assume the reader to

be ready to admit without further discussion, that, if the

process were continued long enough, words making sense

would appear; nay more, that any book we chose to mention,--Milton's

\_Paradise Lost\_ or the plays of Shakespeare,

for example,--would be produced in this way at last. It

would take more days than we have space in this volume to

represent in figures, to make tolerably certain of obtaining

the former of these works by thus drawing letters out of a

bag, but the desired result would be obtained at length.[8]

Now many people have not unnaturally thought it derogatory

to genius to suggest that its productions could

have also been obtained by chance, whilst others have gone

on to argue, If this be the case, might not the world itself in

this manner have been produced by chance?

22. We will begin with the comparatively simple, determinate,

and intelligible problem of the possible production

of the works of a great human genius by chance. With

regard to this possibility, it may be a consolation to some

timid minds to be reminded that the power of producing the

works of a Shakespeare, \_in time\_, is not confined to consummate

genius and to mere chance. There is a third alternative,

viz. that of purely mechanical procedure. Any one,

down almost to an idiot, might do it, if he took sufficient

time about the task. For suppose that the required number

of letters were procured and arranged, not by chance, but

designedly, and according to rules suggested by the theory

of permutations: the letters of the alphabet and the number

of them to be employed being finite, every order in which

they could occur would come in its due turn, and therefore

every thing which can be expressed in language would be

arrived at some time or other.

There is really nothing that need shock any one in such

a result. Its possibility arises from the following cause.

The number of letters, and therefore of words, at our disposal

is limited; whatever therefore we may desire to express

in language necessarily becomes subject to corresponding

limitation. The possible variations of thought are literally

infinite, so are those of spoken language (by intonation of

the voice, &c.); but when we come to words there is a limitation,

the nature of which is distinctly conceivable by the

mind, though the restriction is one that in practice will

never be appreciable, owing to the fact that the number of

combinations which may be produced is so enormous as to

surpass all power of the imagination to realize.[9] The answer

therefore is plain, and it is one that will apply to many other

cases as well, that to put a finite limit upon the number of

ways in which a thing can be done, is to determine that any

one who is able and willing to try long enough shall succeed

in doing it. If a great genius condescends to perform it

under these circumstances, he must submit to the possibility

of having his claims rivalled or disputed by the chance-man

and idiot. If Shakespeare were limited to the use of eight

or nine assigned words, the time within which the latter

agents might claim equality with him would not be very

great. As it is, having had the range of the English language

at his disposal, his reputation is not in danger of being

assailed by any such methods.

23. The case of the possible production of the world

by chance leads us into an altogether different region of discussion.

We are not here dealing with figures the nature

and use of which are within the fair powers of the understanding,

however the imagination may break down in attempting

to realize the smallest fraction of their full significance.

The understanding itself is wandering out of its

proper province, for the conditions of the problem cannot be

assigned. When we draw letters out of a bag we know very

well what we are doing; but what is really meant by producing

a world by chance? By analogy of the former case,

we may assume that some kind of agent is presupposed;--perhaps

therefore the following supposition is less absurd

than any other. Imagine some being, not a Creator but a

sort of Demiurgus, who has had a quantity of materials put

into his hands, and he assigns them their collocations and

their laws of action, blindly and at haphazard: what are the

odds that such a world as we actually experience should have

been brought about in this way?

If it were worth while seriously to set about answering

such a question, and if some one would furnish us with the

number of the letters of such an alphabet, and the length of

the work to be written with them, we could proceed to indicate

the result. But so much as this may surely be affirmed

about it;--that, far from merely finding the length of this

small volume insufficient for containing the figures in which

the adverse odds would be given, all the paper which the

world has hitherto produced would be used up before we had

got far on our way in writing them down.

24. The most seductive form in which the difficulty

about the occurrence of very rare events generally presents

itself is probably this. 'You admit (some persons will be

disposed to say) that such an event may sometimes happen;

nay, that it does sometimes happen in the infinite course of

time. How then am I to know that \_this\_ occasion is not one

of these possible occurrences?' To this, one answer only can

be given,--the same which must always be given where

statistics and probability are concerned,--'The present \_may\_

be such an occasion, but it is inconceivably unlikely that it

should be one. Amongst countless billions of times in which

you, and such as you, urge this, one person only will be

justified; and it is not likely that you are that one, or that

this is that occasion.'

25. There is another form of this practical inability to

distinguish between one high number and another in the

estimation of chances, which deserves passing notice from its

importance in arguments about heredity. People will often

urge an objection to the doctrine that qualities, mental and

bodily, are transmitted from the parents to the offspring, on

the ground that there are a multitude of instances to the

contrary, in fact a great majority of such instances. To

raise this objection implies an utter want of appreciation of

the very great odds which possibly may exist, and which the

argument in support of heredity implies \_do\_ exist against any

given person being distinguished for intellectual or other

eminence. This is doubtless partly a matter of definition,

depending upon the degree of rarity which we consider to be

implied by eminence; but taking any reasonable sense of the

term, we shall readily see that a very great proportion of

failures may still leave an enormous preponderance of evidence

in favour of the heredity doctrine. Take, for instance,

that degree of eminence which is implied by being one of

four thousand. This is a considerable distinction, though,

since there are about two thousand such persons to be found

amongst the total adult male population of Great Britain, it

is far from implying any conspicuous genius. Now suppose

that in examining the cases of a large number of the children

of such persons, we had found that 199 out of 200 of

them failed to reach the same distinction. Many persons

would conclude that this was pretty conclusive evidence

against any hereditary transmission. To be able to adduce

only one favourable, as against 199 hostile instances, would

to them represent the entire break-down of any such theory.

The error, of course, is obvious enough, and one which, with

the figures thus before him, hardly any one could fail to

avoid. But if one may judge from common conversation

and other such sources of information, it is found in practice

exceedingly difficult adequately to retain the conviction that

even though only one in 200 instances were favourable,

this would represent odds of about 20 to 1 in favour of the

theory. If hereditary transmission did not prevail, only one

in 4000 sons would thus rival their fathers; but we find

actually, let us say (we are of course taking imaginary proportions

here), that one in 200 does. Hence, if the statistics

are large enough to be satisfactory, there has been some

influence at work which has improved the chances of mere

coincidence in the ratio of 20 to 1. We are in fact so little

able to realise the meaning of very large numbers,--that is,

to retain the \_ratios\_ in the mind, where large numbers are

concerned,--that unless we repeatedly check ourselves by

arithmetical considerations we are too apt to treat and estimate

all beyond certain limits as equally vast and vague.

26. (VI.) In discussing the nature of the connexion

between Probability and Induction, we examined the claims

of a rule commonly given for inferring the probability that

an event which had been repeatedly observed would recur

again. I endeavoured to show that all attempts to obtain

and prove such a rule were necessarily futile; if these reasons

were conclusive the employment of such a rule must of

course be regarded as fallacious. A few examples may conveniently

be added here, tending to show how instead of

there being merely a single rule of succession we might better

divide the possible forms into three classes.

(1) In some cases when a thing has been observed to

happen several times it becomes in consequence \_more\_ likely

that the thing should happen again. This agrees with the

ordinary form of the rule, and is probably the case of most

frequent occurrence. The necessary vagueness of expression

when we talk of the 'happening of a thing' makes it quite

impossible to tolerate the rule in this general form, but if we

specialize it a little we shall find it assume a more familiar

shape. If, for example, we have observed two or more properties

to be frequently associated together in a succession of

individuals, we shall conclude with some force that they will

be found to be so connected in future. The strength of our

conviction however will depend not merely on the number

of observed coincidences, but on far more complicated considerations;

for a discussion of which the reader must be

referred to regular treatises on Inductive evidence. Or again,

if we have observed one of two events succeed the other

several times, the occurrence of the former will excite in

most cases some degree of expectation of the latter. As

before, however, the degree of our expectation is not to be

assigned by any simple formula; it will depend in part upon

the supposed intimacy with which the events are connected.

To attempt to lay down definite rules upon the subject

would lead to a discussion upon laws of causation, and the

circumstances under which their existence may be inferred,

and therefore any further consideration of the matter must

be abandoned here.

27. (2) Or, secondly, the past recurrence may in itself

give no valid grounds for inference about the future;

this is the case which most properly belongs to Probability.[10]

That it does so belong will be easily seen if we bear in mind

the fundamental conception of the science. We are there

introduced to a series,--for purposes of inference an indefinitely

extended series,--of terms, about the details of which,

information, except on certain points, is not given; our knowledge

being confined to the statistical fact, that, say, one in

ten of them has some attribute which we will call X. Suppose

now that five of these terms in succession have been X,

what hint does this give about the sixth being also an X?

Clearly none at all; this past fact tells us nothing; the formula

for our inference is still precisely what it was before,

that one in ten being X it is one to nine that the next term

is X. And however many terms in succession had been of

one kind, precisely the same formula would still be given.

28. The way in which events will justify the answer

given by this formula is often misunderstood. For the

benefit therefore of those unacquainted with some of the

conceptions familiar to mathematicians, a few words of explanation

may be added. Suppose then that we have had

X twelve times in succession. This is clearly an anomalous

state of things. To suppose anything like this continuing

to occur would be obviously in opposition to the statistics,

which assert that in the long run only one in ten is X.

But how is this anomaly got over? In other words, how

do we obviate the conclusion that X's must occur more

frequently than once in ten times, after such a long succession

of them as we have now had? Many people seem to

believe that there must be a diminution of X's afterwards

to counterbalance their past preponderance. This however

would be quite a mistake; the proportion in which they

occur in future must remain the same throughout; it cannot

be altered if we are to adhere to our statistical formula.

The fact is that the rectification of the exceptional disturbance

in the proportion will be brought about simply by the

continual influx of fresh terms in the series. These will in

the long run neutralize the disturbance, not by any special

adaptation, as it were, for the purpose, but by the mere

weight of their overwhelming numbers. At every stage

therefore, in the succession, whatever might have been the

number and nature of the preceding terms, it will still be

true to say that one in ten of the terms will be an X.

If we had to do only with a finite number of terms,

however large that number might be, such a disturbance as

we have spoken of would, it is true, need a special alteration

in the subsequent proportions to neutralize its effects. But

when we have to do with an infinite number of terms, this

is not the case; the 'limit' of the series, which is what we

then have to deal with, is unaffected by these temporary

disturbances. In the continued progress of the series we

shall find, as a matter of fact, more and more of such disturbances,

and these of a more and more exceptional character.

But whatever the point we may occupy at any time, if we

look forward or backward into the indefinite extension of the

series, we shall still see that the ultimate limit to the proportion

in which its terms are arranged remains the same;

and it is with this limit, as above mentioned, that we are

concerned in the strict rules of Probability.

The most familiar example, perhaps, of this kind is that

of tossing up a penny. Suppose we have had four heads in

succession; people[11] have tolerably realized by now that 'head

the fifth time' is still an even chance, as 'head' was each

time before, and will be ever after. The preceding paragraph

explains how it is that these occasional disturbances

in the average become neutralized in the long run.

29. (3) There are other cases which, though rare,

are by no means unknown, in which such an inference as

that obtained from the Rule of Succession would be the direct

reverse of the truth. The oftener a thing happens, it

may be, the more unlikely it is to happen again. This is the

case whenever we are drawing things from a limited source

(as balls from a bag without replacing them), or whenever

the act of repetition itself tends to prevent the succession

(as in giving false alarms).

I am quite ready to admit that we believe the results described

in the last two classes on the strength of some such

general Inductive rule, or rather principle, as that involved

in the first. But it would be a great error to confound this

with an admission of the validity of the rule in each special

instance. We are speaking about the application of the rule

to individual cases, or classes of cases; this is quite a distinct

thing, as was pointed out in a previous chapter, from

giving the grounds on which we rest the rule itself. If a

man were to lay it down as a universal rule, that the testimony

of all persons was to be believed, and we adduced an

instance of a man having lied, it would not be considered

that he saved his rule by showing that we believed that it

was a lie on the word of other persons. But it is perfectly

consistent to give as a merely general, but not universal,

rule, that the testimony of men is credible; then to separate

off a second class of men whose word is not to be trusted,

and finally, if any one wants to know our ground for the

second rule, to rest it upon the first. If we were speaking

of \_necessary\_ laws, such a conflict as this would be as

hopeless as the old 'Cretan' puzzle in logic; but in instances

of Inductive and Analogical extension it is perfectly

harmless.

30. A familiar example will serve to bring out the

three different possible conclusions mentioned above. We

have observed it rain on ten successive days. A and B conclude

respectively for and against rain on the eleventh day;

C maintains that the past rain affords no data whatever for

an opinion. Which is right? We really cannot determine

\_à priori\_. An appeal must be made to direct observation, or

means must be found for deciding on independent grounds

to which class we are to refer the instance. If, for example,

it were known that every country produces its own rain, we

should choose the third rule, for it would be a case of drawing

from a limited supply. If again we had reasons to believe

that the rain for our country might be produced anywhere

on the globe, we should probably conclude that the past

rainfall threw no light whatever on the prospect of a continuance

of wet weather, and therefore take the second.

Or if, finally, we knew that rain came in long spells or seasons,

as in the tropics, then the occurrence of ten wet days

in succession would make us believe that we had entered on

one of these seasons, and that therefore the next day would

probably resemble the preceding ten.

Since then all these forms of such an Inductive rule are

possible, and we have often no \_à priori\_ grounds for preferring

one to another, it would seem to be unreasonable to attempt

to establish any universal formula of anticipation. All that

we can do is to ascertain what are the circumstances under

which one or other of these rules is, as a matter of fact,

found to be applicable, and to make use of it under those

circumstances.

31. (VII.) In the cases discussed in (V.) the almost

infinitely small chances with which we were concerned were

rightly neglected from all practical consideration, however

proper it might be, on speculative grounds, to keep our minds

open to their actual existence. But it has often occurred to

me that there is a common error in neglecting to take them into

account when they may, though individually small, make up

for their minuteness by their number. As the mathematician

would express it, they may occasionally be capable of being

\_integrated\_ into a finite or even considerable magnitude.

For instance, we may be confronted with a difficulty out

of which there appears to be only one appreciably possible

mode of escape. The attempt is made to force us into

accepting this, however great the odds apparently are against

it, on the ground that improbable as it may seem, it is at

any rate vastly more probable than any of the others. I

can quite admit that, on practical grounds, we may often find

it reasonable to adopt this course; for we can only \_act\_ on

one supposition, and we naturally and rightly choose, out

of a quantity of improbabilities, the least improbable. But

when we are not forced to act, no such decisive preference is

demanded of us. It is then perfectly reasonable to refuse

assent to the proposed explanation; even to say distinctly

that we do not believe it, and at the same time to decline,

at present, to accept any other explanation. We remain, in

fact, in a state of suspense of judgment, a state perfectly

right and reasonable so long as no action demanding a specific

choice is forced upon us. One alternative may be

decidedly probable as compared with any other individually,

but decidedly improbable as compared with all others collectively.

This in itself is intelligible enough; what people

often fail to see is that there is no necessary contradiction

between saying and feeling this, and yet being prepared

vigorously to act, when action is forced upon us, as though

this alternative were really the true one.

32. To take a specific instance, this way of regarding

the matter has often occurred to me in disputes upon 'Spiritualist'

manifestations. Assent is urged upon us because,

it is said, no other possible solution can be suggested. It

may be quite true that apparently overwhelming difficulties

may lie as against each separate alternative solution; but is

it always sufficiently realized how numerous such solutions

may be? No matter that each individually may be almost

incredible: they ought all to be massed together and thrown

into the scale against the proffered solution, when the only

question asked is, Are we to accept this solution? There is

no unfairness in such a course. We are perfectly ready to

adopt the same plan against any other individual alternative,

whenever any person takes to claiming this as \_the\_

solution of the difficulty. We are looking at the matter

from a purely logical point of view, and are quite willing, so

far, to place every solution, spiritualist or otherwise, upon the

same footing. The partisans of every alternative are in

somewhat the same position as the members of a deliberative

assembly, in which no one will support the motion of any

other member. Every one can aid effectively in rejecting

every other motion, but no one can succeed in passing his

own. Pressure of urgent necessity may possibly force them

out of this state of practical inaction, by, so to say, breaking

through the opposition at some point of least resistance; but

unless aided by some such pressure they are left in a state of

hopeless dead-lock.

33. Assuming that the spiritualistic solution admits

of, and is to receive, scientific treatment, this, it seems to

me, is the conclusion to which one might sometimes be led

in the face of the evidence offered. We might have to say to

every individual explanation, It is incredible, I cannot accept

it; and unless circumstances should (which it is hardly possible

that they should) force us to a hasty decision,--a decision,

remember, which need indicate no preference of the

judgment beyond what is just sufficient to turn the scale in its

favour as against any other single alternative,--we leave the

matter thus in abeyance. It will very likely be urged that

one of the explanations (assuming that all the possible ones

had been included) must be true; this we readily admit.

It will probably also be urged that (on the often-quoted

principle of Butler) we ought forthwith to accept the one

which, as compared with the others, is the most plausible,

whatever its absolute worth may be. This seems distinctly

an error. To say that such and such an explanation is the

one we should accept, \_if\_ circumstances compelled us to anticipate

our decision, is quite compatible with its present

rejection. The only rational position surely is that of

admitting that the truth is somewhere amongst the various

alternatives, but confessing plainly that we have no such

preference for one over another as to permit our saying anything

else than that we disbelieve each one of them.

34. (VIII.) The very common fallacy of 'judging by

the event,' as it is generally termed, deserves passing notice

here, as it clearly belongs to Probability rather than to Logic;

though its nature is so obvious to those who have grasped the

general principles of our science, that a very few words of

remark will suffice. In one sense every proposition must

consent to be judged by the event, since this is merely, in

other words, submitting it to the test of experience. But

there is the widest difference between the test appropriate

to a universal proposition and that appropriate to a merely

proportional or statistical one. The former is subverted by a

single exception; the latter not merely admits exceptions,

but implies them. Nothing, however, is more common than

to blame advice (in others) because it has happened to turn

out unfortunately, or to claim credit for it (in oneself) because

it has happened to succeed. Of course if the conclusion was

avowedly one of a probable kind we must be prepared with

complacency to accept a hostile event, or even a succession of

them; it is not until the succession shows a disposition to

continue over long that suspicion and doubt should arise, and

then only by a comparison of the degree of the assigned

probability, and the magnitude of the departure from it

which experience exhibits. For any single failure the reply

must be, 'the advice was sound' (supposing, that is, that it

was to be justified in the long run), 'and I shall offer it again

under the same circumstances.'

35. The distinction drawn in the above instance

deserves careful consideration; for owing to the wide difference

between the kind of propositions dealt with in Probability

and in ordinary Logic, and the consequent difference in

the nature of the proof offered, it is quite possible for arguments

of the same general appearance to be valid in the

former and fallacious in the latter, and conversely.

For instance, take the well-known fallacy which consists

in simply converting a universal affirmative, i.e. in passing

from All A is B to All B is A. When, as in common Logic,

the conclusion is to be as certain as the premise, there is not

a word to be said for such a step. But if we look at the

process with the more indulgent eye of Induction or Probability

we see that a very fair case may sometimes be made

out for it. The mere fact that 'Some B is A' raises a certain

presumption that any particular B taken at random will

be an A. There is some reason, at any rate, for the belief,

though in the absence of statistics as to the relative frequency

of A and B we are unable to assign a value to this

belief. I suspect that there may be many cases in which a

man has inferred that some particular B is an A on the

ground that All A is B, who might justly plead in his behalf

that he never meant it to be a necessary, but only a probable

inference. The same remarks will of course apply

also to the logical fallacy of Undistributed Middle.

Now for a case of the opposite kind, i.e. one in which

Probability fails us, whereas the circumstances seem closely

analogous to those in which ordinary inference would be

able to make a stand. Suppose that I know that one letter

in a million is lost when in charge of the post. I write to a

friend and get no answer. Have I any reason to suppose

that the fault lies with him? Here is an event (viz. the

loss of the letter) which has certainly happened; and we suppose

that, of the only two causes to which it can be assigned,

the 'value,' i.e. statistical frequency, of one is accurately

assigned, does it not seem natural to suppose that something

can be inferred as to the likelihood that the other cause had

been operative? To say that nothing can be known about

its adequacy under these circumstances looks at first sight

like asserting that an equation in which there is only one

unknown term is theoretically insoluble.

As examples of this kind have been amply discussed in

the chapter upon Inverse rules of Probability I need do no

more here than remind the reader that no conclusion whatever

can be drawn as to the likelihood that the fault lay

with my friend rather than with the Post Office. Unless we

either know, or make some assumption about, the frequency

with which he neglects to answer the letters he receives, the

problem remains insoluble.

The reason why the apparent analogy, indicated above,

to an equation with only one unknown quantity, fails to hold

good, is that for the purposes of Probability there are really

\_two\_ unknown quantities. What we deal with are proportional

or statistical propositions. Now we are only told that

in the instance in question the letter was lost, not that they

were found to be lost in such and such a proportion of cases.

Had this latter information been given to us we should really

have had but one unknown quantity to determine, viz. the

relative frequency with which my correspondent neglects to

answer his letters, and we could then have determined this

with the greatest ease.

1. Discussed by Mr F. Y. Edgeworth, in the \_Phil. Mag.\_ for April,

1887.

2. \_Journal of the Statistical Soc.\_ (Vol. XLII. p. 328) Dare one

suspect a joke?

3. It appears to have been long known to gamblers under the name of

the \_Martingale\_. There is a paper by Babbage (\_Trans. of Royal

Soc. of Edinburgh\_, for 1823) which discusses certain points

connected with it, but scarcely touches on the subject of the

sections which follow.

4. Attention will be further directed to this distinction in the

chapter on Insurance and Gambling.

5. As by Prévost in the \_Bibliothèque Universelle de Genève\_,

Oct. 1829. The explanation is noted, and apparently accepted, by

Quetelet (\_Physique Sociale\_, I. 171).

6. \_Essay on Probabilities\_, p. 126.

7. This theoretical or absolute neglect of what is very rare must not

be confused with the practical neglect sometimes recommended by

astronomical and other observers. A criterion, known as Chauvenet's,

for indicating the limits of such rejection will be found described

in Mr Merriman's \_Least Squares\_ (p. 166). But this rests on the

understanding that a smaller balance of error would thus result in

the long run. The very rare event is deliberately rejected, not

overlooked.

8. The process of calculation may be readily indicated. There are,

say, about 350,000 letters in the work in question. Since any of the

26 letters of the alphabet may be drawn each time, the possible

number of combinations would be 26^{350,000}; a number which, as may

easily be inferred from a table of logarithms, would demand for its

expression nearly 500,000 figures. Only one of these combinations is

favourable, if we reject variations of spelling. Hence unity divided

by this number would represent the chance of getting the desired

result by successive random selection of the required number of

350,000 letters.

If this chance is thought too small, and any one asks how often the

above random selection must be repeated in order to give him odds of

2 to 1 in favour of success, this also can be easily shown. If the

chance of an event on each occasion is 1/n, the chance of getting it

once at least in n trials is 1 - ((n - 1)/n)^{n}; for we shall do

this unless we fail n times running. When (as in the case in

question) n is very large, this may be shown algebraically to be

equivalent to odds of about 2 to 1. That is, when we have drawn the

requisite quantity of letters a number of times equal to the

inconceivably great number above represented, it is still only 2 to

1 that we shall have secured what we want:--and then we have to

recognize it.

9. The longest life which could reasonably be attributed to any

language would of course dwindle into utter insignificance in the

face of such periods of time as are being here arithmetically

contemplated.

10. We are here assuming of course that the ultimate limit to which

our average tends is known, either from knowledge of the causes or

from previous extensive experience. We are assuming that e.g. the

die is known to be a fair one; if this is not known but a possible

bias has to be inferred from its observed performances, the case

falls under the former head.

11. Except indeed the gamblers. According to a gambling acquaintance

whom Houdin, the conjurer, describes himself as having met at Spa,

"the oftener a particular combination has occurred the more certain

it is that it will not be repeated at the next \_coup\_: this is the

groundwork of all theories of probabilities and is termed the

maturity of chances" (\_Card-sharping exposed\_, p. 85).

CHAPTER XV.

\_INSURANCE AND GAMBLING.\_

1. If the reader will recall to mind the fundamental

postulate of the Science of Probability, established and explained

in the first few chapters, and so abundantly illustrated

since, he will readily recognize that the two opposite

characteristics of individual irregularity and average regularity

will naturally be differently estimated by different

minds. To some persons the elements of uncertainty may

be so painful, either in themselves or in their consequences,

that they are anxious to adopt some means of diminishing

them. To others the ultimate regularity of life, at any rate

within certain departments, its monotony as they consider it,

may be so wearisome that \_they\_ equally wish to effect some

alteration and improvement in its characteristics. We shall

discuss briefly these mental tendencies, and the most simple

and obvious modes of satisfying them.

To some persons, as we have said, the world is all too full

of change and irregularity and consequent uncertainty. Civilization

has done much to diminish these characteristics in

certain directions, but it has unquestionably aggravated them

in other directions, and it might not be very easy to say with

certainty in which of these respects its operation has been, at

present, on the whole most effective. The diminution of

irregularity is exemplified, amongst other things, in the case

of the staple products which supply our necessary food and

clothing. With respect to them, famine and scarcity are by

comparison almost unknown now, at any rate in tolerably

civilized communities. As a consequence of this, and of the

vast improvements in the means of transporting goods and

conveying intelligence, the fluctuations in the price of such

articles are much less than they once were. In other directions,

however, the reverse has been the case. Fashion, for

instance, now induces so many people in every large community

simultaneously to desire the same thing, that great

fluctuations in value may ensue. Moreover a whole group of

causes (to enter upon any discussion of which would be to

trench upon the ground of Political Economy) combine to

produce great and frequent variations in matters concerning

credit and the currency, which formerly had no existence.

Bankruptcy, for instance, is from the nature of the case,

almost wholly a creation of modern times. We will not attempt

to strike any balance between these opposite results

of modern civilization, beyond remarking that in matters of

prime importance the actual uncertainties have been probably

on the whole diminished, whereas in those which affect

the pocket rather than the life, they have been rather increased.

It might also be argued with some plausibility

that in cases where the actual uncertainties have not become

greater, they have for all practical purposes done so, by their

consequences frequently becoming more serious, or by our

estimate of these consequences becoming higher.

2. However the above question, as to the ultimate

balance of gain or loss, should be decided, there can be no

doubt that many persons find the present amount of uncertainty

in some of the affairs of life greater than suits their

taste. How are they to diminish it? Something of course

may be done, as regards the individual cases, by prudence

and foresight. Our houses may be built with a view not to

take fire so readily, or precautions may be taken that there

shall be fire-engines at hand. In the warding off of death

from disease and accident, something may be done by every

one who chooses to live prudently. Precautions of the above

kind, however, do not introduce any questions of Probability.

These latter considerations only come in when we begin to

invoke the regularity of the average to save us from the

irregularities of the details. We cannot, it is true, remove

the uncertainty in itself, but we can so act that the consequences

of that uncertainty shall be less to us, or to those in

whom we are interested. Take the case of Life Insurance.

A professional man who has nothing but the income he earns

to depend upon, knows that the whole of that income may

vanish in a moment by his death. This is a state of things

which he cannot prevent; and if he were the only one in

such a position, or were unable or unwilling to combine with

his fellow-men, there would be nothing more to be done in

the matter except to live within his income as much as possible,

and so leave a margin of savings.

3. There is however an easy mode of escape for him.

All that he has to do is to agree with a number of others,

who are in the same position as himself, to make up, so to

say, a common purse. They may resolve that those of their

number who live to work beyond the average length of life

shall contribute to support the families of those who die

earlier. If a few only concurred in such a resolution they

would not gain very much, for they would still be removed

by but a slight step from that uncertainty which they are

seeking to escape. What is essential is that a considerable

number should thus combine so as to get the benefit of that

comparative regularity which the average, as is well known,

almost always tends to exhibit.

4. The above simple considerations really contain the

essence of all insurance. Such points as the fact that the

agreement for indemnity extends only to a certain definite

sum of money; and that instead of calling for an occasional

general contribution at the time of the death of each member

they substitute a fixed annual premium, out of the proceeds

of which the payment is to be made, are merely accidents of

convenience and arrangement. Insurance is simply equivalent

to a mutual contract amongst those who dread the consequences

of the uncertainty of their life or employment,

that they will employ the aggregate regularity to neutralize

as far as possible the individual irregularity. They know

that for every one who gains by such a contract another will

lose as much; or if one gains a great deal many must have

lost a little. They know also that hardly any of their number

can expect to find the arrangement a 'fair' one, in the

sense that they just get back again what they have paid in

premiums, after deducting the necessary expenses of management;

but they deliberately prefer this state of things.

They consist of a body of persons who think it decidedly

better to leave behind them a comparatively fixed fortune,

rather than one which is extremely uncertain in amount;

although they are perfectly aware that, owing to the unavoidable

expenses of managing the affairs of such a society,

the comparatively fixed sum, so to be left, will be a trifle less

than the average fortunes which would have been left had

no such system of insurance been adopted.

As this is not a regular treatise upon Insurance no more

need be said upon the exact nature of such societies, beyond

pointing out that they are of various different kinds. Sometimes

they really are what we have compared them with,

viz. mutual agreements amongst a group of persons to make

up each other's losses to a certain extent. Into this category

fall the Mutual Insurance Societies, Benefit Societies, Trades

Unions (in respect of some of their functions), together with

innumerable other societies which go by various names.

Sometimes they are companies worked by proprietors or

shareholders for a profit, like any other industrial enterprise.

This is the case, I believe, with the majority of the

ordinary Life Insurance Societies. Sometimes, again, it is

the State which undertakes the management, as in the case

of our Post Office Insurance business.

5. It is clear that there is no necessary limit to the

range of application of this principle.[1] It is quite conceivable

that the majority of the inhabitants of some nation

might be so enamoured of security that they should devise

a grand insurance society to cover almost every concern in

life. They could not indeed abolish uncertainty, for the

conditions of life are very far from permitting this, but they

could without much difficulty get rid of the worst of the

\_consequences\_ of it. They might determine to insure not

merely their lives, houses, ships, and other things in respect

of which sudden and total loss is possible, but also

to insure their business; in the sense of avoiding not only

bankruptcy, but even casual bad years, on the same principle

of commutation. Unfamiliar as such an aim may appear

when introduced in this language, it is nevertheless one

which under a name of suspicious import to the conservative

classes has had a good deal of attention directed to it. It is

really scarcely anything else than Communism, which might

indeed be defined as a universal and compulsory[2] insurance

society which is to take account of all departments of business,

and, in some at least of its forms, to invade the province

of social and domestic life as well.

Although nothing so comprehensive as this is likely to

be practically carried out on any very large scale, it deserves

notice that the principle itself is steadily spreading in every

direction in matters of detail. It is, for instance, the great

complaint against Trades Unions that they too often seek to

secure these results in respect of the equalization of the

workmen's wages, thus insuring to some degree against incompetence,

as they rightly and wisely do against illness and

loss of work. Again, there is the Tradesman's Mutual Protection

Society, which insures against the occasional loss

entailed by the necessity of having to conduct prosecutions

at law. There are societies in many towns for the prosecution

of petty thefts, with the object of escaping the same

uncertain and perhaps serious loss. Amongst instances of

insurance \_for\_ the people rather than \_by\_ them, there is of

course the giant example of the English Poor Law, in

which the resemblance to an initial Communistic system

becomes very marked. The poor are insured against loss

of work arising not only from illness and old age, but from

any cause except wilful idleness. They do not, it is true,

pay the whole premium, but since they mostly bear some

portion of the burden of municipal and county taxation

they must certainly be considered as paying a part of

the premium. In some branches also of the public and

private services the system is adopted of deducting a percentage

from the wage or salary, for the purpose of a

semi-compulsory insurance against death, illness or superannuation.

6. Closely connected with Insurance, as an application

of Probability, though of course by contrast, stands Gambling.

Though we cannot, in strictness, term either of these practices

the converse of the other, it seems nevertheless correct

to say that they spring from opposite mental tendencies.

Some persons, as has been said, find life too monotonous for

their taste, or rather the region of what can be predicted

with certainty is too large and predominant in their estimation.

They can easily adopt two courses for securing the

changes they desire. They may, for one thing, aggravate

and intensify the results of events which are comparatively

incapable of prevision, these events not being in themselves

of sufficient importance to excite any strong emotions. The

most obvious way of doing this is by betting upon them.

Or again, they may invent games or other pursuits, the

individual contingencies of which are entirely removed from

all possible human prevision, and then make heavy money

consequences depend upon these contingencies. This is

gambling proper, carried on mostly by means of cards and

dice and the roulette.

The gambling spirit, as we have said, seeks for the excitement

of uncertainty and variety. When therefore people

make a long continued practice of playing, especially if the

stakes for which they play are moderate in comparison with

their fortune, this uncertainty from the nature of the case

begins to diminish. The thoroughly practised gambler, if

he possesses more than usual skill (in games where skill

counts for something), must be regarded as a man following

a profession, though a profession for the most part of a risky

and exciting kind, to say nothing of its ignoble and often

dishonest character. If, on the other hand, his skill is below

the average, or the game is one in which skill does not tell

and the odds are slightly in favour of his antagonist, as in

the gaming tables, one light in which he can be regarded

is that of a man who is following a favourite amusement;

if this amusement involves a constant annual outlay on his

part, that is nothing more than what has to be said of most

other amusements.

7. We cannot, of course, give such a rational explanation

as the above in every case. There are plenty of

novices, and plenty of fanatics, who go on steadily losing in

the full conviction that they will eventually come out winners.

But it is hard to believe that such ignorance, or such

intellectual twist, can really be so widely prevalent as would

be requisite to constitute them the rule rather than the exception.

There must surely be some very general impulse

which is gratified by such resources, and it is not easy to see

what else this can be than a love of that variety and consequent

excitement which can only be found in perfection

where exact prevision is impossible.

It is of course very difficult to make any generalization

here as to the comparative prevalence of various motives

amongst mankind; but when one considers what is the

difference which most quiet ordinary whist players feel

between a game for 'love' and one in which there is a

small stake, one cannot but assign a high value to the

influence of a wish to emphasize the excitement of loss

and gain.

I would not for a moment underrate the practical dangers

which are found to attend the practice of gambling. It

is remarked that the gambler, if he continues to play for a

long time, is under an almost irresistible impulse to increase

his stakes, and so re-introduce the element of uncertainty.

It is in fact this tendency to be thus led on, which makes

the principal danger and mischief of the practice. Risk and

uncertainty are still such normal characteristics of even civilized

life, that the mere extension of such tendencies into

new fields does not in itself offer any very alarming prospect.

It is only to be deprecated in so far as there is a

danger, which experience shows to be no trifling one, that

the fascination found in the pursuit should lead men into

following it up into excessive lengths.[3]

8. The above general treatment of Gambling and

Insurance seems to me the only rational and sound principle

of division;--namely, that on which the different practices

which, under various names, are known as gambling

or insurance, are arranged in accordance with the spirit of

which they are the outcome, and therefore of the results

which they are designed to secure. If we were to attempt

to judge and arrange them according to the names which

they currently bear, we should find ourselves led to no

kind of systematic division whatever; the fact being that

since they all alike involve, as their essential characteristic,

payments and receipts, one or both of which are necessarily

uncertain in their date or amount, the names may often be

interchanged.

For instance, a lottery and an ordinary insurance society

against accident, if we merely look to the processes performed

in them, are to all intents and purposes identical.

In each alike there is a small payment which is certain in

amount, and a great receipt which is uncertain in amount.

A great many persons pay the small premium, whereas a

few only of their number obtain a prize, the rest getting

no return whatever for their outlay. In each case alike,

also, the aggregate receipts and losses are intended to

balance each other, after allowing for the profits of those

who carry on the undertaking. But of course when we

take into account the occasions upon which the insurers

get their prizes, we see that there is all the difference in the

world between receiving them at haphazard, as in a lottery,

and receiving them as a partial set-off to a broken limb or

injured constitution, as in the insurance society.

Again, the language of betting may be easily made to

cover almost every kind of insurance. Indeed De Morgan

has described life insurance as a bet which the individual

makes with the company, that he will not live beyond a

certain age. If he dies young, he is pecuniarily a gainer, if

he dies late he is a loser.[4] Here, too, though the expression

is technically quite correct (since any such deliberate risk of

money, upon an unproductive venture, may fall under the

definition of a bet), there is the broadest distinction between

betting with no other view whatever than that of risking

money, and betting with the view of diminishing risk and

loss as much as possible. In fact, if the language of sporting

life is to be introduced into the matter, we ought, I presume,

to speak of the insurer as 'hedging' against his death.

9. Again, in Tontines we have a system of what is

often called Insurance, and in certain points rightly so,

but which is to all intents and purposes simply and absolutely

a gambling transaction. They have been entirely

abandoned, I believe, for some time, but were once rather

popular, especially in France. On this plan the State, or

whatever society manages the business, does not gain anything

until the last member of the Tontine is dead. As the

number of the survivors diminishes, the same sum-total of

annuities still continues to be paid amongst them, as long as

any are left alive, so that each receives a gradually increasing

sum. Hence those who die early, instead of receiving the

most, as on the ordinary plan, receive the least; for at the

death of each member the annuity ceases absolutely, so far

as he and his relations are concerned. The whole affair

therefore is to all intents and purposes a gigantic system

of betting, to see which can live the longest; the State

being the common stake-holder, and receiving a heavy commission

for its superintendence, this commission being naturally

its sole motive for encouraging such a transaction. It is

recorded of one of the French Tontines[5] that a widow of 97

was left, as the last survivor, to receive an annuity of 73,500

livres during the rest of the life which she could manage to

drag on after that age;--she having originally subscribed a

single sum of 300 livres only. It is obvious that such a system

as this, though it may sometimes go by the name of insurance,

is utterly opposed to the spirit of true insurance,

since it tends to aggravate existing inequalities of fortune

instead of to mitigate them. The insurer here bets that

he will die old; in ordinary insurance he bets that he will

die young.

Again, to take one final instance, common opinion often

regards the bank or company which keeps a \_rouge et noir\_

table, and the individuals who risk their money at it, as being

both alike engaged in gambling. So they may be, technically,

but for all practical purposes such a bank is as sure

and safe a business as that of any ordinary insurance society,

and probably far steadier in its receipts than the majority of

ordinary trades in a manufacturing or commercial city. The

bank goes in for many and small transactions, in proportion

to its capital; their customers, very often, in proportion to

their incomes go in for very heavy transactions. That the

former comes out a gainer year after year depends, of course,

upon the fact that the tables are notoriously slightly in their

favour. But the \_steadiness\_ of these gains when compared

with the unsteadiness of the individual losses depends simply

upon,--in fact, is merely an illustration of,--the one great

permanent contrast which lies at the basis of all reasoning

in Probability.

10. We have so far regarded Insurance and Gambling

as being each the product of a natural impulse, and as having

each, if we look merely to experience, a great mass of human

judgment in its favour. The popular moral judgment, however,

which applauds the one and condemns the other rests in great

part upon an assumption, which has doubtless much truth

in it, but which is often interpreted with an absoluteness which

leads to error in each direction;--the duty of insurance being

too peremptorily urged upon every one, and the practice of

gambling too universally regarded as involving a sacrifice of

real self-interest, as being in fact little better than a persistent

blunder. The assumption in question seems to be

extracted from the acknowledged advantages of insurance,

and then invoked to condemn the practice of gambling. But

in so doing the fact does not seem to be sufficiently recognized

that the latter practice, if we merely look to the extent

and antiquity of the tacit vote of mankind in its favour,

might surely claim to carry the day.

It is of course obvious that in all cases with which we

are concerned, the aggregate wealth is unaltered; money

being merely transferred from one person to another. The

loss of one is precisely equivalent to the gain of another.

At least this is the approximation to the truth with which we

find it convenient to start.[6] Now if the happiness which is

yielded by wealth were always in direct proportion to its

amount, it is not easy to see why insurance should be advocated

or gambling condemned. In the case of the latter this

is obvious enough. I have lost £50, say, but others (one or

more as the case may be) have gained it, and the increase

of their happiness would exactly balance the diminution of

mine. In the case of Insurance there is a slight complication,

arising from the fact that the falling in of the policy

does not happen at random (otherwise, as already pointed

out, it would be simply a lottery), but is made contingent

upon some kind of loss, which it is intended as far as possible

to balance. I insure myself on a railway journey, break my

leg in an accident, and, having paid threepence for my

ticket, receive say £200 compensation from the insurance

company. The same remarks, however, apply here; the

happiness I acquire by this £200 would only just balance the

aggregate loss of the 16,000 who have paid their threepences

and received no return for them, were happiness always

directly proportional to wealth.

11. The practice of Insurance does not, I think, give

rise to many questions of theoretic interest, and need not

therefore detain us longer. The fact is that it has hardly

yet been applied sufficiently long and widely, or to matters

which admit of sufficiently accurate statistical treatment,

except in one department. This, of course, is Life Insurance;

but the subject is one which requires constant attention to

details of statistics, and is (rightly) mainly carried out in

strict accordance with routine. As an illustration of this

we need merely refer to the works of De Morgan,--a professional

actuary as well as a writer on the theory of Probability,--who

has found but little opportunity to aid his speculative

treatment of Probability by examples drawn from this class

of considerations.

With Gambling it is otherwise. Not only have a variety

of interesting single problems been discussed (of which the

Petersburg problem is the best known) but several speculative

questions of considerable importance have been raised.

One of these concerns the disadvantages of the practice of

gambling. There have been a number of writers who, not

content with dwelling upon the obvious moral and indirect

mischief which results, in the shape of over-excitement,

consequent greed, withdrawal from the steady business

habits which alone insure prosperity in the long run, diversion

of wealth into dishonest hands, &c., have endeavoured

to demonstrate the necessary loss caused by the practice.

12. These attempts may be divided into two classes.

There are (1) those which appeal to merely numerical considerations,

and (2) those which introduce what is called the

'moral' as distinguished from the mathematical value of a

future contingency.

(1) For instance, an ingenious attempt has been made

by Mr Whitworth to prove that gambling is necessarily

disadvantageous on purely mathematical grounds.

When two persons play against each other one of the two

must be ruined sooner or later, even though the game be a

fair one, supposing that they go on playing long enough; the

one with the smaller income having of course the worst

chance of being the lucky survivor. If one of them has a

finite, and the other an infinite income, it must clearly be

the former who will be the ultimate sufferer if they go on

long enough. It is then maintained that this is in fact every

individual gambler's position, "no one is restricted to gambling

with one single opponent; the speculator deals with the

public at large, with a world whose resources are practically

unlimited. There is a prospect that his operations may

terminate to his own disadvantage, through his having nothing

more to stake; but there is no prospect that it will

terminate to his advantage through the exhaustion of the

resources of the world. Every one who gambles is carrying

on an unequal warfare: he is ranged with a restricted capital

against an adversary whose means are infinite."[7]

In the above argument it is surely overlooked that the

adversaries against whom he plays are not one body with a

common purse, like the bank in a gambling establishment.

Each of these adversaries is in exactly the same position as

he himself is, and a precisely similar proof might be employed

to show that each of them must be eventually ruined

which is of course a reduction to absurdity. Gambling can

only transfer money from one player to another, and therefore

none of it can be actually lost.

13. What really becomes of the money, when they

play to extremity, is not difficult to see. First suppose a

limited number of players. If they go on long enough, the

money will at last all find its way into the pocket of some

one of their number. If their fortunes were originally equal,

each stands the same chance of being the lucky survivor;

in which case we cannot assert, on any numerical grounds,

that the prospect of the play is disadvantageous to any one

of them. If their fortunes were unequal, the one who had

the largest sum to begin with can be shown to have the

best chance, according to some assignable law, of being left

the final winner; in which case it must be just as advantageous

for him, as it was disadvantageous for his less wealthy

competitors.

When, instead of a limited number of players, we suppose

an unlimited number, each as he is ruined retiring

from the table and letting another come in, the results are

more complicated, but their general tendency can be readily

distinguished. If we supposed that no one retired except

when he was ruined, we should have a state of things in

which all the old players were growing gradually richer. In

this case the prospect before the new comers would steadily

grow worse and worse, for their chance of winning against

such rich opponents would be exceedingly small. But as

this is an unreasonable supposition, we ought rather to

assume that not only do the ruined victims retire, but also

that those who have gained fortunes of a certain amount

retire also, so that the aggregate and average wealth of the

gambling body remains pretty steady. What chance any

given player has of being ruined, and how long he may

expect to hold out before being ruined, will depend of course

upon the initial incomes of the players, the rules of the

game, the stakes for which they play, and other considerations.

But it is clear that for all that is lost by one, a

precisely equal sum must be gained by others, and that

therefore any particular gambler can only be cautioned beforehand

that his conduct is not to be recommended, by

appealing to some such suppositions as those already mentioned

in a former section.

14. As an additional justification of this view the

reader may observe that the state of things in the last

example is one which, expressed in somewhat different

language and with a slight alteration of circumstances, is

being incessantly carried on upon a gigantic scale upon

every side of us. Call it the competition of merchants and

traders in a commercial country, and the general results are

familiar enough. It is true that in so far as skill comes into

the question, they are not properly gamblers; but in so far

as chance and risk do, they may be fairly so termed, and in

many branches of business this must necessarily be the case

to a very considerable extent. Whenever business is carried

on in a reckless way, the comparison is on general grounds

fair enough. In each case alike we find some retiring ruined,

and some making their fortunes; and in each case alike

also the chances, \_coeteris paribus\_, lie with those who have

the largest fortunes. Every one is, in a sense, struggling

against the collective commercial world, but since each of his

competitors is doing the same, we clearly could not caution

any of them (except indeed the poorer ones) that their efforts

must finally end in disadvantage.

15. If we wish to see this result displayed in its

most decisive form we may find a good analogy in a very

different class of events, viz. in the fate of \_surnames\_. We

are all gamblers in this respect, and the game is carried

out to the last farthing with a rigour unknown at Newmarket

or Monte Carlo. In its complete treatment the

subject is a very intricate one,[8] but a simple example will

serve to display the general tendency. Suppose a colony

comprising 1000 couples of different surnames, and suppose

that each of these has four children who grow up to marry.

Approximately, one in 16 of these families will consist of

girls only; and therefore, under ordinary conventions, about

62 of the names will have disappeared for ever after the

next generation. Four again out of 16 will have but one

boy, each of whom will of course be in the same position

as his father, viz. the sole representative of his name.

Accordingly in the next generation one in 16 of these names

will again drop out, and so the process continues. The

number which disappears in each successive generation becomes

smaller, as the stability of the survivors becomes

greater owing to their larger numbers. But there is no

check to the process.

16. The analogy here is a very close one, the names

which thus disappear corresponding to the gamblers who

retire ruined and those which increase in number corresponding

to the lucky winners. The ultimate goal in each

case alike,--of course an exceedingly remote one,--is the

exclusive survival of one at the expense of all the others.

That one surname does thus drop out after another must

have struck every one who has made any enquiry into family

genealogy, and various fanciful accounts have been given

by those unfamiliar with the theory of probability. What

is often apt to be overlooked is the extreme slightness of

what may be termed the "turn of the tables" in favour

of the survival at each generation. In the above numerical

example we have made an extravagantly favourable supposition,

by assuming that the population doubles at every

generation. In an old and thickly populated country where

the numbers increase very slowly, we should be much nearer

the mark in assuming that the average effective family,--that

is, the average number of children who live to marry,--was

only \_two\_. In this case every family which was represented

at any time by but a single male would have but

three chances in four of surviving extinction, and of course

the process of thinning out would be a more rapid one.

17. The most interesting class of attempts to prove

the disadvantages of gambling appeal to what is technically

called 'moral expectation' as distinguished from

'mathematical expectation.' The latter may be defined

simply as the average money value of the venture in

question; that is, it is the product of the amount to be

gained (or lost) and the chance of gaining (or losing) it.

For instance, if I bet four to one in sovereigns against the

occurrence of ace with a single die there would be, on the

average of many throws, a loss of four pounds against a gain

of five pounds on each set of six occurrences; i.e. there

would be an average gain of three shillings and fourpence

on each throw. This is called the true or mathematical

expectation. The so-called 'moral expectation', on the other

hand, is the subjective value of this mathematical expectation.

That is, instead of reckoning a money fortune in

the ordinary way, as what it \_is\_, the attempt is made to

reckon it at what it is \_felt\_ to be. The elements of computation

therefore become, not pounds and shillings, but sums

of pleasure enjoyed actually or in prospect. Accordingly

when reckoning the present value of a future gain, we must

now multiply, not the objective but the subjective value,

by the chance we have of securing that gain.

With regard to the exact relation of this moral fortune

to the physical various more or less arbitrary assumptions

have been made. One writer (Buffon) considers that the

moral value of any given sum varies inversely with the

total wealth of the person who gains it. Another (D. Bernoulli)

starting from a different assumption, which we shall

presently have to notice more particularly, makes the moral

value of a fortune vary as the logarithm of its actual amount.[9]

A third (Cramer) makes it vary with the square root of the

amount.

18. Historically, these proposals have sprung from

the wish to reconcile the conclusions of the Petersburg

problem with the dictates of practical common sense; for,

by substituting the moral for the physical estimate the

total value of the expectation could be reduced to a finite

sum. On this ground therefore such proposals have no great

interest, for, as we have seen, there is no serious difficulty in

the problem when rightly understood.

These same proposals however have been employed in

order to prove that gambling is necessarily disadvantageous,

and this to both parties. Take, for instance, Bernoulli's supposition.

It can be readily shown that if two persons each

with a sum of £50 to start with choose to risk, say, £10 upon

an even wager there will be a loss of happiness as a result;

for the pleasure gained by the possessor of £60 will not

be equal to that which is lost by the man who leaves off

with £40.[10]

19. This is the form of argument commonly adopted;

but, as it stands, it does not seem conclusive. It may surely

be replied that all which is thus proved is that \_inequality\_ is

bad, on the ground that two fortunes of £50 are better than

one of £60 and one of £40. Conceive for instance that the

original fortunes had been £60 and £40 respectively, the

event may result in an increase of happiness; for this will

certainly be the case if the richer man loses and the fortunes

are thus equalized. This is quite true; and we are therefore

obliged to show,--what can be very easily shown,--that if

the other alternative had taken place and the two fortunes

had been made still more unequal (viz. £65 and £35 respectively)

the happiness thus lost would more than balance

what would have been gained by the equalization. And

since these two suppositions are equally likely there will be a

loss in the long run.

The consideration just adduced seems however to show

that the common way of stating the conclusion is rather

misleading; and that, on the assumption in question as

to the law of dependence of happiness on wealth, it really is

the case that the effective element in rendering gambling

disadvantageous is its tendency to the increase of the inequality

in the distribution of wealth.

20. This raises two questions, one of some speculative

interest in connection with our subject, and the other of

supreme importance in the conduct of life. The first is this:

quite apart from any particular assumption which we make

about moral fortunes or laws of variation of happiness, is

it the fact that gambling tends to increase the existing

inequalities of wealth? Theoretically there is no doubt that

this is so. Take the simplest case and suppose two people

tossing for a pound. If their fortunes were equal to begin

with there must be resultant inequality. If they were

unequal there is an even chance of the inequality being

increased or diminished; but since the increase is proportionally

greater than the decrease, the final result remains of

the same kind as when the fortunes were equal.[11] Taking a

more general view the same conclusion underlies all our

reasoning as to the averages of large numbers, viz. that the

resultant divergencies increase absolutely (however they

diminish relatively) as the numbers become greater. And of

course we refer to these absolute divergencies when we

are talking of the distribution of wealth.

21. This is the theoretic conclusion. How far the

actual practice of gambling introduces counteracting agencies

must be left to the determination of those who are competent

to pronounce. So far as outsiders are authorised

to judge from what they read in the newspapers and other

public sources of information, it would appear that these

counteracting agencies are very considerable, and that in

consequence it is a rather insecure argument to advance

against gambling. Many a large fortune has notoriously

been squandered on the race-course or in gambling saloons,

and most certainly a large portion, if not the major part,

has gone to swell the incomes of many who were by comparison

poor. But the solution of this question must clearly be

left to those who have better opportunities of knowing

the facts than is to be expected on the part of writers on

Probability.

22. The general conclusion to be drawn is that those

who invoked this principle of moral fortune as an argument

against gambling were really raising a much more intricate

and far-reaching problem than they were aware of. What

they were at work upon was the question, What is the

distribution of wealth which tends to secure the maximum

of happiness? Is this best secured by equality or inequality?

Had they really followed out the doctrine on which their

denunciation of gambling was founded they ought to have

adopted the Socialist's ideal as being distinctly that which

tends to increase happiness. And they ought to have

brought under the same disapprobation which they expressed

against gambling all those tendencies of modern

civilized life which work in the same direction. For instance;

keen competition, speculative operations, extended

facilities of credit, mechanical inventions, enlargement of

business operations into vast firms:--all these, and other

similar tendencies too numerous to mention here, have had

some influence in the way of adding to existing inequalities.

They are, or have been, in consequence denounced by

socialists: are we honestly to bring them to this test in

order to ascertain whether or not they are to be condemned?

The reader who wishes to see what sort of problems this

assumption of 'moral fortune' ought to introduce may be

recommended to read Mr F. Y. Edgeworth's \_Mathematical

Psychics\_, the only work with which I am acquainted which

treats of these questions.

1. The question of the advisability of inoculation against the

small-pox, which gave rise to much discussion amongst the writers on

Probability during the last century, is a case in point of the same

principles applied to a very different kind of instance. The loss

against which the insurance was directed was death by small-pox, the

premium paid was the illness and other inconvenience, and the very

small risk of death, from the inoculation. The disputes which thence

arose amongst writers on the subject involved the same difficulties

as to the balance between certain moderate loss and contingent great

loss. In the seventeenth century it seems to have been an occasional

practice, before a journey into the Mediterranean, to insure against

capture by Moorish pirates, with a view to secure having the ransom

paid. (See, for an account of some extraordinary developments of the

insurance principle, Walford's \_Insurance Guide and Handbook\_. It is

not written in a very scientific spirit, but it contains much

information on all matters connected with insurance.)

2. All that is meant by the above comparison is that the ideal aimed

at by Communism is similar to that of Insurance. If we look at the

processes by which it would be carried out, and the means for

enforcing it, the matter would of course assume a very different

aspect. Similarly with the action of Trades Unionism referred to in

the next paragraph.

3. One of the best discussions that I have recently seen on these

subjects, by a writer at once thoroughly competent and well

informed, is in Mr Proctor's \_Chance and Luck\_. It appears to me

however that he runs into an extreme in his denunciation not of the

folly but of the dishonesty of all gambling. Surely also it is a

strained use of language to speak of all lotteries as 'unfair' and

even 'swindling' on the ground that the sum-total of what they

distribute in prizes is less than that of what they receive in

payments. The difference, in respect of information deliberately

withheld and false reports wilfully spread, between most of the

lotteries that have been supported, and the bubble companies which

justly deserve the name of swindles, ought to prevent the same name

being applied to both.

4. "A fire insurance is a simple bet between the office and the party,

and a life insurance is a collection of wagers. There is something

of the principle of a wager in every transaction in which the

results of a future event are to bring gain or loss." \_Penny

Cyclopædia\_, under the head of \_Wager\_.

5. \_Encyclopédie Methodique\_, under the head of \_Tontines\_.

6. Of course, if we introduce considerations of Political Economy,

corrections will have to be made. For one thing, every Insurance

Office is, as De Morgan repeatedly insists, a \_Savings Bank\_ as well

as an Insurance Office. The Office invests the premiums, and can

therefore afford to pay a larger sum than would otherwise be the

case. Again, in the case of gambling, a large loss of capital by any

one will almost necessarily involve an actual destruction of wealth;

to say nothing of the fact that, practically, gambling often causes

a constant transfer of wealth from productive to unproductive

purposes.

7. \_Choice and Chance\_, Ed. II. p. 208.

8. It was, I believe, first treated as a serious problem by Mr Galton.

(See the \_Journal Anthrop. Inst.\_ Vol. IV. 1875, where a complete

mathematical solution is indicated by Mr H. W. Watson.)

9. Bernoulli himself does not seem to have based his conclusions upon

actual experience. But it is a noteworthy fact that the assumption

with which he starts, viz. that the subjective value of any small

increment (dx) is inversely proportional to the sum then possessed

(x), and which leads at once to the logarithmic law above mentioned,

is identical with one which is now familiar enough to every

psychologist. It is what is commonly called Fechner's Law, which he

has established by aid of an enormous amount of careful experiment

in the case of a number of our simple sensations. But I do not

believe that he has made any claim that such a law holds good in the

far more intricate dependence of happiness upon wealth.

10. The formula expressive of this moral happiness is c log x/a; where

x stands for the physical fortune possessed at the time, and a for

that small value of it at which happiness is supposed to disappear:

c being an arbitrary constant. Let two persons, whose fortune is x,

risk y on an even bet. Then the balance, as regards happiness, must

be drawn between

c log x/a and 1/2c log (x + y)/a + 1/2c log (x - y)/a,

or log x^{2} and log(x + y)(x - y),

or x^{2} and x^{2} - y^{2},

the former of which is necessarily the greater.

11. This may be seen more clearly as follows. Suppose two pair of

gamblers, each pair consisting of men possessing £50 and £30

respectively. Now if we suppose the richer man to win in one case

and the poorer in the other these two results will be a fair

representation of the average; for there are only two alternatives

and these will be equally frequent in the long run. It is obvious

that we have had two fortunes of £50 and two of £30 converted into

one of £20, two of £40, and one of £60. And this is clearly an

increase of inequality.

CHAPTER XVI.

\_THE APPLICATION OF PROBABILITY TO TESTIMONY.\_

1. On the principles which have been adopted in this

work, it becomes questionable whether several classes of

problems which may seem to have acquired a prescriptive

right to admission, will not have to be excluded from the

science of Probability. The most important, perhaps, of

these refer to what is commonly called the credibility of

testimony, estimated either at first hand and directly, or as

influencing a juryman, and so reaching us through his

sagacity and trustworthiness. Almost every treatise upon

the science contains a discussion of the principles according

to which credit is to be attached to combinations of the

reports of witnesses of various degrees of trustworthiness, or

the verdicts of juries consisting of larger or smaller numbers.

A great modern mathematician, Poisson, has written an

elaborate treatise expressly upon this subject; whilst a considerable

portion of the works of Laplace, De Morgan, and

others, is devoted to an examination of similar enquiries. It

would be presumptuous to differ from such authorities as

these, except upon the strongest grounds; but I confess that

the extraordinary ingenuity and mathematical ability which

have been devoted to these problems, considered as questions

in Probability, fails to convince me that they ought to have

been so considered. The following are the principal grounds

for this opinion.

2. It will be remembered that in the course of the

chapter on Induction we entered into a detailed investigation

of the process demanded of us when, instead of the

appropriate propositions from which the inference was to be

made being set before us, the \_individual\_ presented himself,

and the task was imposed upon us of selecting the requisite

groups or series to which to refer him. In other words, instead

of calculating the chance of an event from determinate

conditions of frequency of its occurrence (these being either

obtained by direct experience, or deductively inferred) we

have to \_select\_ the conditions of frequency out of a plurality

of more or less suitable ones. When the problem is presented

to us at such a stage as this, we may of course assume

that the preliminary process of obtaining the statistics

which are extended into the proportional propositions has

been already performed; we may suppose therefore that we

are already in possession of a quantity of such propositions,

our principal remaining doubt being as to which of them

we should then employ. This selection was shown to be

to a certain extent arbitrary; for, owing to the fact of the

individual possessing a large number of different properties,

he became in consequence a member of different series or

groups, which might present different averages. We must

now examine, somewhat more fully than we did before,

the practical conditions under which any difficulty arising

from this source ceases to be of importance.

3. One condition of this kind is very simple and obvious.

It is that the different statistics with which we are

presented should not in reality offer materially different

results, If, for instance, we were enquiring into the probability

of a man aged forty dying within the year, we might

if we pleased take into account the fact of his having red

hair, or his having been born in a certain county or town.

Each of these circumstances would serve to specialize the

individual, and therefore to restrict the limits of the statistics

which were applicable to his case. But the consideration of

such qualities as these would either leave the average precisely

as it was, or produce such an unimportant alteration

in it as no one would think of taking into account. Though

we could hardly say with certainty of any conceivable

characteristic that it has absolutely no bearing on the

result, we may still feel very confident that the bearing of

such characteristics as these is utterly insignificant. Of

course in the extreme case of the things most perfectly

suited to the Calculus of Probability, viz. games of pure

chance, these subsidiary characteristics are quite irrelevant.

Any further particulars about the characteristics of the cards

in a really fair pack, beyond those which are familiar to all

the players, would convey no information whatever about the

result.

Or again; although the different sets of statistics may

not as above give almost identical results, yet they may do

what practically comes to very much the same thing, that

is, arrange themselves into a small number of groups, all of

the statistics in any one group practically coinciding in their

results. If for example a consumptive man desired to insure

his life, there would be a marked difference in the statistics

according as we took his peculiar state of health into account

or not. We should here have two sets of statistics, so clearly

marked off from one another that they might almost rank

with the distinctions of natural kinds, and which would in

consequence offer decidedly different results. If we were

to specialize still further, by taking into account insignificant

qualities like those mentioned in the last paragraph, we

might indeed get more limited sets of statistics applicable

to persons still more closely resembling the individual in

question, but these would not differ sufficiently in their

results to make it worth our while to do so. In other words,

the different propositions which are applicable to the case in

point arrange themselves into a limited number of groups,

which, and which only, need be taken into account; whence

the range of choice amongst them is very much diminished

in practice.

4. The reasons for the conditions above described are

not difficult to detect. Where these conditions exist the

process of selecting a series or class to which to refer any

individual is very simple, and the selection is, for the particular

purposes of inference, final. In any case of insurance,

for example, the question we have to decide is of the very

simple kind; Is A. B. a man of a certain age? If so one in

fifty in his circumstances will die in the course of the year.

If any further questions have to be decided they would be of

the following description. Is A. B. a healthy man? Does he

follow a dangerous trade? But here too the classes in

question are but few, and the limits by which they are

bounded are tolerably precise; so that the reference of an

individual to one or other of them is easy. And when we

have once chosen our class we remain untroubled by any

further considerations; for since no other statistics are supposed

to offer a materially different average, we have no

occasion to take account of any other properties than those

already noticed.

The case of games of chance, already referred to, offers

of course an instance of these conditions in an almost ideal

state of perfection; the same circumstances which fit them

so eminently for the purposes of fair gambling, fitting them

equally to become examples in Probability. When a die is

to be thrown, all persons alike stand on precisely the same

footing of knowledge and of ignorance about the result; the

only data to which any one could appeal being that each

face turns up on an average once in six times.

5. Let us now examine how far the above conditions

are fulfilled in the case of problems which discuss what is

called the credibility of testimony. The following would be

a fair specimen of one of the elementary enquiries out of

which these problems are composed;--Here is a statement

made by a witness who lies once in ten times, what am I to

conclude about its truth? Objections might fairly be raised

against the possibility of thus assigning a man his place

upon a graduated scale of mendacity. This however we will

pass over, and will assume that the witness goes about the

world bearing stamped somehow on his face the appropriate

class to which he belongs, and consequently, the degree of

credit to which he has a claim on such general grounds.

But there are other and stronger reasons against the admissibility

of this class of problems.

6. That which has been described in the previous

sections as the 'individual' which had to be assigned to an

appropriate class or series of statistics is, of course, in this

case, \_a statement\_. In the particular instance in question this

individual statement is already assigned to a class, that

namely of statements made by a witness of a given degree of

veracity; but it is clearly optional with us whether or not we

choose to confine our attention to this class in forming our

judgment; at least it would be optional whenever we were

practically called on to form an opinion. But in the case of

this statement, as in that of the mortality of the man whose

insurance we were discussing, there are a multitude of other

properties observable, besides the one which is supposed to

mark the given class. Just as in the latter there were

(besides his age), the place of his birth, the nature of his

occupation, and so on; so in the former there are (besides its

being a statement by a certain kind of witness), the fact of its

being uttered at a certain time and place and under certain

circumstances. At the time the statement is made all these

qualities or attributes of the statement are present to us, and

we clearly have a right to take into account as many of them

as we please. Now the question at present before us seems to

be simply this;--Are the considerations, which we might thus

introduce, as immaterial to the result in the case of the truth

of a statement of a witness, as the corresponding considerations

are in the case of the insurance of a life? There can

surely be no hesitation in the reply to such a question.

Under ordinary circumstances we soon know all that we can

know about the conditions which determine us in judging of

the prospect of a man's death, and we therefore rest content

with general statistics of mortality; but no one who heard a

witness speak would think of simply appealing to his figure

of veracity, even supposing that this had been authoritatively

communicated to us. The circumstances under which the

statement is made instead of being insignificant, are of overwhelming

importance. The appearance of the witness, the

tone of his voice, the fact of his having objects to gain,

together with a countless multitude of other circumstances

which would gradually come to light as we reflect upon the

matter, would make any sensible man discard the assigned

average from his consideration. He would, in fact, no more

think of judging in this way than he would of appealing to

the Carlisle or Northampton tables of mortality to determine

the probable length of life of a soldier who was already in

the midst of a battle.

7. It cannot be replied that under these circumstances

we still refer the witness to a class, and judge of his veracity

by an average of a more limited kind; that we infer, for example,

that of men who look and act like him under such

circumstances, a much larger proportion, say nine-tenths,

are found to lie. There is no appeal to a class in this way at

all, there is no immediate reference to statistics of any kind

whatever; at least none which we are conscious of using at

the time, or to which we should think of resorting for justification

afterwards. The decision seems to depend upon the

quickness of the observer's senses and of his apprehension

generally.

Statistics about the veracity of witnesses seem in fact to

be permanently as inappropriate as all other statistics occasionally

may be. We may know accurately the percentage

of recoveries after amputation of the leg; but what surgeon

would think of forming his judgment solely by such tables

when he had a case before him? We need not deny, of

course, that the opinion he might form about the patient's

prospects of recovery might ultimately rest upon the proportions

of deaths and recoveries he might have previously witnessed.

But if this were the case, these data are lying, as

one may say, obscurely in the background. He does not

appeal to them directly and immediately in forming his

judgment. There has been a far more important intermediate

process of apprehension and estimation of what is

essential to the case and what is not. Sharp senses, memory,

judgment, and practical sagacity have had to be called into

play, and there is not therefore the same direct conscious

and sole appeal to statistics that there was before. The

surgeon may have in his mind two or three instances in

which the operation performed was equally severe, but in

which the patient's constitution was different; the latter

element therefore has to be properly allowed for. There may

be other instances in which the constitution was similar, but

the operation more severe; and so on. Hence, although the

ultimate appeal may be to the statistics, it is not so directly;

their value has to be estimated through the somewhat hazy

medium of our judgment and memory, which places them

under a very different aspect.

8. Any one who knows anything of the game of whist

may supply an apposite example of the distinction here

insisted on, by recalling to mind the alteration in the nature

of our inferences as the game progresses. At the commencement

of the game our sole appeal is rightfully made to the

theory of Probability. All the rules upon which each player

acts, and therefore upon which he infers that the others will

act, rest upon the observed frequency (or rather upon the

frequency which calculation assures us will be observed) with

which such and such combinations of cards are found to

occur. Why are we told, if we have more than four trumps,

to lead them out at once? Because we are convinced, on

pure grounds of probability, capable of being stated in the

strictest statistical form, that in a majority of instances we

shall draw our opponent's trumps, and therefore be left with

the command. Similarly with every other rule which is

recognized in the early part of the play.

But as the play progresses all this is changed, and

towards its conclusion there is but little reliance upon any

rules which either we or others could base upon statistical

frequency of occurrence, observed or inferred. A multitude

of other considerations have come in; we begin to be influenced

partly by our knowledge of the character and

practice of our partner and opponents; partly by a rapid

combination of a multitude of judgments, founded upon

our observation of the actual course of play, the grounds

of which we could hardly realize or describe at the time

and which may have been forgotten since. That is, the

particular combination of cards, now before us, does not

readily fall into any well-marked class to which alone it can

reasonably be referred by every one who has the facts before

him.

9. A criticism somewhat resembling the above has

been given by Mill (\_Logic,\_ Bk. III. Chap. XVIII. §3) upon

the applicability of the theory of Probability to the credibility

of witnesses. But he has added other reasons which

do not appear to me to be equally valid; he says "common

sense would dictate that it is impossible to strike a general

average of the veracity, and other qualifications for true

testimony, of mankind or any class of them; and if it were

possible, such an average would be no guide, the credibility of

almost every witness being either below or above the average,"

The latter objection would however apply with equal force

to estimating the length of a man's life from tables of mortality;

for the credibility of different witnesses can scarcely

have a wider range of variation than the length of different

lives. If statistics of credibility could be obtained, and

could be conveniently appealed to when they were obtained,

they might furnish us in the long run with as accurate

inferences as any other statistics of the same general description.

These statistics would however in practice naturally

and rightly be neglected, because there can hardly fail

to be circumstances in each individual statement which would

more appropriately refer it to some new class depending on

different statistics, and affording a far better chance of our

being right in that particular case. In most instances of

the kind in question, indeed, such a change is thus produced

in the mode of formation of our opinion, that, as already

pointed out, the mental operation ceases to be in any proper

sense founded on appeal to statistics.[1]

10. The Chance problems which are concerned with testimony

are not altogether confined to such instances as those

hitherto referred to. Though we must, as it appears to me,

reject all attempts to estimate the credibility of any particular

witness, or to refer him to any assigned class in

respect of his trustworthiness, and consequently abandon as

unsuitable any of the numerous problems which start from

such data as 'a witness who is wrong once in ten times,'

yet it does not follow that testimony may not to a slight

extent be treated by our science in a somewhat different

manner. We may be quite unable to estimate, except in

the roughest possible way, the veracity of any particular

witness, and yet it may be possible to form some kind of

opinion upon the veracity of certain classes of witnesses;

to say, for instance, that Europeans are superior in this way

to Orientals. So we might attempt to explain why, and to

what extent, an opinion in which the judgments of ten persons,

say jurors, concur, is superior to one in which five only

concur. Something may also be done towards laying down

the principles in accordance with which we are to decide

whether, and why, extraordinary stories deserve less credence

than ordinary ones, even if we cannot arrive at any precise

and definite decision upon the point. This last question is

further discussed in the course of the next chapter.

11. The change of view in accordance with which it

follows that questions of the kind just mentioned need not

be entirely rejected from scientific consideration, presents itself

in other directions also. It has, for instance, been already

pointed out that the individual characteristics of any sick

man's disease would be quite sufficiently important in most

cases to prevent any surgeon from judging about his recovery

by a genuine and direct appeal to statistics, however such

considerations might indirectly operate upon his judgment.

But if an opinion had to be formed about a considerable

number of cases, say in a large hospital, statistics might

again come prominently into play, and be rightly recognized

as the principal source of appeal. We should feel able to

compare one hospital, or one method of treatment, with

another. The ground of the difference is obvious. It arises

from the fact that the characteristics of the individuals,

which made us so ready to desert the average when we had

to judge of them separately, do not produce the same disturbance

when were have to judge about a group of cases.

The averages then become the most secure and available

ground on which to form an opinion, and therefore Probability

again becomes applicable.

But although some resort to Probability may be admitted

in such cases as these, it nevertheless does not

appear to me that they can ever be regarded as particularly

appropriate examples to illustrate the methods and resources

of the theory. Indeed it is scarcely possible to resist the

conviction that the refinements of mathematical calculation

have here been pushed to lengths utterly unjustifiable, when

we bear in mind the impossibility of obtaining any corresponding

degree of accuracy and precision in the data from

which we have to start. To cite but one instance. It would

be hard to find a case in which love of consistency has prevailed

over common sense to such an extent as in the admission

of the conclusion that it is unimportant what are

the numbers for and against a particular statement, provided

the actual majority is the same. That is, the unanimous

judgment of a jury of eight is to count for the same as a

majority of ten to two in a jury of twelve. And yet this

conclusion is admitted by Poisson. The assumptions under

which it follows will be indicated in the course of the next

chapter.

Again, perfect independence amongst the witnesses or

jurors is an almost necessary postulate. But where can this

be secured? To say nothing of direct collusion, human

beings are in almost all instances greatly under the influence

of sympathy in forming their opinions. This influence, under

the various names of political bias, class prejudice, local

feeling, and so on, always exists to a sufficient degree to

induce a cautious person to make many of those individual

corrections which we saw to be necessary when we were

estimating the trustworthiness, in any given case, of a single

witness; that is, they are sufficient to destroy much, if not

all, of the confidence with which we resort to statistics and

averages in forming our judgment. Since then this Essay is

mainly devoted to explaining and establishing the general

principles of the science of Probability, we may very fairly

be excused from any further treatment of this subject, beyond

the brief discussions which are given in the next chapter.

1. It may be remarked also that there is another reason which tends to

dissuade us from appealing to principles of Probability in the

majority of the cases where testimony has to be estimated. It often,

perhaps usually happens, that we are not absolutely forced to come

to a decision; at least so far as the acquitting of an accused

person may be considered as avoiding a decision. It may be of much

greater importance to us to attain not merely truth on the average,

but truth in each individual instance, so that we had rather not

form an opinion at all than form one of which we can only say in its

justification that it will tend to lead us right in the long run.

CHAPTER XVII.

\_ON THE CREDIBILITY OF EXTRAORDINARY STORIES.\_

1. It is now time to recur for fuller investigation to an

enquiry which has been already briefly touched upon more

than once; that is, the validity of testimony to establish,

as it is frequently expressed, an otherwise improbable story.

It will be remembered that in a previous chapter (the

twelfth) we devoted some examination to an assertion by

Butler, which seemed to be to some extent countenanced

by Mill, that a great improbability before the proof might

become but a very small improbability after the proof. In

opposition to this it was pointed out that the different

estimates which we undoubtedly formed of the credibility

of the examples adduced, had nothing to do with the

fact of the event being past or future, but arose from a

very different cause; that the conception of the event

which we entertain at the moment (which is all that is then

and there actually present to us, and as to the correctness

of which as a representation of facts we have to make up our

minds) comes before us in two very different ways. In one

instance it was a mere guess of our own which we knew

from statistics would be right in a certain proportion of

cases; in the other instance it was the assertion of a witness,

and therefore the appeal was not now primarily to statistics of

the event, but to the trustworthiness of the witness. The conception,

or 'event' if we will so term it, had in fact passed

out of the category of guesses (on statistical grounds), into

that of assertions (most likely resting on some specific evidence),

and would therefore be naturally regarded in a very

different light.

2. But it may seem as if this principle would lead us

to somewhat startling conclusions. For, by transferring the

appeal from the frequency with which the event occurs to

the trustworthiness of the witness who makes the assertion,

is it not implied that the probability or improbability of an

assertion depends solely upon the veracity of the witness?

If so, ought not any story whatever to be believed when

it is asserted by a truthful person?

In order to settle this question we must look a little

more closely into the circumstances under which such

testimony is commonly presented to us. As it is of course

necessary, for clearness of exposition, to take a numerical

example, let us suppose that a given statement is made by

a witness who, on the whole and in the long run, is right

in what he says nine times out of ten.[1] Here then is an

average given to us, an average veracity that is, which

includes all the particular statements which the witness has

made or will make.

3. Now it has been abundantly shown in a former

chapter (Ch. IX. §§14-32) that the mere fact of a particular

average having been assigned, is no reason for our

being forced invariably to adhere to it, even in those cases

in which our most natural and appropriate ground of judgment

is found in an appeal to statistics and averages.

The general average may constantly have to be corrected

in order to meet more accurately the circumstances of particular

cases. In statistics of mortality, for instance; instead

of resorting to the wider tables furnished by people in

general of a given age, we often prefer the narrower tables

furnished by men of a particular profession, abode, or mode

of life. The reader may however be conveniently reminded

here that in so doing we must not suppose that we are able,

by any such device, in any special or peculiar way to secure

truth. The general average, if persistently adhered to

throughout a sufficiently wide and varied experience, would

in the long run tend to give us the truth; all the advantage

which the more special averages can secure for us is to give

us the same tendency to the truth with fewer and slighter

aberrations.

4. Returning then to our witness, we know that if

we have a very great many statements from him upon all

possible subjects, we may feel convinced that in nine out of

ten of these he will tell us the truth, and that in the tenth

case he will go wrong. This is nothing more than a matter

of definition or consistency. But cannot we do better than

thus rely upon his general average? Cannot we, in almost

any given case, specialize it by attending to various characteristic

circumstances in the nature of the statement

which he makes; just as we specialize his prospects of

mortality by attending to circumstances in his constitution

or mode of life?

Undoubtedly we may do this; and in any of the practical

contingencies of life, supposing that we were at all guided

by considerations of this nature, we should act very foolishly

if we did not adopt some such plan. Two methods of thus

correcting the average may be suggested: one of them being

that which practical sagacity would be most likely to employ,

the other that which is almost universally adopted by writers

on Probability. The former attempts to make the correction

by the following considerations: instead of relying upon the

witness' general average, we assign to it a sort of conjectural

correction to meet the case before us, founded on our experience

or observation; that is, we appeal to experience to

establish that stories of such and such a kind are more or

less likely to be true, as the case may be, than stories in

general. The other proceeds upon a different and somewhat

more methodical plan. It is here endeavoured to

show, by an analysis of the nature and number of the

sources of error in the cases in question, that such and such

kinds of stories must be more or less likely to be correctly

reported, and this in certain numerical proportions.

5. Before proceeding to a discussion of these methods

a distinction must be pointed out to which writers upon the

subject have not always attended, or at any rate to which

they have not generally sufficiently directed their readers'

attention.[2] There are, broadly speaking, two different ways

in which we may suppose testimony to be given. It may, in

the first place, take the form of a reply to an alternative

question, a question, that is, framed to be answered by \_yes\_

or \_no\_. Here, of course, the possible answers are mutually

contradictory, so that if one of them is not correct the other

must be so:--Has A happened, yes or no? The common

mode of illustrating this kind of testimony numerically is by

supposing a lottery with a prize and blanks, or a bag of balls

of two colours only, the witness knowing that there are only

two, or at any rate being confined to naming one or other of

them. If they are black and white, and he errs when black

is drawn, he must say 'white,' The reason for the prominence

assigned to examples of this class is, probably, that

they correspond to the very important case of verdicts of

juries; juries being supposed to have nothing else to do than

to say 'guilty' or 'not guilty.'

On the other hand, the testimony may take the form of a

more original statement or piece of information. Instead of

saying, Did A happen? we may ask, What happened? Here

if the witness speaks truth he must be supposed, as before,

to have but one way of doing so; for the occurrence of some

specific event was of course contemplated. But if he errs he

has many ways of going wrong, possibly an infinite number.

Ordinarily however his possible false statements are assumed

to be limited in number, as must generally be more or less

the result in practice. This case is represented numerically

by supposing the balls in the bag not to be of two colours

only, but to be all distinct from each other; say by their

being all numbered successively. It may of course be objected

that a large number of the statements that are made

in the world are not in any way answers to questions, either

of the alternative or of the open kind. For instance, a man

simply asserts that he has drawn the seven of spades from a

pack of cards; and we do not know perhaps whether he had

been asked 'Has that card been drawn?' or 'What card has

been drawn?' or indeed whether he had been asked anything

at all. Still more might this be so in the case of any ordinary

historical statement.

This objection is quite to the point, and must be recognized

as constituting an additional difficulty. All that we

can do is to endeavour, as best we may, to ascertain, from

the circumstances of the case, what number of alternatives

the witness may be supposed to have had before him. When

he simply testifies to some matter well known to be in dispute,

and does not go much into detail, we may fairly consider

that there were practically only the two alternatives

before him of saying 'yes' or 'no.' When, on the other hand,

he tells a story of a more original kind, or (what comes to

much the same thing) goes into details, we must regard him

as having a wide comparative range of alternatives before

him.

These two classes of examples, viz. that of the black and

white balls, in which only one form of error is possible, and

the numbered balls, in which there may be many forms of

error, are the only two which we need notice. In practice it

would seem that they may gradually merge into each other,

according to the varying ways in which we choose to frame

our question. Besides asking, Did you see A strike B? and,

What did you see? we may introduce any number of intermediate

leading questions, as, What did A do? What did

he do to B? and so on. In this way we may gradually narrow

the possible openings to wrong statement, and so approach

to the direct alternative question. But it is clear that all

these cases may be represented numerically by a supposed

diminution in the number of the balls which are thus distinguished

from each other.

6. Of the two plans mentioned in §4 we will begin

with the latter, as it is the only methodical and scientific one

which has been proposed. Suppose that there is a bag with

1000 balls, only one of which is white, the rest being all

black. A ball is drawn at random, and our witness whose

veracity is 9/10 reports that the white ball was drawn. Take

a great many of his statements upon this particular subject,

say 10,000; that is, suppose that 10,000 balls having been

successively drawn out of this bag, or bags of exactly the

same kind, he makes his report in each case. His 10,000

statements being taken as a fair sample of his general average,

we shall find, by supposition, that 9 out of every 10 of

them are true and the remaining one false. What will be

the nature of these false statements? Under the circumstances

in question, he having only one way of going wrong,

the answer is easy. In the 10,000 drawings the white ball

would come out 10 times, and therefore be rightly asserted

9 times, whilst on the one of these occasions on which he

goes wrong he has nothing to say but 'black.' So with the

9990 occasions on which black is drawn; he is right and

says black on 8991 of them, and is wrong and therefore says

white on 999 of them. On the whole, therefore, we conclude

that out of every 1008 times on which he says that white is

drawn he is wrong 999 times and right only 9 times. That

is, his special veracity, as we may term it, for cases of this

description, has been reduced from 9/10 to 9/1008. As it would

commonly be expressed, the latter fraction represents the

chance that this particular statement of his is true.[3]

7. We will now take the case in which the witness

has many ways of going wrong, instead of merely one. Suppose

that the balls were all numbered, from 1 to 1,000, and

the witness knows this fact. A ball is drawn, and he tells

me that it was numbered 25, what are the odds that he is

right? Proceeding as before, in 10,000 drawings this ball

would be obtained 10 times, and correctly named 9 times.

But on the 9990 occasions on which it was not drawn there

would be a difference, for the witness has now many openings

for error before him. It is, however, generally considered

reasonable to assume that his errors will all take the form of

announcing wrong numbers; and that, there being no apparent

reason why he should choose one number rather than another,

he will be likely to announce all the wrong ones equally

often. Hence his 999 errors, instead of all leading him now

back again to one spot, will be uniformly spread over as

many distinct ways of going wrong. On one only of these

occasions, therefore, will he mention 25 as having been

drawn. It follows therefore that out of every 10 times that

he names 25 he is right 9 times; so that in this case his

average or general truthfulness applies equally well to the

special case in point.

8. With regard to the truth of these conclusions, it

must of course be admitted that if we grant the validity of

the assumptions about the limits within which the blundering

or mendacity of the witness are confined, and the complete

impartiality with which his answers are disposed within

those limits, the reasoning is perfectly sound. But are not

these assumptions extremely arbitrary, that is, are not our

lotteries and bags of balls rendered perfectly precise in many

respects in which, in ordinary life, the conditions supposed to

correspond to them are so vague and uncertain that no such

method of reasoning becomes practically available? Suppose

that a person whom I have long known, and of whose measure

of veracity and judgment I may be supposed therefore

to have acquired some knowledge, informs me that there is

something to my advantage if I choose to go to certain

trouble or expense in order to secure it. As regards the

general veracity of the witness, then, there is no difficulty;

we suppose that this is determined for us. But as regards

his story, difficulty and vagueness emerge at every point.

What is the number of balls in the bag here? What in fact

are the nature and contents of the bag out of which we suppose

the drawing to have been made? It does not seem

that the materials for any rational judgment exist here.

But if we are to get at any such amended figure of veracity

as those attained in the above example, these questions must

necessarily be answered with some degree of accuracy; for

the main point of the method consists in determining how

often the event must be considered \_not\_ to happen, and thence

inferring how often the witness will be led wrongly to assert

that it has happened.

It is not of course denied that considerations of the kind

in question have some influence upon our decision, but only

that this influence could under any ordinary circumstances

be submitted to numerical determination. We are doubtless

liable to have information given to us that we have

come in for some kind of fortune, for instance, when no

such good luck has really befallen us; and this not once

only but repeatedly. But who can give the faintest intimation

of the nature and number of the occasions on which,

a blank being thus really drawn, a prize will nevertheless

be falsely announced? It appears to me therefore that

numerical results of any practical value can seldom, if ever,

be looked for from this method of procedure.

9. Our conclusion in the case of the lottery, or, what

comes to the same thing, in the case of the bag with black

and white balls, has been questioned or objected to[4] on the

ground that it is contrary to all experience to suppose that

the testimony of a moderately good witness could be so

enormously depreciated under such circumstances. I should

prefer to base the objection on the ground that experience

scarcely ever presents such circumstances as those supposed;

but if we postulate their existence the given conclusion seems

correct enough. Assume that a man is merely required to

say \_yes\_ or \_no\_; assume also a group or succession of cases in

which \_no\_ should rightly be said very much oftener than

\_yes\_. Then, assuming almost any general truthfulness of the

witness, we may easily suppose the rightful occasions for

denial to be so much the more frequent that a majority of

his affirmative answers will actually occur as false 'noes'

rather than as correct 'ayes.' This of course lowers the

average value of his 'ayes,' and renders them comparatively

untrustworthy.

Consider the following example. I have a gardener whom

I trust as to all ordinary matters of fact. If he were to

tell me some morning that my dog had run away I should

fully believe him. He tells me however that the dog has

gone mad. Surely I should accept the statement with much

hesitation, and on the grounds indicated above. It is not

that he is more likely to be wrong when the dog \_is\_ mad;

but that experience shows that there are other complaints

(e.g. fits) which are far more common than madness, and

that most of the assertions of madness are erroneous assertions

referring to these. This seems a somewhat parallel

case to that in which we find that most of the assertions

that a white ball had been drawn are really false assertions

referring to the drawing of a black ball. Practically I do

not think that any one would feel a difficulty in thus exorbitantly

discounting some particular assertion of a witness

whom in most other respects he fully trusted.

10. There is one particular case which has been regarded

as a difficulty in the way of this treatment of the

problem, but which seems to me to be a decided confirmation

of it; always, be it understood, within the very narrow

and artificial limits to which we must suppose ourselves to

be confined. This is the case of a witness whose veracity is

just one-half; that is, one who, when a mere \_yes\_ or \_no\_ is

demanded of him, is as often wrong as right. In the case of

any other assigned degree of veracity it is extremely difficult

to get anything approaching to a confirmation from practical

judgment and experience. We are not accustomed to

estimate the merits of witnesses in this way, and hardly appreciate

what is meant by his numerical degree of truthfulness.

But as regards the man whose veracity is one-half, we

are (as Mr C. J. Monro has very ingeniously suggested) only

too well acquainted with such witnesses, though under a

somewhat different name; for this is really nothing else than

the case of a person confidently answering a question about

a subject-matter of which he knows nothing, and can therefore

only give a mere guess.

Now in the case of the lottery with one prize, when the

witness whose veracity is one-half tells us that we have

gained the prize, we find on calculation that his testimony

goes for absolutely nothing; the chances that we have got

the prize are just the same as they would be if he had never

opened his lips, viz. 1/1000. But clearly this is what ought

to be the result, for the witness who knows nothing about

the matter leaves it exactly as he found it. He is indeed,

in strictness, scarcely a witness at all; for the natural function

of a witness is to examine the matter, and so to add

confirmation, more or less, according to his judgment and

probity, but at any rate to offer an improvement upon the

mere guesser. If, however, we will give heed to his mere

guess we are doing just the same thing as if we were to guess

ourselves, in which case of course the odds that we are right

are simply measured by the frequency of occurrence of the

events.

We cannot quite so readily apply the same rule to the

other case, namely to that of the numbered balls, for there

the witness who is right every other time may really be a

very fair, or even excellent, witness. If he has many ways

of going wrong, and yet is right in half his statements, it is

clear that he must have taken some degree of care, and cannot

have merely guessed. In a case of \_yes\_ or \_no\_, any one can

be right every other time, but it is different where truth

is single and error is manifold. To represent the case of a

simply worthless witness when there were 1000 balls and

the drawing of one assigned ball was in question, we should

have to put his figure of veracity at 1/1000. If this were

done we should of course get a similar result.

11. It deserves notice therefore that the figure of

veracity, or fraction representing the general truthfulness

of a witness, is in a way relative, not absolute; that is, it

depends upon, and varies with, the general character of the

answer which he is supposed to give. Two witnesses of

equal intrinsic veracity and worth, one of whom confined

himself to saying \_yes\_ and \_no\_, whilst the other ventured to

make more original assertions, would be represented by

different fractions; the former having set himself a much

easier task than the latter. The real caution and truthfulness

of the witness are only one factor, therefore, in his

actual figure of veracity; the other factor consists of the

nature of his assertions, as just pointed out. The ordinary

plan therefore, in such problems, of assigning an average

truthfulness to the witness, and accepting this alike in the

case of each of the two kinds of answers, though convenient,

seems scarcely sound. This consideration would however

be of much more importance were not the discussions upon

the subject mainly concerned with only one description of

answer, namely that of the 'yes or no' kind.

12. So much for the methodical way of treating such

a problem. The way in which it would be taken in hand by

those who had made no study of Probability is very different.

It would, I apprehend, strike them as follows. They would

say to themselves, Here is a story related by a witness who

tells the truth, say, nine times out of ten. But it is a story

of a kind which experience shows to be very generally made

untruly, say 99 times out of 100. Having then these opposite

inducements to belief, they would attempt in some way

to strike a balance between them. Nothing in the nature of

a strict rule could be given to enable them to decide how

they might escape out of the difficulty. Probably, in so far

as they did not judge at haphazard, they would be guided

by still further resort to experience, or unconscious recollections

of its previous teachings, in order to settle which

of the two opposing inductions was better entitled to carry

the day in the particular case before them. The reader

will readily see that any general solution of the problem,

when thus presented, is impossible. It is simply the now

familiar case (Chap. IX. §§14-32) of an individual which

belongs equally to two distinct, or even, in respect of their

characteristics, opposing classes. We cannot decide off-hand

to which of the two its characteristics most naturally and

rightly refer it. A fresh induction is needed in order to

settle this point.

13. Rules have indeed been suggested by various

writers in order to extricate us from the difficulty. The controversy

about miracles has probably been the most fertile

occasion for suggestions of this kind on one side or the

other. It is to this controversy, presumably, that the phrase

is due, so often employed in discussions upon similar subjects,

'a contest of opposite improbabilities.' What is meant

by such an expression is clearly this: that in forming a

judgment upon the truth of certain assertions we may find

that they are comprised in two very distinct classes, so that,

according as we regarded them as belonging to one or the

other of these distinct classes, our opinion as to their truth

would be very different. Such an assertion belongs to one

class, of course, by its being a statement of a particular

witness, or kind of witness; it belongs to the other by its

being a particular kind of story, one of what is called an

improbable nature. Its belonging to the former class is so

far favourable to its truth, its belonging to the latter is so far

hostile to its truth. It seems to be assumed, in speaking of

a contest of opposite improbabilities, that when these different

sources of conviction co-exist together, they would each in

some way retain their probative force so as to produce a

contest, ending generally in a victory to one or other of

them. Hume, for instance, speaks of our \_deducting\_ one

probability from the other, and apportioning our belief to

the remainder.[5] Thomson, in his \_Laws of Thought\_, speaks

of one probability as entirely superseding the other.

14. It does not appear to me that the slightest philosophical

value can be attached to any such rules as these.

They doubtless may, and indeed will, hold in individual

cases, but they cannot lay claim to any generality. Even

the notion of a contest, as any necessary ingredient in the

case, must be laid aside. For let us refer again to the way

in which the perplexity arises, and we shall readily see, as

has just been remarked, that it is nothing more than a particular

exemplification of a difficulty which has already been

recognized as incapable of solution by any general \_à priori\_

method of treatment. All that we are supposed to have before

us is a statement. On this occasion it is made by a witness

who lies, say, once in ten times in the long run; that is, who

mostly tells the truth. But on the other hand, it is a statement

which experience, derived from a variety of witnesses on

various occasions, assures us is mostly false; stated numerically

it is found, let us suppose, to be false 99 times in a hundred.

Now, as was shown in the chapter on Induction, we are

thus brought to a complete dead lock. Our science offers no

principles by which we can form an opinion, or attempt to

decide the matter one way or the other; for, as we found,

there are an indefinite number of conclusions which are all

equally possible. For instance, all the witness' extraordinary

assertions may be true, or they may all be false, or they may

be divided into the true and the false in any proportion

whatever. Having gone so far in our appeal to statistics as

to recognize that the witness is generally right, but that his

story is generally false, we cannot stop there. We ought to

make still further appeal to experience, and ascertain how it

stands with regard to his stories when they are of that

particular nature: or rather, for this would be to make a

needlessly narrow reference, how it stands with regard to

stories of that kind when advanced by witnesses of his

general character, position, sympathies, and so on.[6]

15. That extraordinary stories are in many cases, probably

in a great majority of cases, less trustworthy than

others must be fully admitted. That is, if we were to make

two distinct classes of such stories respectively, we should

find that the same witness, or similar witnesses, were proportionally

more often wrong when asserting the former than

when asserting the latter. But it does not by any means

appear to me that this must always be the case. We may

well conceive, for instance, that with some people the mere

fact of the story being of a very unusual character may make

them more careful in what they state, so as actually to add

to their veracity. If this were so we might be ready to

accept their extraordinary stories with even more readiness

than their ordinary ones.

Such a supposition as that just made does not seem to me

by any means forced. Put such a case as this: let us suppose

that two persons, one of them a man of merely ordinary

probity and intelligence, the other a scientific naturalist,

make a statement about some common event. We believe

them both. Let them now each report some extraordinary

\_lusus naturæ\_ or monstrosity which they profess to have seen.

Most persons, we may presume, would receive the statement

of the naturalist in this latter case almost as readily as in

the former: whereas when the same story came from the

unscientific observer it would be received with considerable

hesitation. Whence arises the difference? From the conviction

that the naturalist will be far more careful, and

therefore to the full as accurate, in matters of this kind as

in those of the most ordinary description, whereas with the

other man we feel by no means the same confidence. Even

if any one is not prepared to go this length, he will probably

admit that the difference of credit which he would attach to

the two kinds of story, respectively, when they came from

the naturalist, would be much less than what it would be

when they came from the other man.

16. Whilst we are on this part of the subject, it must

be pointed out that there is considerable ambiguity and

consequent confusion about the use of the term 'an extraordinary

story.' Within the province of pure Probability it

ought to mean simply a story which asserts an \_unusual\_ event.

At least this is the view which has been adopted and maintained,

it is hoped consistently, throughout this work. So

long as we adhere to this sense we know precisely what we

mean by the term. It has a purely objective reference; it

simply connotes a very low degree of relative statistical

frequency, actual or prospective. Out of a great number of

events we suppose a selection of some particular kind to be

contemplated, which occurs relatively very seldom, and this

is termed an unusual or extraordinary event. It follows, as

was abundantly shown in a former chapter, that owing to

the rarity of the event we are very little disposed to expect

its occurrence in any given case. Our guess about it, in case

we thus anticipated it, would very seldom be justified, and

we are therefore apt to be much surprised when it does

occur. This, I take it, is the only legitimate sense of 'extraordinary'

so far as Probability is concerned.

But there is another and very different use of the word,

which belongs to Induction, or rather to the science of

evidence in general, more than to that limited portion of it

termed Probability. In this sense the 'extraordinary,' and

still more the 'improbable,' event is not merely one of

extreme statistical rarity, which we could not expect to

guess aright, but which on moderate evidence we may pretty

readily accept; it is rather one which possesses, so to say, an

actual evidence-resisting power. It may be something which

affects the credibility of the witness at the fountain-head,

which makes, that is, his statements upon such a subject

essentially inferior to those on other subjects. This is the

case, for instance, with anything which excites his prejudices

or passions or superstitions. In these cases it would seem

unreasonable to attempt to estimate the credibility of the

witness by calculating (as in §6) how often his errors would

mislead us through his having been wrongly brought to an

affirmation instead of adhering correctly to a negation. We

should rather be disposed to put our correction on the witness'

average veracity at once.

17. In true Probability, as has just been remarked,

every event has its own definitely recognizable degree of

frequency of occurrence. It may be excessively rare, rare to

any extreme we like to postulate, but still every one who

understands and admits the data upon which its occurrence

depends will be able to appreciate within what range of

experience it may be expected to present itself. We do not

expect it in any individual case, nor within any brief range,

but we do confidently expect it within an adequately extensive

range. How therefore can miraculous stories be similarly

taken account of, when the disputants, on one side at

least, are not prepared to admit their actual occurrence anywhere

or at any time? How can any arrangement of bags

and balls, or other mechanical or numerical illustrations of

unlikely events, be admitted as fairly illustrative of miraculous

occurrences, or indeed of many of those which come

under the designation of 'very extraordinary' or 'highly

improbable'? Those who contest the occurrence of a particular

miracle, as reported by this or that narrator, do not

admit that miracles are to be confidently expected sooner or

later. It is not a question as to whether what must happen

sometimes has happened some particular time, and therefore

no illustration of the kind can be regarded as apposite.

How unsuitable these merely rare events, however excessive

their rarity may be, are as examples of miraculous

events, will be evident from a single consideration. No one,

I presume, who admitted the occasional occurrence of an exceedingly

unusual combination, would be in much doubt if

he considered that he had actually seen it himself.[7] On the

other hand, few men of any really scientific turn would

readily accept a miracle even if it appeared to happen under

their very eyes. They might be staggered at the time, but

they would probably soon come to discredit it afterwards,

or so explain it as to evacuate it of all that is meant by

miraculous.

18. It appears to me therefore, on the whole, that

very little can be made of these problems of testimony in

the way in which it is generally intended that they should

be treated; that is, in obtaining specific rules for the estimation

of the testimony under any given circumstances.

Assuming that the veracity of the witness can be measured,

we encounter the real difficulty in the utter impossibility of

determining the limits within which the failures of the event

in question are to be considered to lie, and the degree of

explicitness with which the witness is supposed to answer

the enquiry addressed to him; both of these being characteristics

of which it is necessary to have a numerical estimate

before we can consider ourselves in possession of the

requisite data.

Since therefore the practical resource of most persons,

viz. that of putting a direct and immediate correction, of

course of a somewhat conjectural nature, upon the general

trustworthiness of the witness, by a consideration of the

nature of the circumstances under which his statement is

made, is essentially unscientific and irreducible to rule; it

really seems to me that there is something to be said in

favour of the simple plan of trusting in all cases alike to

the witness' general veracity.[8] That is, whether his story

is ordinary or extraordinary, we may resolve to put it on

the same footing of credibility, provided of course that the

event is fully recognized as one which does or may occasionally

happen. It is true that we shall thus go constantly

astray, and may do so to a great extent, so that if there

were any rational and precise method of specializing his

trustworthiness, according to the nature of his story, we

should be on much firmer ground. But at least we may

thus know what to expect on the average. Provided we

have a sufficient number and variety of statements from

him, and always take them at the same constant rate or

degree of trustworthiness, we may succeed in balancing and

correcting our conduct in the long run so as to avoid any

ruinous error.

19. A few words may now be added about the combination

of testimony. No new principles are introduced here,

though the consequent complication is naturally greater. Let

us suppose two witnesses, the veracity of each being 9/10.

Now suppose 100 statements made by the pair; according to

the plan of proceeding adopted before, we should have them

both right 81 times and both wrong once, in the remaining

18 cases one being right and the other wrong. But since

they are both supposed to give the same account, what we

have to compare together are the number of occasions on

which they agree and are right, and the total number on which

they agree whether right or wrong. The ratio of the former

to the latter is the fraction which expresses the trustworthiness

of their combination of testimony in the case in question.

In attempting to decide this point the only difficulty is

in determining how often they will be found to agree when

they are both wrong, for clearly they must agree when they

are both right. This enquiry turns of course upon the number

of ways in which they can succeed in going wrong.

Suppose first the case of a simple \_yes\_ or \_no\_ (as in §6), and

take the same example, of a bag with 1000 balls, in which

one only is white. Proceeding as before, we should find that

out of 100,000 drawings (the number required in order to

obtain a complete cycle of all possible occurrences, as well as

of all possible reports about them) the two witnesses agree

in a correct report of the appearance of white in 81, and

agree in a wrong report of it in 999. The Probability therefore

of the story when so attested is 81/1080; the fact therefore

of two such witnesses of equal veracity having concurred

makes the report nearly 9 times as likely as when it rested

upon the authority of only one of them.[9]

20. When however the witnesses have many ways of

going wrong, the fact of their agreeing makes the report far

more likely to be true. For instance, in the case of the 1000

numbered balls, it is very unlikely that when they both mistake

the number they should (without collusion) happen to

make the same misstatement. Whereas, in the last case,

every combined misstatement necessarily led them both to

the assertion that the event in question had happened, we

should now find that only once in 999 × 999 times would

they both be led to assert that \_some given number\_ (say, as

before, 25) had been drawn. The odds in favour of the

event in fact now become 80919/80920, which are enormously

greater than when there was only one witness.

It appears therefore that when two, and of course still

more when many, witnesses agree in a statement in a matter

about which they might make many and various errors, the

combination of their favourable testimony adds enormously

to the likelihood of the event; provided always that there

is no chance of collusion. And in the extreme case of the

opportunities for error being, as they well may be, practically

infinite in number, such combination would produce almost

perfect certainty. But then this condition, viz. absence of

collusion, very seldom can be secured. Practically our main

source of error and suspicion is in the possible existence of

some kind of collusion. Since we can seldom entirely get

rid of this danger, and when it exists it can never be submitted

to numerical calculation, it appears to me that combination

of testimony, in regard to detailed accounts, is yet

more unfitted for consideration in Probability than even that

of single testimony.

21. The impossibility of any adequate or even appropriate

consideration of the credibility of miraculous stories

by the rules of Probability has been already noticed in §17.

But, since the grounds of this impossibility are often very

insufficiently appreciated, a few pages may conveniently be

added here with a view to enforcing this point. If it be

regarded as a digression, the importance of the subject and

the persistency with which various writers have at one time

or another attempted to treat it by the rules of our science

must be the excuse for entering upon it.

A necessary preliminary will be to decide upon some definition

of a miracle. It will, we may suppose, be admitted by

most persons that in calling a miracle 'a suspension of a law

of causation,' we are giving what, though it may not amount

to an adequate definition, is at least true as a description.

It is true, though it may not be the whole truth. Whatever

else the miracle may be, this is its physical aspect: this is the

point at which it comes into contact with the subject-matter

of science. If it were not considered that any suspension of

causation were involved, the event would be regarded merely

as an ordinary one to which some special significance was

attached, that is, as a type or symbol rather than a miracle.

It is this aspect moreover of the miracle which is now exposed

to the main brunt of the attack, and in support of

which therefore the defence has generally been carried on.

Now it is obvious that this, like most other definitions or

descriptions, makes some assumption as to matters of fact,

and involves something of a theory. The assumption clearly

is, that laws of causation prevail universally, or almost universally,

throughout nature, so that infractions of them are

marked and exceptional. This assumption is made, but it

does not appear that anything more than this is necessarily

required; that is, there is nothing which need necessarily

make us side with either of the two principal schools which

are divided as to the nature of these laws of causation. The

definition will serve equally well whether we understand by

\_law\_ nothing more than uniformity of antecedent and consequent,

or whether we assert that there is some deeper and

more mysterious tie between the events than mere sequence.

The use of the term 'causation' in this minimum of signification

is common to both schools, though the one might

consider it inadequate; we may speak, therefore, of 'suspensions

of causation' without committing ourselves to either.

22. It should be observed that the aspect of the question

suggested by this definition is one from which we can

hardly escape. Attempts indeed have been sometimes made

to avoid the necessity of any assumption as to the universal

prevalence of law and order in nature, by defining a miracle

from a different point of view. A miracle may be called, for

instance, 'an immediate exertion of creative power,' 'a sign

of a revelation,' or, still more vaguely, an 'extraordinary

event.' But nothing would be gained by adopting any such

definitions as these. However they might satisfy the theologian,

the student of physical science would not rest content

with them for a moment. He would at once assert his own

belief, and that of other scientific men, in the existence of

universal law, and enquire what was the connection of the

definition with this doctrine. An answer would imperatively

be demanded to the question, Does the miracle, as you have

described it, imply an infraction of one of these laws, or does

it not? And an answer must be given, unless indeed we

reject his assumption by denying our belief in the existence

of this universal law, in which case of course we put ourselves

out of the pale of argument with him. The necessity

of having to recognize this fact is growing upon men day by

day, with the increased study of physical science. And since

this aspect of the question has to be met some time or other,

it is as well to place it in the front. The difficulty, in its

scientific form, is of course a modern one, for the doctrine out

of which it arises is modern. But it is only one instance, out

of many that might be mentioned, in which the growth of

some philosophical conception has gradually affected the

nature of the dispute, and at last shifted the position of the

battle-ground, in some discussion with which it might not at

first have appeared to have any connection whatever.

23. So far our path is plain. Up to this point disciples

of very different schools may advance together; for in laying

down the above doctrine we have carefully abstained from

implying or admitting that it contains the whole truth. But

from this point two paths branch out before us, paths as

different from each other in their character, origin, and

direction, as can well be conceived. As this enquiry is only

a digression, we may confine ourselves to stating briefly what

seem to be the characteristics of each, without attempting to

give the arguments which might be used in their support.

(I.) On the one hand, we may assume that this principle

of causation is the ultimate one. By so terming it, we do

not mean that it is one from which we consciously start in

our investigations, as we do from the axioms of geometry,

but rather that it is the final result towards which we find

ourselves drawn by a study of nature. Finding that,

throughout the scope of our enquiries, event follows event in

never-failing uniformity, and finding moreover (some might

add) that this experience is supported or even demanded by

a tendency or law of our nature (it does not matter here how

we describe it), we may come to regard this as the one

fundamental principle on which all our enquiries should rest.

(II.) Or, on the other hand, we may admit a class of

principles of a very different kind. Allowing that there is

this uniformity so far as our experience extends, we may yet

admit what can hardly be otherwise described than by

calling it a Superintending Providence, that is, a Scheme or

Order, in reference to which Design may be predicated

without using merely metaphorical language. To adopt an

aptly chosen distinction, it is not to be understood as \_over-ruling\_

events, but rather as \_underlying\_ them.

24. Now it is quite clear that according as we come to

the discussion of any particular miracle or extraordinary

story under one or other of these prepossessions, the question

of its credibility will assume a very different aspect. It

is sometimes overlooked that although a difference about

\_facts\_ is one of the conditions of a \_bonâ fide\_ argument, a difference

which reaches to ultimate principles is fatal to all

argument. The possibility of present conflict is banished in

such a case as absolutely as that of future concord. A large

amount of popular literature on the subject of miracles

seems to labour under this defect. Arguments are stated

and examined for and against the credibility of miraculous

stories without the disputants appearing to have any

adequate conception of the chasm which separates one side

from the other.

25. The following illustration may serve in some

degree to show the sort of inconsistency of which we are

speaking. A sailor reports that in some remote coral island

of the Pacific, on which he had landed by himself, he had

found a number of stones on the beach disposed in the exact

form of a cross. Now if we conceive a debate to arise about

the truth of his story, in which it is attempted to decide the

matter simply by considerations about the validity of testimony,

without introducing the question of the existence of

inhabitants, and the nature of their customs, we shall have

some notion of the unsatisfactory nature of many of the

current arguments about miracles. All illustrations of this

subject are imperfect, but a case like this, in which a supposed

trace of human agency is detected interfering with the

orderly sequence of other and non-intelligent natural causes,

is as much to the point as any illustration can be. The

thing omitted here from the discussion is clearly the one important

thing. If we suppose that there is no inhabitant, we

shall probably disbelieve the story, or consider it to be

grossly exaggerated. If we suppose that there are inhabitants,

the question is at once resolved into a different and

somewhat more intricate one. The credibility of the witness

is not the only element, but we should necessarily have to

take into consideration the character of the supposed inhabitants,

and the object of such an action on their part.

26. Considerations of this character are doubtless

often introduced into the discussion, but it appears to me

that they are introduced to a very inadequate extent. It is

often urged, after Paley, 'Once believe in a God, and miracles

are not incredible.' Such an admission surely demands

some modification and extension. It should rather be stated

thus, Believe in a God whose working may be traced throughout

the whole moral and physical world. It amounts, in

fact, to this;--Admit that there may be a \_design\_ which we

can trace somehow or other in the course of things; admit

that we are not wholly confined to tracing the connection of

events, or following out their effects, but that we can form

some idea, feeble and imperfect though it be, of a \_scheme\_.[10]

Paley's advice sounds too much like saying, Admit that there

are fairies, and we can account for our cups being cracked.

The admission is not to be made in so off-hand a manner.

To any one labouring under the difficulty we are speaking

of, this belief in a God almost out of any constant relation to

nature, whom we then imagine to occasionally manifest himself

in a perhaps irregular manner, is altogether impossible.

The only form under which belief in the Deity can gain entrance

into his mind is as the controlling Spirit of an infinite

and orderly system. In fact, it appears to me, paradoxical

as the suggestion may appear, that it might even be more

easy for a person thoroughly imbued with the spirit of Inductive

science, though an atheist, to believe in a miracle

which formed a part of a vast system, than for such a person,

as a theist, to accept an isolated miracle.

27. It is therefore with great prudence that Hume,

and others after him, have practically insisted on commencing

with a discussion of the credibility of the single miracle,

treating the question as though the Christian Revelation

could be adequately regarded as a succession of such events.

As well might one consider the living body to be represented

by the aggregate of the limbs which compose it. What is to

be complained of in so many popular discussions on the subject

is the entire absence of any recognition of the different

ground on which the attackers and defenders of miracles are

so often really standing. Proofs and illustrations are produced

in endless number, which involving, as they almost all

do in the mind of the disputants on one side at least, that

very principle of causation, the absence of which in the case

in question they are intended to establish, they fail in the

single essential point. To attempt to induce any one to disbelieve

in the existence of physical causation, in a given

instance, by means of illustrations which to him seem only

additional examples of the principle in question, is like trying

to make a dam, in order to stop the flow of a river, by

shovelling in snow. Such illustrations are plentiful in times

of controversy, but being in reality only modified forms of

that which they are applied to counteract, they change their

shape at their first contact with the disbeliever's mind, and

only help to swell the flood which they were intended to

check.

1. Reasons were given in the last chapter against the propriety of

applying the rules of Probability with any strictness to such

examples as these. But although all approach to numerical accuracy

is unattainable, we do undoubtedly recognize in ordinary life a

distinction between the credibility of one witness and another; such

a rough practical distinction will be quite sufficient for the

purposes of this chapter. For convenience, and to illustrate the

theory, the examples are best stated in a numerical form, but it is

not intended thereby to imply that any such accuracy is really

attainable in practice.

2. I must plead guilty to this charge myself, in the first edition of

this work. The result was to make the treatment of this part of the

subject obscure and imperfect, and in some respects erroneous.

3. The generalized algebraical form of this result is as follows. Let

p be the \_à priori\_ probability of an event, and x be the

credibility of the witness. Then, if he asserts that the event

happened, the probability that it really did happen is

px/(px + (1 - p)(1 - x)); whilst if he asserts that it did \_not\_

happen the probability that it did happen is

p(1 - x)/(p(1 - x) + (1 - p)x).

In illustration of some remarks to be presently made, the reader

will notice that on making either of these expressions = p, we

obtain in each case x = 1/2. That is, a witness whose veracity = 1/2

leaves the \_à priori\_ probability of an event (of this kind)

unaffected.

If, on the other hand, we make these expressions equal to x and 1 - x

respectively, we obtain in each case p = 1/2. That is, when an event

(of this kind) is as likely to happen as not, the ordinary veracity

of the witness in respect of it remains unaffected.

4. Todhunter's \_History\_, p. 400. \_Philosophical Magazine\_, July, 1864.

5. "When therefore these two kinds of experience are contrary, we have

nothing to do but subtract the one from the other, and embrace an

opinion, either on one side or the other, with that assurance which

arises from the remainder." (Essay on Miracles.)

6. Considerations of this kind have indeed been introduced into the

mathematical treatment of the subject. The common algebraical

solution of the problem in §5 (to begin with the simplest case) is

of course as follows. Let p be the antecedent probability of the

event, and t the measure of the truthfulness of the witness; then

the chance of his statement being true is pt/(pt + (1 - p)(1 - t)).

This supposes him to lie as much when the event does not happen as

when it does. But we may meet the cases supposed in the text by

assuming that t' is the measure of his veracity when the event does

not happen, so that the above formula becomes pt/(pt + (1 - p)(1 - t')).

Here t' and t measure respectively his trustworthiness in usual and

unusual events. As a formal solution this certainly meets the

objections stated above in §§14 and 15. The determination however

of t' would demand, as I have remarked, continually renewed appeal

to experience. In any case the practical methods which would be

adopted, if any plans of the kind indicated above were resorted to,

seem to me to differ very much from that adopted by the

mathematicians, in their spirit and plan.

7. Laplace, for instance (\_Essai\_, ed. 1825, p. 149), says that if we

saw 100 dies (known of course to be fair ones) all give the same

face, we should be bewildered at the time, and need confirmation

from others, but that, after due examination, no one would feel

obliged to postulate hallucination in the matter. But the chance of

this occurrence is represented by a fraction whose numerator is 1,

and denominator contains 77 figures, and is therefore utterly

inappreciable by the imagination. It must be admitted, though, that

there is something hypothetical about such an example, for we could

not really know that the dies were fair with a confidence even

distantly approaching such prodigious odds. In other words, it is

difficult here to keep apart those different aspects of the question

discussed in Chap. XIV. §§28-33.

8. In the first edition this was stated, as it now seems to me, in

decidedly too unqualified a manner. It must be remembered, however,

that (as was shown in §7) this plan is really the best theoretical

one which can be adopted in certain cases.

9. It is on this principle that the remarkable conclusion mentioned on

p. 405 is based. Suppose an event whose probability is p; and that,

of a number of witnesses of the same veracity (y), m assert that it

happened, and n deny this. Generalizing the arithmetical reasoning

given above we see that the chance of the event being asserted

varies as

py^{m} (1 - y)^{n} + (1 - p) y^{n} (1 - y)^{m};

(viz. as the chance that the event happens, and that m are right and

n are wrong; \_plus\_ the chance that it does not happen, and that n

are right and m are wrong). And the chance of its being rightly

asserted as py^{m} (1 - y)^{n}. Therefore the chance that when we

have an assertion before us it is a true one is

py^{m} (1 - y)^{n}

----------------------------------------------

py^{m} (1 - y)^{n} + (1 - p) y^{n} (1 - y)^{m},

which is equal to

py^{m-n}

--------------------------------

py^{m-n} + (1 - p) (1 - y)^{m-n}.

But this last expression represents the probability of an assertion

which is unanimously supported by m - n such witnesses.

10. The stress which Butler lays upon this notion of a scheme is, I

think, one great merit of his \_Analogy\_.

CHAPTER XVIII.

\_THE NATURE AND USE OF AN AVERAGE, AND ON THE DIFFERENT KINDS OF

AVERAGE.\_[1]

1. There is much need of some good account, accessible to the ordinary

English reader, of the nature and properties of the principal kinds

of Mean. The common text-books of Algebra suggest that there are

only three such, viz. the arithmetical, the geometrical and the

harmonical:--thus including two with which the statistician has

little or nothing to do, and excluding two or more with which he

should have a great deal to do. The best three references I can give

the reader are the following. (1) The article \_Moyenne\_ in the

\_Dictionnaire des Sciences Médicales\_, by Dr Bertillon. This is

written somewhat from the Quetelet point of view. (2) A paper by

Fechner in the \_Abhandlungen d. Math. phys. Classe

d. Kön. Sächs. Gesellschaft d. Wiss\_. 1878; pp. 1-76. This contains

a very interesting discussion, especially for the statistician, of a

number of different kinds of mean. His account of the median is

remarkably full and valuable. But little mathematical knowledge is

demanded. (3) A paper by Mr F. Y. Edgeworth in the

\_Camb. Phil. Trans.\_ for 1885, entitled \_Observations and

Statistics\_. This demands some mathematical knowledge. Instead of

dealing, as such investigations generally do, with only one Law of

Error and with only one kind of mean, it covers a wide field of

investigation.

1. We have had such frequent occasion to refer to

\_averages\_, and to the kind of uniformity which they are apt

to display in contrast with individual objects or events, that

it will now be convenient to discuss somewhat more minutely

what are the different kinds of available average, and what

exactly are the functions they perform.

The first vague notion of an average, as we now understand

it, seems to me to involve little more than that of a

something \_intermediate\_ to a number of objects. The objects

must of course resemble each other in certain respects, otherwise

we should not think of classing them together; and

they must also differ in certain respects, otherwise we should

not distinguish between them. What the average does for

us, under this primitive form, is to enable us conveniently to

retain the group together as a whole. That is, it furnishes a

sort of representative value of the quantitative aspect of the

things in question, which will serve for certain purposes to

take the place of any single member of the group.

It would seem then that the first dawn of the conception

which science reduces to accuracy under the designation of

an average or mean, and then proceeds to subdivide into

various distinct species of means, presents itself as performing

some of the functions of a general name. For what

is the main use of a general name? It is to reduce a plurality

of objects to unity; to group a number of things

together by reference to some qualities which they possess

in common. The ordinary general name rests upon a considerable

variety of attributes, mostly of a qualitative

character, whereas the average, in so far as it serves the

same sort of purpose, rests rather upon a single quantitative

attribute. It directs attention to a certain kind and degree

of magnitude. When the grazier says of his sheep that 'one

with another they will fetch about 50 shillings,' or the

farmer buys a lot of poles which 'run to about 10 feet,' it is

true that they are not strictly using the equivalent of either

a general or a collective name. But they are coming very

near to such use, in picking out a sort of type or specimen of

the magnitude to which attention is to be directed, and in

classing the whole group by its resemblance to this type.

The grazier is thinking of his sheep: not in a merely general

sense, as sheep, and therefore under that name or conception,

but as sheep of a certain approximate money value.

Some will be more, some less, but they are all near enough

to the assigned value to be conveniently classed together as

if by a name. Many of our rough quantitative designations

seem to be of this kind, as when we speak of 'eight-day

clocks' or 'twelve-stone men,' &c.; unless of course we intend

(as we sometimes do in these cases) to assign a maximum

or minimum value. It is not indeed easy to see how else we

could readily convey a merely general notion of the quantitative

aspect of things, except by selecting a type as above,

or by assigning certain limits within which the things are

supposed to lie.

2. So far there is not necessarily any idea introduced

of comparison,--of comparison, that is, of one group with

another,--by aid of such an average. As soon as we begin

to think of this we have to be more precise in saying what

we mean by an average. We can easily see that the number

of possible kinds of average, in the sense of intermediate

values, is very great; is, in fact, indefinitely great. Out of

the general conception of an intermediate value, obtained by

some treatment of the original magnitudes, we can elicit as

many subdivisions as we please, by various modes of treatment.

There are however only three or four which for our

purposes need be taken into account.

(1) In the first place there is the arithmetical average

or mean. The rule for obtaining this is very simple: add

all the magnitudes together, and divide the sum by their

number. This is the only kind of average with which the

unscientific mind is thoroughly familiar. But we must not

let this simplicity and familiarity blind us to the fact that

there are definite reasons for the employment of this average,

and that it is therefore appropriate only in definite circumstances.

The reason why it affords a safe and accurate

intermediate value for the actual divergent values, is that

for many of the ordinary purposes of life, such as purchase

and sale, we come to exactly the same result, whether we

take account of those existent divergences, or suppose all

the objects equated to their average. What the grazier

must be understood to mean, if he wishes to be accurate, by

saying that the average price of his sheep is 50 shillings, is,

that so far as that flock is concerned (and so far as he is

concerned), it comes to exactly the same thing, whether they

are each sold at different prices, or are all sold at the 'average'

price. Accordingly, when he compares his sales of one

year with those of another; when he says that last year the

sheep averaged 48 shillings against the 50 of this year; the

employment of this representative or average value is a great

simplification, and is perfectly accurate for the purpose in

question.

3. (2) Now consider this case. A certain population is

found to have doubled itself in 100 years: can we talk of an

'average' increase here of 1 per cent. annually? The circumstances

are not quite the same as in the former case, but

the analogy is sufficiently close for our purpose. The answer

is decidedly, No. If 100 articles of any kind are sold for £100,

we say that the average price is £1. By this we mean that

the total amount is the same whether the entire lot are sold

for £100, or whether we split the lot up into individuals

and sell each of these for £1. The average price here is a

convenient fictitious substitute, which can be applied for

each individual without altering the aggregate total. If

therefore the question be, Will a supposed increase of 1 p. c.

in each of the 100 years be equivalent to a total increase to

double the original amount? we are proposing a closely

analogous question. And the answer, as just remarked, must

be in the negative. An annual increase of 1 p. c. continued

for 100 years will more than double the total; it will multiply

it by about 2.7. The true annual increment required is measured

by sqrt[100]{2}; that is, the population may be said to have

increased 'on the average' 0.7 p. c. annually.

We are thus directed to the second kind of average discussed

in the ordinary text-books of algebra, viz. the geometrical.

When only two quantities are concerned, with a single

intermediate value between them, the geometrical mean constituting

this last is best described as the mean proportional

between the two former. Thus, since 3 : sqrt{15} :: sqrt{15} : 5,

sqrt{15} is the geometrical mean between 3 and 5. When a

number of geometrical means have to be interposed between

two quantities, they are to be so chosen that every term in

the entire succession shall bear the same constant ratio to

its predecessor. Thus, in the example in the last paragraph,

99 intermediate steps were to be interposed between 1 and 2,

with the condition that the 100 ratios thus produced were to

be all equal.

It would seem therefore that wherever accurate quantitative

results are concerned, the selection of the appropriate

kind of average must depend upon the answer to the question,

What particular intermediate value may be safely

substituted for the actual variety of values, so far as the

precise object in view is concerned? This is an aspect of

the subject which will have to be more fully considered in

the next chapter. But it may safely be laid down that for

purposes of general comparison, where accurate numerical

relations are not required, almost any kind of intermediate

value will answer our purpose, provided we adhere to the

same throughout. Thus, if we want to compare the statures

of the inhabitants of different counties or districts in England,

or of Englishmen generally with those of Frenchmen,

or to ascertain whether the stature of some particular class

or district is increasing or diminishing, it really does not

seem to matter what sort of average we select provided, of

course, that we adhere to the same throughout our investigations.

A very large amount of the work performed by

averages is of this merely comparative or non-quantitative

description; or, at any rate, nothing more than this is really

required. This being so, we should naturally resort to the

arithmetical average; partly because, having been long in

the field, it is universally understood and appealed to, and

partly because it happens to be remarkably simple and easy

to calculate.

4. The arithmetical mean is for most ordinary purposes

the simplest and best. Indeed, when we are dealing

with a small number of somewhat artificially selected magnitudes,

it is the only mean which any one would think of

employing. We should not, for instance, apply any other

method to the results of a few dozen measurements of lengths

or estimates of prices.

When, however, we come to consider the results of a very

large number of measurements of the kind which can be

grouped together into some sort of 'probability curve' we

begin to find that there is more than one alternative before

us. Begin by recurring to the familiar curve represented

on p. 29; or, better still, to the initial form of it represented

in the next chapter (p. 476). We see that there are three

different ways in which we may describe the vertex of the

curve. We may call it the position of the \_maximum\_ ordinate;

or that of the \_centre\_ of the curve; or (as will be seen

hereafter) the point to which the arithmetical average of

all the different values of the variable magnitude directs us.

These three are all distinct ways of describing a position;

but when we are dealing with a symmetrical curve at all

resembling the binomial or exponential form they all three

coincide in giving the same result: as they obviously do in

the case in question.

As soon, however, as we come to consider the case of

asymmetrical, or lop-sided curves, the indications given by

these three methods will be as a rule quite distinct; and

therefore the two former of these deserve brief notice as

representing different kinds of means from the arithmetical

or ordinary one. We shall see that there is something about

each of them which recommends it to common sense as being

in some way natural and appropriate.

5. (3) The first of these selects from amongst the

various different magnitudes that particular one which is

most frequently represented. It has not acquired any technical

designation,[1] except in so far as it is referred to, by

its graphical representation, as the "maximum ordinate"

method. But I suspect that some appeal to such a mean

or standard is really far from uncommon, and that if we

could draw out into clearness the conceptions latent in the

judgments of the comparatively uncultivated, we should find

that there were various classes of cases in which this mean

was naturally employed. Suppose, for instance, that there

was a fishery in which the fish varied very much in size

but in which the commonest size was somewhat near the

largest or the smallest. If the men were in the habit of

selling their fish by \_weight\_, it is probable that they would

before long begin to acquire some kind of notion of what

is meant by the arithmetical mean or average, and would

perceive that this was the most appropriate test. But if the

fish were sorted into sizes, and sold by numbers in each of

these sizes, I suspect that this appeal to a maximum ordinate

would begin to take the place of the other. That is,

the most numerous class would come to be selected as a

sort of type by which to compare the same fishery at one

time and another, or one fishery with others. There is also,

as we shall see in the next chapter, some scientific ground

for the preference of this kind of mean in peculiar cases;

viz. where the quantities with which we deal are true

'errors,' in the estimate of some magnitude, and where also

it is of much more importance to be exactly right, or very

nearly right, than to have merely a low average of error.

6. (4) The remaining kind of mean is that which is

now coming to be called the "median." It is one with

which the writings of Mr Galton have done so much to

familiarize statisticians, and is best described as follows.

Conceive all the objects in question to be marshalled in the

order of their magnitude; or, what comes to the same thing,

conceive them sorted into a number of equally numerous

classes; then the middle one of the row, or the middle one

in the middle class, will be the \_median\_. I do not think

that this kind of mean is at all generally recognized at

present, but if Mr Galton's scheme of natural measurement

by what he calls "per-centiles" should come to be generally

adopted, such a test would become an important one.

There are some conspicuous advantages about this kind of

mean. For one thing, in most statistical enquiries, it is

far the simplest to calculate; and, what is more, the process

of determining it serves also to assign another important

element to be presently noticed, viz. the 'probable error.'

Then again, as Fechner notes, whereas in the arithmetical

mean a few exceptional and extreme values will often cause

perplexity by their comparative preponderance, in the case

of the median (where their number only and not their extreme

magnitude is taken into account) the importance of

such disturbance is diminished.

7. A simple illustration will serve to indicate how these

three kinds of mean coalesce into one when we are dealing

with symmetrical Laws of Error, but become quite distinct

as soon as we come to consider those which are unsymmetrical.

[Figure: Various definitions of the mean.]

Suppose that, in measuring a magnitude along OBDC,

where the extreme limits are OB and OC, the law of error

is represented by the triangle BAC: the length OD will

be at once the arithmetical mean, the median, and the most

frequent length: its frequency being represented by the

maximum ordinate AD. But now suppose, on the other

hand, that the extreme lengths are OD and OC, and that

the triangle ADC represents the law of error. The most

frequent length will be the same as before, OD, marked by

the maximum ordinate AD. But the mean value will now

be OX, where DX = 1/3 DC; and the median will be OY,

where DY = (1 - 1/sqrt{2}) DC.

Another example, taken from natural phenomena, may

be found in the heights of the barometer as taken at the

same hour on successive days. So far as 4857 of these may

be regarded as furnishing a sufficiently stable basis of experience,

it certainly seems that the resulting curve of frequency

is asymmetrical. The mean height here was found

to be 29.98: the median was 30.01: the most frequent

height was 30.05. The close approximation amongst these

is an indication that the asymmetry is slight.[2]

8. It must be clearly understood that the average, of

whatever kind it may be, from the mere fact of its being a

single substitute for an actual plurality of observed values,

must let slip a considerable amount of information. In fact

it is only introduced for economy. It may entail no loss

when used for some one assigned purpose, as in our example

about the sheep; but for purposes in general it

cannot possibly take the place of the original diversity, by

yielding all the information which they contained. If all

this is to be retained we must resort to some other method.

Practically we generally do one of two things: either (1) we

put all the figures down in statistical tables, or (2) we appeal

to a diagram. This last plan is convenient when the

data are very numerous, or when we wish to display or to

discover the nature of the law of facility under which they

range.

The mere assignment of an average lets drop nearly all

of this, confining itself to the indication of an intermediate

value. It gives a "middle point" of some kind, but says

nothing whatever as to how the original magnitudes were

grouped about this point. For instance, whether two magnitudes

had been respectively 25 and 27, or 15 and 37, they

would yield the same arithmetical average of 26.

9. To break off at this stage would clearly be to leave

the problem in a very imperfect condition. We therefore

naturally seek for some simple test which shall indicate

how closely the separate results were grouped about their

average, so as to recover some part of the information which

had been let slip.

If any one were approaching this problem entirely anew,--that

is, if he had no knowledge of the mathematical exigencies

which attend the theory of "Least Squares,"--I apprehend

that there is but one way in which he would set

about the business. He would say, The average which we

have already obtained gave us a rough indication, by assigning

an intermediate point amongst the original magnitudes.

If we want to supplement this by a rough indication

as to how near together these magnitudes lie, the

best way will be to treat their departures from the mean

(what are technically called the "errors") in precisely the

same way, viz. by assigning \_their\_ average. Suppose there

are 13 men whose heights vary by equal differences from

5 feet to 6 feet, we should say that their average height

was 66 inches, and their average departure from this average

was 3-3/13 inches.

Looked at from this point of view we should then proceed

to try how each of the above-named averages would

answer the purpose. Two of them,--viz. the arithmetical

mean and the median,--will answer perfectly; and, as we

shall immediately see, are frequently used for the purpose.

So too we could, if we pleased, employ the geometrical

mean, though such employment would be tedious, owing

to the difficulty of calculation. The 'maximum ordinate'

clearly would not answer, since it would generally (v. the

diagram on p. 443) refer us back again to the average

already obtained, and therefore give no information.

The only point here about which any doubt could arise

concerns what is called in algebra the \_sign\_ of the errors.

Two equal and opposite errors, added algebraically, would

cancel each other. But when, as here, we are regarding

the errors as substantive quantities, to be considered on

their own account, we attend only to their real magnitude,

and then these equal and opposite errors are to be put upon

exactly the same footing.

10. Of the various means already discussed, two, as

just remarked, are in common use. One of these is familiarly

known, in astronomical and other calculations, as

the 'Mean Error,' and is so absolutely an application of the

same principle of the arithmetical mean to the errors, that

has been already applied to the original magnitudes, that it

needs no further explanation. Thus in the example in the

last section the mean of the heights was 66 inches, the

mean of the errors was 3-3/13 inches.

The other is the Median, though here it is always known

under another name, i.e. as the 'Probable Error';--a technical

and decidedly misleading term. It is briefly defined

as that error which we are as likely to exceed as to fall

short of: otherwise phrased, if we were to arrange all the

errors in the order of their magnitude, it corresponds to that

one of them which just bisects the row. It is therefore the

'median' error: or, if we arrange all the magnitudes in successive

order, and divide them into four equally numerous

classes,--what Mr Galton calls 'quartiles,'--the first and

third of the consequent divisions will mark the limits of

the 'probable error' on each side, whilst the middle one will

mark the 'median.' This median, as was remarked, coincides,

in symmetrical curves, with the arithmetical mean.

It is best to stand by accepted nomenclature, but the

reader must understand that such an error is not in any

strict sense 'probable.' It is indeed highly improbable that

in any particular instance we should happen to get just this

error: in fact, if we chose to be precise and to regard it as

one exact magnitude out of an infinite number, it would be

infinitely unlikely that we should hit upon it. Nor can it be

said to be probable that we shall be \_within\_ this limit of the

truth, for, by definition, we are just as likely to exceed as to

fall short. As already remarked (see note on p. 441), the

'maximum ordinate' would have the best right to be regarded

as indicating the really most probable value.

11. (5) \_The error of mean square.\_ As previously suggested, the plan

which would naturally be adopted by any one who had no concern with

the higher mathematics of the subject, would be to take the 'mean

error' for the purpose of the indication in view. But a very different

kind of average is generally adopted in practice to serve as a test of

the amount of divergence or dispersion. Suppose that we have the

magnitudes x\_{1}, x\_{2}, ... x\_{n}; their ordinary average is

1/n (x\_{1} + x\_{2} + ... + x\_{n}), and their 'errors' are the

differences between this and x\_{1}, x\_{2}, ... x\_{n}. Call these errors

e\_{1}, e\_{2}, ... e\_{n}, then the arithmetical mean of these errors

(irrespective of sign) is 1/n (e\_{1} + e\_{2} + ... + e\_{n}). The Error

of Mean Square,[3] on the other hand, is the square root of

1/n (e\_{1}^{2} + e\_{2}^{2} + ... + e\_{n}^{2}).

The reasons for employing this latter kind of average in

preference to any of the others will be indicated in the following

chapter. At present we are concerned only with the

general logical nature of an average, and it is therefore

sufficient to point out that any such intermediate value will

answer the purpose of giving a rough and summary indication

of the degree of closeness of approximation which our

various measures display to each other and to their common

average. If we were to speak respectively of the 'first' and

the 'second average,' we might say that the former of these

assigns a rough single substitute for the plurality of original

values, whilst the latter gives a similar rough estimate of the

degree of their departure from the former.

12. So far we have only been considering the general

nature of an average, and the principal kinds of average

practically in use. We must now enquire more particularly

what are the principal purposes for which averages are employed.

In this respect the first thing we have to do is to raise

doubts in the reader's mind on a subject on which he

perhaps has not hitherto felt the slightest doubt. Every

one is more or less familiar with the practice of appealing to

an average in order to secure accuracy. But distinctly what

we begin by doing is to sacrifice accuracy; for in place of

the plurality of actual results we get a single result which

very possibly does not agree with any one of them. If I find

the temperature in different parts of a room to be different,

but say that the average temperature is 61°, there may perhaps

be but few parts of the room where this exact temperature

is realized. And if I say that the average stature of

a certain small group of men is 68 inches, it is probable that

no one of them will present precisely this height.

The principal way in which accuracy can be thus secured

is when what we are really aiming at is not the magnitudes

before us but something else of which they are an indication.

If they are themselves 'inaccurate,'--we shall see presently

that this needs some explanation,--then the single average,

which in itself agrees perhaps with none of them, may be

much more nearly what we are actually in want of. We shall

find it convenient to subdivide this view of the subject into

two parts; by considering first those cases in which quantitative

considerations enter but slightly, and in which no determination

of the particular Law of Error involved is demanded,

and secondly those in which such determination cannot be

avoided. The latter are only noticed in passing here, as a

separate chapter is reserved for their fuller consideration.

13. The process, as a practical one, is familiar enough

to almost everybody who has to work with measures of any

kind. Suppose, for instance, that I am measuring any object

with a brass rod which, as we know, expands and contracts

according to the temperature. The results will vary slightly,

being sometimes a little too great and sometimes a little too

small. All these variations are physical facts, and if what

we were concerned with was the properties of brass they

would be the one important fact for us. But when we are

concerned with the length of the object measured, these facts

become superfluous and misleading. What we want to do is

to escape their influence, and this we are enabled to effect by

taking their (arithmetical) average, provided only they are

as often in excess as in defect.[4] For this purpose all that is

necessary is that equal excesses and defects should be

equally prevalent. It is not necessary to know what is the

law of variation, or even to be assured that it is of one particular

kind. Provided only that it is in the language of

the diagram on p. 29, symmetrical, then the arithmetical

average of a suitable and suitably varied number of measurements

will be free from this source of disturbance. And

what holds good of this cause of variation will hold good of

all others which obey the same general conditions. In fact

the equal prevalence of equal and opposite errors seems to

be the sole and sufficient justification of the familiar process

of taking the average in order to secure accuracy.

14. We must now make the distinction to which attention

requires so often to be drawn in these subjects

between the cases in which there respectively is, and is not,

some objective magnitude aimed at: a distinction which the

common use of the same word "errors" is so apt to obscure.

When we talked, in the case of the brass rod, of excesses

and defects being equal, we meant exactly what we said, viz.

that for every case in which the 'true' length (i.e. that determined

by the authorized standard) is exceeded by a given

fraction of an inch, there will be a corresponding case in

which there is an equal defect.

On the other hand, when there is no such fixed objective

standard of reference, it would appear that all that we mean

by equal excesses and defects is permanent symmetry of

arrangement. In the case of the measuring rod we were

able to start with something which existed, so to say, before

its variations; but in many cases any starting point which

we can find is solely determined by the average.

Suppose, for instance, we take a great number of observations

of the height of the barometer at a certain place,

at all times and seasons and in all weathers, we should

generally consider that the average of all these showed the

'true' height for that place. What we really mean is that

the height at any moment is determined partly (and principally)

by the height of the column of air above it, but partly

also by a number of other agencies such as local temperature,

moisture, wind, &c. These are sometimes more and sometimes

less effective, but their range being tolerably constant,

and their distribution through this range being

tolerably symmetrical, the average of one large batch of

observations will be almost exactly the same as that of any

other. This constancy of the average \_is\_ its truth. I am

quite aware that we find it difficult not to suppose that

there must be something more than this constancy, but we

are probably apt to be misled by the analogy of the other

class of cases, viz. those in which we are really aiming at

some sort of mark.

15. As regards the practical methods available for

determining the various kinds of average there is very little

to be said; as the arithmetical rules are simple and definite,

and involve nothing more than the inevitable drudgery

attendant upon dealing with long rows of figures. Perhaps

the most important contribution to this part of the subject is

furnished by Mr Galton's suggestion to substitute the median

for the mean, and thus to elicit the average with sufficient

accuracy by the mere act of grouping a number of objects

together. Thus he has given an ingenious suggestion for

obtaining the average height of a number of men without

the trouble and risk of measuring them all. "A barbarian

chief might often be induced to marshall his men in the

order of their heights, or in that of the popular estimate of

their skill in any capacity; but it would require some apparatus

and a great deal of time to measure each man

separately, even supposing it possible to overcome the usually

strong repugnance of uncivilized people to any such proceeding"

(\_Phil. Mag.\_ Jan. 1875). That is, it being known

from wide experience that the heights of any tolerably

homogeneous set of men are apt to group themselves symmetrically,--the

condition for the coincidence of the three

principal kinds of mean,--the middle man of a row thus

arranged in order will represent the mean or average man,

and him we may subject to measurement. Moreover, since

the intermediate heights are much more thickly represented

than the extreme ones, a moderate error in the selection of

the central man of a long row will only entail a very small

error in the selection of the corresponding height.

16. We can now conveniently recur to a subject which

has been already noticed in a former chapter, viz. the attempt

which is sometimes made to establish a distinction

between an average and a mean. It has been proposed to

confine the former term to the cases in which we are dealing

with a fictitious result of our own construction, that is, with

a mere arithmetical deduction from the observed magnitudes,

and to apply the latter to cases in which there is

supposed to be some objective magnitude peculiarly representative

of the average.

Recur to the three principal classes, of things appropriate

to Probability, which were sketched out in Ch. II. §4. The

first of these comprised the results of games of chance. Toss

a die ten times: the total number of pips on the upper

side may vary from ten up to sixty. Suppose it to be

thirty. We then say that the average of this batch of

ten is three. Take another set of ten throws, and we may

get another average, say four. There is clearly nothing

objective peculiarly corresponding in any way to these

averages. No doubt if we go on long enough we shall

find that the averages tend to centre about 3.5: we then

call this \_the\_ average, or the 'probable' number of points;

and this ultimate average might have been pretty constantly

asserted beforehand from our knowledge of the constitution

of a die. It has however no other truth or reality

about it of the nature of a type: it is simply the limit

towards which the averages tend.

The next class is that occupied by the members of most

natural groups of objects, especially as regards the characteristics

of natural species. Somewhat similar remarks may

be repeated here. There is very frequently a 'limit' towards

which the averages of increasing numbers of individuals tend

to approach; and there is certainly some temptation to regard

this limit as being a sort of type which all had been

intended to resemble as closely as possible. But when we

looked closer, we found that this view could scarcely be

justified; all which could be safely asserted was that this

type represented, for the time being, the most numerous

specimens, or those which under existing conditions could

most easily be produced.

The remaining class stands on a somewhat different

ground. When we make a succession of more or less successful

attempts of any kind, we get a corresponding series

of deviations from the mark at which we aimed. These we

may treat arithmetically, and obtain their averages, just as

in the former cases. These averages are fictions, that is to

say, they are artificial deductions of our own which need

not necessarily have anything objective corresponding to

them. In fact, if they be averages of a \_few\_ only they

most probably will not have anything thus corresponding

to them. Anything answering to a type can only be sought

in the 'limit' towards which they ultimately tend, for this

limit coincides with the fixed point or object aimed at.

17. Fully admitting the great value and interest of

Quetelet's work in this direction,--he was certainly the first

to direct public attention to the fact that so many classes of

natural objects display the same characteristic property,--it

nevertheless does not seem desirable to attempt to mark

such a distinction by any special use of these technical

terms. The objections are principally the two following.

In the first place, a single antithesis, like this between

an average and a mean, appears to suggest a very much

simpler state of things than is actually found to exist in

nature. A reference to the three classes of things just

mentioned, and a consideration of the wide range and diversity

included in each of them, will serve to remind us

not only of the very gradual and insensible advance from

what is thus regarded as 'fictitious' to what is claimed as

'real;' but also of the important fact that whereas the 'real

type' may be of a fluctuating and evanescent character, the

'fiction' may (as in games of chance) be apparently fixed

for ever. Provided only that the conditions of production

remain stable, averages of large numbers will always practically

present much the same general characteristics. The

far more important distinction lies between the average of

a few, with its fluctuating values and very imperfect and

occasional attainment of its ultimate goal, and the average

of many and its gradually close approximation to its ultimate

value: i.e. to its objective point of aim if there happen

to be such.

Then, again, the considerations adduced in this chapter

will show that within the field of the average itself there is

far more variety than Quetelet seems to have recognized.

He did not indeed quite ignore this variety, but he practically

confined himself almost entirely to those symmetrical

arrangements in which three of the principal means coalesce

into one. We should find it difficult to carry out his distinction

in less simple cases. For instance, when there is

some degree of asymmetry, it is the 'maximum ordinate'

which would have to be considered as a 'mean' to the

exclusion of the others; for no appeal to an arithmetical

average would guide us to this point, which however is to

be regarded, if any can be so regarded, as marking out the

position of the ultimate type.

18. We have several times pointed out that it is a

characteristic of the things with which Probability is concerned

to present, in the long run, a continually intensifying

uniformity. And this has been frequently described as what

happens 'on the average.' Now an objection may very

possibly be raised against regarding an arrangement of

things by virtue of which order thus emerges out of disorder

as deserving any special notice, on the ground that from the

nature of the arithmetical average it could not possibly be

otherwise. The process by which an average is obtained, it

may be urged, insures this tendency to equalization amongst

the magnitudes with which it deals. For instance, let there

be a party of ten men, of whom four are tall and four are

short, and take the average of any five of them. Since this

number cannot be made up of tall men only, or of short men

only, it stands to reason that the averages cannot differ so

much amongst themselves as the single measures can. Is

not then the equalizing process, it may be asked, which is

observable on increasing the range of our observations,

one which can be shown to follow from necessary laws of

arithmetic, and one therefore which might be asserted \_à priori\_?

Whatever force there may be in the above objection arises

principally from the limitations of the example selected, in

which the number chosen was so large a proportion of the

total as to exclude the bare possibility of only extreme cases

being contained within it. As much confusion is often felt

here between what is necessary and what is matter of experience,

it will be well to look at an example somewhat

more closely, in order to determine exactly what are the

really necessary consequences of the averaging process.

19. Suppose then that we take ten digits at random

from a table (say) of logarithms. Unless in the highly unlikely

case of our having happened upon the same digit ten

times running, the average of the ten \_must\_ be intermediate

between the possible extremes. Every conception of an

average of any sort not merely involves, but actually means,

the taking of something intermediate between the extremes.

The average therefore of the ten must lie closer to 4.5 (the

average of the extremes) than did some of the single digits.

Now suppose we take 1000 such digits instead of 10. We

can say nothing more about the larger number, with demonstrative

certainty, than we could before about the smaller.

If they were unequal to begin with (i.e. if they were not all

the same) then the average \_must\_ be intermediate, but more

than this cannot be proved arithmetically. By comparison with

such purely arithmetical considerations there is what may be

called a physical fact underlying our confidence in the growing

stability of the average of the larger number. It is that

the constituent elements from which the average is deduced

will themselves betray a growing uniformity:--that the proportions

in which the different digits come out will become

more and more nearly equal as we take larger numbers of

them. If the proportions in which the 1000 digits were

distributed were the same as those of the 10 the averages

would be the same. It is obvious therefore that the arithmetical

process of obtaining an average goes a very little

way towards securing the striking kind of uniformity which

we find to be actually presented.

20. There is another way in which the same thing

may be put. It is sometimes said that whatever may have

been the arrangement of the original elements the process of

continual averaging will necessarily produce the peculiar

binomial or exponential law of arrangement. This statement

is perfectly true (with certain safeguards) but it is not

in any way opposed to what has been said above. Let us

take for consideration the example above referred to. The

arrangement of the individual digits in the long run is the

simplest possible. It would be represented, in a diagram,

not by a curve but by a finite straight line, for each digit

occurs about as often as any other, and this exhausts all the

'arrangement' that can be detected. Now, when we consider

the results of taking averages of ten such digits, we see

at once that there is an opening for a more extensive arrangement.

The totals may range from 0 up to 100, and therefore

the average will have 100 values from 0 to 9; and what

we find is that the frequency of these numbers is determined

according to the Binomial[5] or Exponential Law. The most

frequent result is the true mean, viz. 4.5, and from this they

diminish in each direction towards 0 and 10, which will each

occur but once (on the average) in 10^{10} occasions.

The explanation here is of the same kind as in the former

case. The resultant arrangement, so far as the averages are

concerned, is only 'necessary' in the sense that it is a necessary

result of certain physical assumptions or experiences.

If all the digits tend to occur with equal frequency, and if

they are 'independent' (i.e. if each is associated indifferently

with every other), then it is an arithmetical consequence

that the averages when arranged in respect of their magnitude

and prevalence will display the Law of Facility above

indicated. Experience, so far as it can be appealed to, shows

that the true randomness of the selection of the digits,--i.e.

their equally frequent recurrence, and the impartiality of

their combination,--is very fairly secured in practice. Accordingly

the theoretic deduction that whatever may have

been the original Law of Facility of the individual results

we shall always find the familiar Exponential Law asserting

itself as the law of the averages, is fairly justified by experience

in such a case.

The further discussion of certain corrections and refinements

is reserved to the following chapter.

21. In regard to the three kinds of average employed

to test the amount of dispersion,--i.e. the mean error, the

probable error, and the error of mean square,--two important

considerations must be borne in mind. They will

both recur for fuller discussion and justification in the course

of the next chapter, when we come to touch upon the Method

of Least Squares, but their significance for logical purposes

is so great that they ought not to be entirely passed by at

present.

(1) In the first place, then, it must be remarked that in

order to know what in any case is the real value of an error

we ought in strictness to know what is the position of the

limit or ultimate average, for the amount of an error is

always theoretically measured from this point. But this is

information which we do not always possess. Recurring

once more to the three principal classes of events with which

we are concerned, we can readily see that in the case of

games of chance we mostly do possess this knowledge. Instead

of appealing to experience to ascertain the limit, we

practically deduce it by simple mechanical or arithmetical

considerations, and then the 'error' in any individual case or

group of cases is obviously found by comparing the results

thus obtained with that which theory informs us would ultimately

be obtained in the long run. In the case of deliberate

efforts at an aim (the third class) we may or may

not know accurately the value or position of this aim. In

astronomical observations we do not know it, and the method

of Least Squares is a method for helping us to ascertain it as

well as we can; in such experimental results as firing at a

mark we do know it, and may thus test the nature and

amount of our failure by direct experience. In the remaining

case, namely that of what we have termed natural kinds

or groups of things, not only do we not know the ultimate

limit, but its existence is always at least doubtful, and in

many cases may be confidently denied. Where it does exist,

that is, where the type seems for all practical purposes permanently

fixed, we can only ascertain it by a laborious resort

to statistics. Having done this, we may then test by it the

results of observations on a small scale. For instance, if we

find that the ultimate proportion of male to female births is

about 106 to 100, we may then compare the statistics of

some particular district or town and speak of the consequent

'error,' viz. the departure, in that particular and special

district, from the general average.

What we have therefore to do in the vast majority of

practical cases is to take the average of a finite number of

measurements or observations,--of all those, in fact, which

we have in hand,--and take \_this\_ as our starting point in

order to measure the errors. The errors in fact are not

known for certain but only probably calculated. This however

is not so much of a theoretic defect as it may seem at

first sight; for inasmuch as we seldom have to employ these

methods,--for purposes of calculation, that is, as distinguished

from mere illustration,--except for the purpose of discovering

what the ultimate average is, it would be a sort of

\_petitio principii\_ to assume that we had already secured it.

But it is worth while considering whether it is desirable to

employ one and the same term for 'errors' known to be

such, and whose amount can be assigned with certainty, and

for 'errors' which are only probably such and whose amount

can be only probably assigned. In fact it has been proposed[6]

to employ the two terms 'error' and 'residual' respectively

to distinguish between the magnitudes thus determined, that

is, between the (generally unknown) actual error and the observed

error.

22. (2) The other point involves the question to what

extent either of the first two tests (pp. 446,7) of the closeness

with which the various results have grouped themselves

about their average is trustworthy or complete. The answer

is that they are necessarily incomplete. No single estimate

or magnitude can possibly give us an adequate account of a

number of various magnitudes. The point is a very important

one; and is not, I think, sufficiently attended to, the

consequence being, as we shall see hereafter, that it is far

too summarily assumed that a method which yields the

result with the least 'error of mean square' must necessarily

be the best result for all purposes. It is not however by any

means clear that a test which answers best for one purpose

must do so for all.

It must be clearly understood that each of these tests is

an 'average,' and that every average necessarily rejects a

mass of varied detail by substituting for it a single result.

We had, say, a lot of statures: so many of 60 inches, so

many of 61, &c. We replace these by an 'average' of 68,

and thereby drop a mass of information. A portion of this

we then seek to recover by reconsidering the 'errors' or

departures of these statures from their average. As before,

however, instead of giving the full details we substitute an

average of the errors. The only difference is that instead of

taking the same kind of average (i.e. the arithmetical) we

often prefer to adopt the one called the 'error of mean

square.'

23. A question may be raised here which is of sufficient

importance to deserve a short consideration. When we have

got a set of measurements before us, why is it generally

held to be sufficient simply to assign: (1) the mean value;

and (2) the mean departure from this mean? The answer

is, of course, partly given by the fact that we are only supposed

to be in want of a rough approximation: but there is

more to be said than this. A further justification is to be

found in the fact that we assume that we need only contemplate

the possibility of a single Law of Error, or at any

rate that the departures from the familiar Law will be but

trifling. In other words, if we recur to the figure on p. 29,

we assume that there are only two unknown quantities or

disposable constants to be assigned; viz. first, the position of

the centre, and, secondly, the degree of eccentricity, if one

may so term it, of the curve. The determination of the

mean value directly and at once assigns the former, and the

determination of the mean error (in either of the ways referred

to already) indirectly assigns the latter by confining us

to one alone of the possible curves indicated in the figure.

Except for the assumption of one such Law of Error the

determination of the mean error would give but a slight

intimation of the sort of outline of our Curve of Facility.

We might then have found it convenient to adopt some plan

of successive approximation, by adding a third or fourth

'mean.' Just as we assign the mean value of the magnitude,

and its mean departure from this mean; so we might

take this mean error (however determined) as a fresh starting

point, and assign the mean departure from it. If the point

were worth further discussion we might easily illustrate by

means of a diagram the sort of successive approximations

which such indications would yield as to the ultimate form

of the Curve of Facility or Law of Error.

\* \* \* \* \*

As this volume is written mainly for those who take an interest in the

logical questions involved, rather than as an introduction to the actual

processes of calculation, mathematical details have been throughout avoided

as much as possible. For this reason comparatively few references have

been made to the exponential equation of the Law of Error, or to the

corresponding 'Probability integral,' tables of which are given in several

handbooks on the subject. There are two points however in connection

with these particular topics as to which difficulties are, or should be,

felt by so many students that some notice may be taken of them here

(1) In regard to the ordinary algebraical expression for the law of

error, viz. y = (h/sqrt{π}) e^{-h^{2}x^{2}}, it will have been

observed that I have always spoken of y as being \_proportional\_ to the

number of errors of the particular magnitude x. It would hardly be

correct to say, absolutely, that y \_represents\_ that number, because

of course the actual number of errors of any precise magnitude, where

continuity of possibility is assumed, must be indefinitely small. If

therefore we want to pass from the continuous to the discrete, by

ascertaining the actual number of errors between two consecutive

divisions of our scale, when, as usual in measurements, all within

certain limits are referred to some one precise point, we must modify

our formula. In accordance with the usual differential notation, we

must say that the number of errors falling into one subdivision (dx)

of our scale \_is\_ dx (h/sqrt{π}) e^{-h^{2} x^{2}}, where dx is a

(small) unit of length, in which both h^{-1} and x must be measured.

The difficulty felt by most students is in applying the formula to

actual statistics, in other words in putting in the correct units. To

take an actual numerical example, suppose that 1460 men have been

measured in regard to their height "true to the nearest inch," and let

it be known that the modulus here is 3.6 inches. Then dx = 1 (inch);

h^{-1} = 3.6 inches. Now sum (h/sqrt{π}) e^{-h^{2}x^{2}} dx = 1; that

is, the sum of all the consecutive possible values is equal to

\_unity\_. When therefore we want the sum, as here, to be 1460, we must

express the formula thus;--y = 1460/(sqrt{π} × 3.6) e^{-(x/3.6)^{2}},

or y = 228 e^{-(x/3.6)^{2}}.

Here x stands for the number of inches measured from the central or

mean height, and y stands for the number of men referred to that

height in our statistical table. (The values of e^{-t^{2}} for

successive values of t are given in the handbooks.)

For illustration I give the calculated numbers by this formula for values

of x from 0 to 8 inches, with the actual numbers observed in the Cambridge

measurements recently set on foot by Mr Galton.

inches calculated observed

x = 0 y = 228 = 231

x = 1 y = 212 = 218

x = 2 y = 166 = 170

x = 3 y = 111 = 110

x = 4 y = 82 = 66

x = 5 y = 32 = 31

x = 6 y = 11 = 10

x = 7 y = 4 = 6

x = 8 y = 1 = 3

Here the average height was 69 inches: dx, as stated, = 1 inch. By

saying, 'put x = 0,' we mean, calculate the number of men who are assigned

to 69 inches; i.e. who fall between 68.5 and 69.5. By saying, 'put x = 4,'

we mean, calculate the number who are assigned to 65 or to 73; i.e. who lie

between 64.5 and 65.5, or between 72.5 and 73.5. The observed results, it

will be seen, keep pretty close to the calculated: in the case of the former

the \_means\_ of equal and opposite divergences from the mean have been taken,

the actual results not being always the same in opposite directions.

(2) The other point concerns the interpretation of the familiar

probability integral, (2/sqrt{π}) integral\_{0}^{t} e^{-t^{2}} dt.

Every one who has calculated the chance of an event, by the help

of the tables of this integral given in so many handbooks, knows that

if we assign any numerical value to t, the corresponding value of the

above expression assigns the chance that an error taken at random

shall lie within that same limit, viz. t. Thus put t = 1.5, and we

have the result 0.96; that is, only 4 per cent. of the errors will

exceed 'one and a half.' But when we ask, 'one and a half' \_what?\_ the

answer would not always be very ready. As usual, the main difficulty

of the beginner is not to manipulate the formulæ, but to be quite

clear about his units.

It will be seen at once that this case differs from the preceding in that

we cannot now choose our unit as we please. Where, as here, there is only

one variable (t), if we were allowed to select our own unit, the inch, foot,

or whatever it might be, we might get quite different results. Accordingly

some comparatively natural unit must have been chosen for us in which we

are bound to reckon, just as in the circular measurement of an angle as

distinguished from that by degrees.

The answer is that the unit here is the \_modulus\_, and that to put 't = 1.5'

is to say, 'suppose the error half as great again as the modulus'; the

modulus itself being an error of a certain assignable magnitude depending

upon the nature of the measurements or observations in question. We shall

see this better if we put the integral in the form

(2/sqrt{π}) integral\_{0}^{hx} e^{-h^{2} x^{2}} d(hx);

which is precisely equivalent, since the value of a definite integral is

independent of the particular variable employed. Here hx is the same as

x : 1/h; i.e. it is the ratio of x to 1/h, or x measured in terms of 1/h.

But 1/h is the modulus in the equation

(y = (h/sqrt{π}) e^{-h^{2} x^{2}})

for the law of error. In other words the numerical value of an error in

this formula, is the number of times, whole or fractional, which it

contains the modulus.

1. This kind of mean is called by Fechner and others the "\_dichteste

Werth\_." The most appropriate appeal to it that I have seen is by

Prof. Lexis (\_Massenerscheinungen\_, p. 42) where he shows that it

indicates clearly a sort of normal length of human life, of about 70

years; a result which is almost entirely masked when we appeal to

the arithmetical average.

This mean \_ought\_ to be called the 'probable' value (a name however

in possession of another) on the ground that it indicates the point

of likeliest occurrence; i.e. if we compare all the indefinitely

small and equal units of variation, the one corresponding to this

will tend to be most frequently represented.

2. A diagram illustrative of this number of results was given in

\_Nature\_ (Sept. 1, 1887). In calculating, as above, the different

means, I may remark that the original results were given to three

decimal places; but, in classing them, only one place was

noted. That is, 29.9 includes all values between 29.900 and 29.999.

Thus the value most frequently entered in my tables was 30.0, but on

the usual principles of interpolation this is reckoned as 30.05.

3. There is some ambiguity in the phraseology in use here. Thus Airy

commonly uses the expression 'Error of Mean Square' to represent, as

here, sqrt{(sum e^{2})/n}. Galloway commonly speaks of the 'Mean

Square of the Errors' to represent (sum e^{2})/n. I shall adhere to

the former usage and represent it briefly by E.M.S. Still more

unfortunate (to my thinking) is the employment, by Mr Merriman and

others, of the expression 'Mean Error,' (widely in use in its more

natural signification,) as the equivalent of this E.M.S.

The technical term 'Fluctuation' is applied by Mr F. Y. Edgeworth to

the expression (2 sum e^{2})/n.

4. Practically, of course, we should allow for the expansion or

contraction. But for purposes of logical explanation we may

conveniently take this variation as a specimen of one of those

disturbances which may be neutralised by resort to an average.

5. More strictly \_multinomial\_: the relative frequency of the

different numbers being indicated by the coefficients of the powers

of x in the development of

(1 + x + x^{2} + ... + x^{9})^{10}.

6. By Mr Merriman, in his work on \_Least Squares\_.

CHAPTER XIX.

\_THE THEORY OF THE AVERAGE AS A MEANS OF APPROXIMATION TO THE TRUTH.\_

1. In the last chapter we were occupied with the Average

mainly under its qualitative rather than its quantitative

aspect. That is, we discussed its general nature, its principal

varieties, and the main uses to which it could be put in

ordinary life or in reasoning processes which did not claim to

be very exact. It is now time to enter more minutely into

the specific question of the employment of the average in

the way peculiarly appropriate to Probability. That is, we

must be supposed to have a certain number of measurements,--in

the widest sense of that term,--placed before us,

and to be prepared to answer such questions as; Why do we

take their average? With what degree of confidence?

Must we in all cases take the average, and, if so, one always

of the same kind?

The subject upon which we are thus entering is one

which, under its most general theoretic treatment, has perhaps

given rise to more profound investigation, to a greater

variety of opinion, and in consequence to a more extensive

history and literature, than any other single problem within

the range of mathematics.[1] But, in spite of this, the main

logical principles underlying the methods and processes in

question are not, I apprehend, particularly difficult to grasp:

though, owing to the extremely technical style of treatment

adopted even in comparatively elementary discussions of the

subject, it is far from easy for those who have but a moderate

command of mathematical resources to disentangle these

principles from the symbols in which they are clothed. The

present chapter contains an attempt to remove these difficulties,

so far as a general comprehension of the subject is concerned.

As the treatment thus adopted involves a considerable

number of subdivisions, the reader will probably

find it convenient to refer back occasionally to the table of

contents at the commencement of this volume.

2. The subject, in the form in which we shall discuss

it, will be narrowed to the consideration of the \_average\_, on

account of the comparative simplicity and very wide prevalence

of this aspect of the problem. The problem is however

very commonly referred to, even in non-mathematical treatises,

as the Rule or Method of Least Squares; the fact being

that, in such cases as we shall be concerned with, the Rule

of Least Squares resolves itself into the simpler and more

familiar process of taking the arithmetical average. A very

simple example,--one given by Herschel,--will explain the

general nature of the task under a slightly wider treatment,

and will serve to justify the familiar designation.

Suppose that a man had been firing for some time with a

pistol at a small mark, say a wafer on a wall. We may take

it for granted that the shot-marks would tend to group

themselves about the wafer as a centre, with a density varying

in some way inversely with the distance from the centre.

But now suppose that the wafer which marked the centre

was removed, so that we could see nothing but the surface of

the wall spotted with the shot-marks; and that we were

asked to guess the position of the wafer. Had there been

only \_one\_ shot, common sense would suggest our assuming

(of course very precariously) that this marked the real centre.

Had there been two, common sense would suggest our taking

the mid-point between them. But if three or more were

involved, common sense would be at a loss. It would feel

that some intermediate point ought to be selected, but

would not see its way to a more precise determination, because

its familiar reliance,--the arithmetical average,--does

not seem at hand here. The rule in question tells us how to

proceed. It directs us to select that point which will render

the sum of the squares of all the distances of the various

shot-marks from it the least possible.[2]

This is merely by way of illustration, and to justify the

familiar designation of the rule. The sort of cases with

which we shall be exclusively occupied are those comparatively

simple ones in which only linear magnitude, or some

quality which can be adequately represented by linear magnitude,

is the object under consideration. In respect of these

the Rule of Least Squares reduces itself to the process of

taking the average, in the most familiar sense of that term,

viz. the arithmetical mean; and a single Law of Error, or its

graphical equivalent, a Curve of Facility, will suffice accurately

to indicate the comparative frequency of the different

amounts of the one variable magnitude involved.

3. We may conveniently here again call attention to a

misconception or confusion which has been already noticed

in a former chapter. It is that of confounding the Law of

Error with the Method of Least Squares. These are things

of an entirely distinct kind. The former is of the nature of

a physical fact, and its production is one which in many

cases is entirely beyond our control. The latter,--or any

simplified application of it, such as the arithmetical average,--is

no law whatever in the physical sense. It is rather a

precept or rule for our guidance. The Law states, in any

given case, how the errors tend to occur in respect of their

magnitude and frequency. The Method directs us how to

treat these errors when any number of them are presented

to us. No doubt there is a relation between the two, as will

be pointed out in the course of the following pages; but

there is nothing really to prevent us from using the same

method for different laws of error, or different methods for

the same law. In so doing, the question of distinct right

and wrong would seldom be involved, but rather one of more

or less propriety.

4. The reader must understand,--as was implied in

the illustration about the pistol shots,--that the ultimate

problem before us is an \_inverse\_ one. That is, we are supposed

to have a moderate number of 'errors' before us and

we are to undertake to say whereabouts is the centre from

which they diverge. This resembles the determination of a

cause from the observation of an effect. But, as mostly

happens in inverse problems, we must commence with the

consideration of the direct problem. In other words, so far

as concerns the case before us, we shall have to begin by

supposing that the ultimate object of our aim,--that is, the

true centre of our curve of frequency,--is already known to

us: in which case all that remains to be done is to study the

consequences of taking averages of the magnitudes which

constitute the errors.

5. We shall, for the present, confine our remarks to

what must be regarded as the typical case where considerations

of Probability are concerned; viz. that in which

the law of arrangement or development is of the Binomial

kind. The nature of this law was explained in Chap. II.,

where it was shown that the frequency of the respective

numbers of occurrences was regulated in accordance with

the magnitude of the successive terms of the expansion of

the binomial (1 + 1)^{n}. It was also pointed out that when n becomes

very great, that is, when the number of influencing

circumstances is very large, and their relative individual

influence correspondingly small, the form assumed by a

curve drawn through the summits of ordinates representing

these successive terms of the binomial tends towards that

assigned by the equation y = Ae^{-h^{2}x^{2}}.

For all practical purposes therefore we may talk indifferently

of the Binomial or Exponential law; if only on

the ground that the arrangement of the actual phenomena

on one or other of these two schemes would soon become

indistinguishable when the numbers involved are large.

But there is another ground than this. Even when the

phenomena themselves represent a continuous magnitude,

our measurements of them,--which are all with which we

can deal,--are discontinuous. Suppose we had before us the

accurate heights of a million adult men. For all practical

purposes these would represent the variations of a continuous

magnitude, for the differences between two successive

magnitudes, especially near the mean, would be

inappreciably small. But our tables will probably represent

them only to the nearest inch. We have so many assigned

as 69 inches; so many as 70; and so on. The tabular

statement in fact is of much the same character as if we

were assigning the number of 'heads' in a toss of a handful

of pence; that is, as if we were dealing with discontinuous

numbers on the binomial, rather than with a continuous

magnitude on the exponential arrangement.

6. Confining ourselves then, for the present, to this

general head, of the binomial or exponential law, we must

distinguish two separate cases in respect of the knowledge

we may possess as to the generating circumstances of the

variable magnitudes.

(1) There is, first, the case in which the conditions of

the problem are determinable \_à priori\_: that is, where we are

able to say, prior to specific experience, how frequently each

combination will occur in the long run. In this case the

main or ultimate object for which we are supposing that the

average is employed,--i.e. that of discovering the true mean

value,--is superseded. We are able to say what the mean

or central value in the long run will be; and therefore there

is no occasion to set about determining it, with some trouble

and uncertainty, from a small number of observations. Still

it is necessary to discuss this case carefully, because its

assumption is a necessary link in the reasoning in other

cases.

This comparatively \_à priori\_ knowledge may present itself

in two different degrees as respects its completeness. In the

first place it may, so far as the circumstances in question

are concerned, be absolutely complete. Consider the results

when a handful of ten pence is repeatedly tossed up. We

know precisely what the mean value is here, viz. equal

division of heads and tails: we know also the chance of six

heads and four tails, and so on. That is, if we had to plot

out a diagram showing the relative frequency of each combination,

we could do so without appealing to experience.

We could draw the appropriate binomial curve from the

generating conditions given in the statement of the problem.

But now consider the results of firing at a target consisting

of a long and narrow strip, of which one point is

marked as the centre of aim.[3] Here (assuming that there

are no causes at work to produce permanent bias) we know

that this centre will correspond to the mean value. And we

know also, in a general way, that the dispersion on each side

of this will follow \_a\_ binomial law. But if we attempted to

plot out the proportions, as in the preceding case, by erecting

ordinates which should represent each degree of frequency

as we receded further from the mean, we should find that

we could not do so. Fresh data must be given or inferred.

A good marksman and a bad marksman will both distribute

their shot according to the same general law; but the

rapidity with which the shots thin off as we recede from the

centre will be different in the two cases. Another 'constant'

is demanded before the curve of frequency could be correctly

traced out.

7. (2) The second division, to be next considered,

corresponds for all logical purposes to the first. It comprises

the cases in which though we have no à priori knowledge

as to the situation about which the values will tend to

cluster in the long run, yet we have sufficient experience at

hand to assign it with practical certainty. Consider for

instance the tables of human stature. These are often very

extensive, including tens or hundreds of thousands. In such

cases the mean or central value is determinable with just as

great certainty as by any \_à priori\_ rule. That is, if we took

another hundred thousand measurements from the same

class of population, we should feel secure that the average

would not be altered by any magnitude which our measuring

instruments could practically appreciate.

8. But the mere assignment of the mean or central

value does not here, any more than in the preceding case,

give us all that we want to know. It \_might\_ so happen that

the mean height of two populations was the same, but that

the law of dispersion about that mean was very different:

so that a man who in one series was an exceptional giant or

dwarf should, in the other, be in no wise remarkable.

To explain the process of thus determining the actual magnitude of the

dispersion would demand too much mathematical detail; but some

indication may be given. What we have to do is to determine the

constant h in the equation[4] y = (h/sqrt{π}) e^{-h^{2} x^{2}}. In

technical language, what we have to do is to determine the \_modulus\_

of this equation. The quantity 1/h in the above expression is called

the modulus. It measures the degree of contraction or dispersion about

the mean indicated by this equation. When it is large the dispersion

is considerable; that is the magnitudes are not closely crowded up

towards the centre, when it is small they are thus crowded up. The

smaller the modulus in the curve representing the thickness with which

the shot-marks clustered about the centre of the target, the better

the marksman.

9. There are several ways of determining the modulus. In the first of

the cases discussed above, where our theoretical knowledge is

complete, we are able to calculate it \_à priori\_ from our knowledge of

the chances. We should naturally adopt this plan if we were tossing up

a large handful of pence.

The usual \_à posteriori\_ plan, when we have the measurements

of the magnitudes or observations before us, is this:--Take

the mean square of the errors, and double this; the result

gives the square of the modulus. Suppose, for instance,

that we had the five magnitudes, 4, 5, 6, 7, 8. The mean of

these is 6: the 'errors' are respectively 2, 1, 0, 1, 2. Therefore

the 'modulus squared' is equal to 10/5; i.e. the modulus is sqrt{2}.

Had the magnitudes been 2, 4, 6, 8, 10; representing

the same mean (6) as before, but displaying a greater dispersion

about it, the modulus would have been larger, viz. sqrt{8}

instead of sqrt{2}.

Mr Galton's method is more of a graphical nature. It

is described in a paper on Statistics by Intercomparison

(\_Phil. Mag.\_ 1875), and elsewhere. It may be indicated as

follows. Suppose that we were dealing with a large number

of measurements of human stature, and conceive that all the

persons in question were marshalled in the order of their

height. Select the average height, as marked by the central

man of the row. Suppose him to be 69 inches. Then raise

(or depress) the scale from this point until it stands at such

a height as just to include one half of the men above (or

below) the mean. (In practice this would be found to require

about 1.71 inches: that is, one quarter of any large

group of such men will fall between 69 and 70.71 inches.)

Divide this number by 0.4769 and we have the modulus. In

the case in question it would be equal to about 3.6 inches.

Under the assumption with which we start, viz. that the law of error

displays itself in the familiar binomial form, or in some form

approximating to this, the three methods indicated above will coincide

in their result. Where there is any doubt on this head, or where we do

not feel able to calculate beforehand what will be the rate of

dispersion, we must adopt the second plan of determining the modulus.

This is the only universally applicable mode of calculation: in fact

that it should yield the modulus is a truth of definition; for in

determining the error of mean square we are really doing nothing else

than determining the modulus, as was pointed out in the last chapter.

10. The position then which we have now reached is this. Taking it for

granted that the Law of Error will fall into the symbolic form

expressed by the equation y = (h/sqrt{π}) e^{-h^{2} x^{2}}, we have

rules at hand by which h may be determined. We therefore, for the

purposes in question, know all about the curve of frequency: we can

trace it out on paper: given one value,--say the central one,--we can

determine any other value at any distance from this. That is, knowing

how many men in a million, say, are 69 inches high, we can determine

without direct observation how many will be 67, 68, 70, 71, and so on.

We can now adequately discuss the principal question of logical

interest before us; viz. \_why\_ do we take averages or means? What is

the exact nature and amount of the advantage gained by so doing? The

advanced student would of course prefer to work out the answers to

these questions by appealing at once to the Law of Error in its

ultimate or exponential form. But I feel convinced that the best

method for those who wish to gain a clear conception of the logical

nature of the process involved, is to begin by treating it as a

question of combinations such as we are familiar with in elementary

algebra; in other words to take a finite number of errors and to see

what comes of averaging these. We can then proceed to work out

arithmetically the results of combining two or more of the errors

together so as to get a new series, not contenting ourselves with the

general character merely of the new law of error, but actually

calculating what it is in the given case. For the sake of simplicity

we will not take a series with a very large number of terms in it, but

it will be well to have enough of them to secure that our law of error

shall roughly approximate in its form to the standard or exponential

law.

For this purpose the law of error or divergence given by supposing our

effort to be affected by ten causes, each of which produces an equal

error, but which error is equally likely to be positive and negative

(or, as it might perhaps be expressed, 'ten equal and indifferently

additive and subtractive causes') will suffice. This is the lowest

number formed according to the Binomial law, which will furnish to the

eye a fair indication of the limiting or Exponential law.[5] The whole

number of possible cases here is 2^{10} or 1024; that is, this is the

number required to exhibit not only all the cases which can occur (for

there are but eleven really distinct cases), but also the relative

frequency with which each of these cases occurs in the long run. Of

this total, 252 will be situated at the mean, representing the 'true'

result, or that given when five of the causes of disturbance just

neutralize the other five. Again, 210 will be at what we will call one

unit's distance from the mean, or that given by six causes combining

against four; and so on; until at the extreme distance of five places

from the mean we get but one result, since in only one case out of the

1024 will all the causes combine together in the same direction. The

set of 1024 efforts is therefore a fair representation of the

distribution of an infinite number of such efforts. A graphical

representation of the arrangement is given here.

[Figure: Binomial distribution for the tenth power.]

11. This representing a complete set of single observations or

efforts, what will be the number and arrangement in the corresponding

set of combined or reduced observations, say of two together? With

regard to the number we must bear in mind that this is not a case of

the combinations of things which cannot be repeated; for any given

error, say the extreme one at F, can obviously be repeated twice

running. Such a repetition would be a piece of very bad luck no doubt,

but being possible it must have its place in the set. Now the possible

number of ways of combining 1024 things two together, where the same

thing may be repeated twice running, is 1024 × 1024 or 1048576. This

then is the number in a complete cycle of the results taken two and

two together.

12. So much for their number; now for their arrangement or

distribution. What we have to ascertain is, firstly, how many times

each possible pair of observations will present itself; and, secondly,

where the new results, obtained from the combination of each pair, are

to be placed. With regard to the first of these enquiries;--it will be

readily seen that on one occasion we shall have F repeated twice; on

20 occasions we shall have F combined with E (for F coming first we

may have it followed by any one of the 10 at E, or any one of these

may be followed by F); E can be repeated in 10 × 10, or 100 ways, and

so on.

Now for the position of each of these reduced observations, the

relative frequency of whose component elements has thus been pointed

out. This is easy to determine, for when we take two errors there is

(as was seen) scarcely any other mode of treatment than that of

selecting the mid-point between them; this mid-point of course

becoming identical with each of them when the two happen to

coincide. It will be seen therefore that F will recur once on the new

arrangement, viz. by its being repeated twice on the old one. G midway

between E and F, will be given 20 times. E, on our new arrangement,

can be got at in two ways, viz. by its being repeated twice (which

will happen 100 times), and by its being obtained as the mid-point

between D and F (which will happen 90 times). Hence E will occur 190

times altogether.

The reader who chooses to take the trouble may work out the frequency

of all possible occurrences in this way, and if the object were simply

to illustrate the principle in accordance with which they occur, this

might be the best way of proceeding. But as he may soon be able to

observe, and as the mathematician would at once be able to prove, the

new 'law of facility of error' can be got at more quickly deductively,

viz. by taking the successive terms of the expansion of (1 + 1)^{20}.

They are given, below the line, in the figure on p. 476.

13. There are two apparent obstacles to any direct comparison between

the distribution of the old set of simple observations, and the new

set of combined or reduced ones. In the first place, the number of

the latter is much greater. This, however, is readily met by reducing

them both to the same scale, that is by making the same total number

of each. In the second place, half of the new positions have no

representatives amongst the old, viz. those which occur midway between

F and E, E and D, and so on. This can be met by the usual plan of

interpolation, viz. by filling in such gaps by estimating what would

have been the number at the missing points, on the same scale, had

they been occupied. Draw a curve through the vertices of the

ordinates at A, B, C, &c., and the lengths of the ordinates at the

intermediate points will very fairly represent the corresponding

frequency of the errors of those magnitudes respectively. When the

gaps are thus filled up, and the numbers thus reduced to the same

scale, we have a perfectly fair basis of comparison. (See figure on

next page.)

Similarly we might proceed to group or 'reduce' three observations, or

any greater number. The number of possible groupings naturally becomes

very much larger, being (1024)^{3} when they are taken three

together. As soon as we get to three or more observations, we have (as

already pointed out) a variety of possible modes of treatment or

reduction, of which that of taking the arithmetical mean is but one.

14. The following figure is intended to illustrate the nature of the

advantage secured by thus taking the arithmetical mean of several

observations.

The curve ABCD represents the arrangement of a given number of

'errors' supposed to be disposed according to the binomial law already

mentioned, when the angles have been smoothed off by drawing a curve

through them. A'CD' represents the similar arrangement of the same

number when given not as simple errors, but as averages of pairs of

errors. A"BD", again, represents the similar arrangement obtained as

averages of errors taken three together. They are drawn as carefully

to scale as the small size of the figure permits.

[Figure: Gaussian distributions for two or three observations.]

15. A glance at the above figure will explain to the reader, better

than any verbal description, the full significance of the statement

that the result of combining two or more measurements or observations

together and taking the average of them, instead of stopping short at

the single elements, is to make large errors comparatively more

scarce. The advantage is of the same general description as that of

fishing in a lake where, of the same number of fish, there are more

big and fewer little ones than in another water: of dipping in a bag

where of the same number of coins there are more sovereigns and fewer

shillings; and so on. The extreme importance, however, of obtaining a

perfectly clear conception of the subject may render it desirable to

work this out a little more fully in detail.

For one thing, then, it must be clearly understood that the result of

a set of 'averages' of errors is nothing else than another set of

'errors,' No device can make the attainment of the true result

certain,--to suppose the contrary would be to misconceive the very

foundations of Probability,--no device even can obviate the

possibility of being actually worse off as the result of our

labour. The average of two, three, or any larger number of single

results, may give a worse result, i.e. one further from the ultimate

average, than was given by the first observation we made. We must

simply fall back upon the justification that big deviations are

rendered scarcer in the long run.

Again; it may be pointed out that though, in the above investigation,

we have spoken only of the arithmetical average as commonly understood

and employed, the same general results would be obtained by resorting

to almost any symmetrical and regular mode of combining our

observations or errors. The two main features of the regularity

displayed by the Binomial Law of facility were (1) ultimate symmetry

about the central or true result, and (2) increasing relative

frequency as this centre was approached. A very little consideration

will show that it is no peculiar prerogative of the arithmetical mean

to retain the former of these and to increase the latter. In saying

this, however, a distinction must be attended to for which it will be

convenient to refer to a figure.

16. Suppose that O, in the line D'OD, was the point aimed at by any

series of measurements; or, what comes to the same thing for our

present purpose, was the ultimate average of all the measurements

made. What we mean by a symmetrical arrangement of the values in

regard to O, is that for every error OB, there shall be in the long

run a precisely corresponding opposite one OB'; so that when we erect

the ordinate BQ, indicating the frequency with which B is yielded, we

must erect an equal one B'Q'. Accordingly the two halves of the curve

on each side of P, viz. PQ and PQ' are precisely alike.

[Figure: Symmetry of a distribution.]

It then readily follows that the secondary curve, viz. that marking

the law of frequency of the averages of two or more simple errors,

will also be symmetrical. Consider any three points B, C, D: to these

correspond another three B', C', D'. It is obvious therefore that any

regular and symmetrical mode of dealing with all the groups, of which

BCD is a sample, will result in symmetrical arrangement about the

centre O. The ordinary familiar arithmetical average is but one out of

many such modes. One way of describing it is by saying that the

average of B, C, D, is assigned by choosing a point such that the sum

of the squares of its distances from B, C, D, is a minimum. But we

might have selected a point such that the cubes, or the fourth powers,

or any higher powers should be a minimum. These would all yield curves

resembling in a general way the dotted line in our figure. Of course

there would be insuperable practical objections to any such courses as

these; for the labour of calculation would be enormous, and the

results so far from being better would be worse than those afforded by

the employment of the ordinary average. But so far as concerns the

general principle of dealing with discordant and erroneous results, it

must be remembered that the familiar average is but one out of

innumerable possible resources, all of which would yield the same sort

of help.

17. Once more. We saw that a resort to the average had the effect of

'humping up' our curve more towards the centre, expressive of the fact

that the errors of averages are of a better, i.e. smaller kind. But it

must be noticed that exactly the same characteristics will follow, as

a general rule, from any other such mode of dealing with the

individual errors. No strict proof of this fact can be given here, but

a reference to one of the familiar results of taking combinations of

things will show whence this tendency arises. Extreme results, as

yielded by an average of any kind, can only be got in one way, viz. by

repetitions of extremes in the individuals from which the averages

were obtained. But intermediate results can be got at in two ways,

viz. either by intermediate individuals, or by combinations of

individuals in opposite directions. In the case of the Binomial Law of

Error this tendency to thicken towards the centre was already strongly

predominant in the individual values before we took them in hand for

our average; but owing to this characteristic of combinations we may

lay it down (broadly speaking) that any sort of average applied to any

sort of law of distribution will give a result which bears the same

general relation to the individual values that the dotted lines above

bear to the black line.[6]

18. This being so, the speculative advantages of one method of

combining, or averaging, or reducing, our observations, over another

method,--irrespective, that is, of the practical conveniences in

carrying them out,--will consist solely in the degree of rapidity with

which it tends thus to cluster the result about the centre. We shall

have to subject this merit to a somewhat further analysis, but for the

present purpose it will suffice to say that if one kind of average

gave the higher dotted line in the figure on p. 479 and another gave

the lower dotted line, we should say that the former was the better

one. The advantage is of the same general kind as that which is

furnished in algebraical calculation, by a series which converges

rapidly towards the true value as compared with one which converges

slowly. We can do the work sooner or later by the aid of either; but

we get nearer the truth by the same amount of labour, or get as near

by a less amount of labour, on one plan than on the other.

As we are here considering the case in which the individual

observations are supposed to be grouped in accordance with the

Binomial Law, it will suffice to say that in this case there is no

doubt that the arithmetical average is not only the simplest and

easiest to deal with, but is also the best in the above sense of the

term. And since this Binomial Law, or something approximating to it,

is of very wide prevalence, a strong \_primâ facie\_ case is made out

for the general employment of the familiar average.

19. The analysis of a few pages back carried the results of the

averaging process as far as could be conveniently done by the help of

mere arithmetic. To go further we must appeal to higher mathematics,

but the following indication of the sort of results obtained will

suffice for our present purpose. After all, the successive steps,

though demanding intricate reasoning for their proof, are nothing more

than generalizations of processes which could be established by simple

arithmetic.[7] Briefly, what we do is this:--

(1) We first extend the proof from the binomial form, with its finite

number of elements, to the limiting or exponential form. Instead of

confining ourselves to a small number of discrete errors, we then

recognize the possibility of any number of errors of any magnitude

whatever.

(2) In the next place, instead of confining ourselves to the

consideration of an average of two or three only,--already, as we have

seen, a tedious piece of arithmetic,--we calculate the result of an

average of any number, n. The actual result is extremely simple. If

the modulus of the single errors is c, that of the average of n of

these will be c ÷ sqrt{n}.

(3) Finally we draw similar conclusions in reference to the \_sum or

difference\_ of two averages of any numbers. Suppose, for instance,

that m errors were first taken and averaged, and then n similarly

taken and averaged. These averages will be nearly, but not quite,

equal. Their sum or difference,--these, of course, are

indistinguishable in the end, since positive and negative errors are

supposed to be equal and opposite,--will itself be an 'error', every

magnitude of which will have a certain assignable probability or

facility of occurrence. What we do is to assign the modulus of \_these\_

errors. The actual result again is simple. If c had been the modulus

of the single errors, that of the sum or difference of the averages of

m and n of them will be c sqrt{1/m + 1/n}.

20. So far, the problem under investigation has been of a direct

kind. We have supposed that the ultimate mean value or central

position has been given to us; either \_à priori\_ (as in many games of

chance), or from more immediate physical considerations (as in aiming

at a mark), or from extensive statistics (as in tables of human

stature). In all such cases therefore the main desideratum is already

taken for granted, and it may reasonably be asked what remains to be

done. The answers are various. For one thing we may want to estimate

the value of an average of many when compared with an average of a

few. Suppose that one man has collected statistics including 1000

instances, and another has collected 4000 similar instances. Common

sense can recognize that the latter are better than the former; but it

has no idea \_how much\_ better they are. Here, as elsewhere,

quantitative precision is the privilege of science. The answer we

receive from this quarter is that, in the long run, the modulus,--and

with this the probable error, the mean error, and the error of mean

square, which all vary in proportion,--diminishes inversely as the

square root of the number of measurements or observations. (This

follows from the second of the above formulæ.) Accordingly the

probable error of the more extensive statistics here is one half that

of the less extensive. Take another instance. Observation shows that

"the mean height of 2,315 criminals differs from the mean height of

8,585 members of the general adult population by about two inches"

(v. Edgeworth, Methods of Statistics: \_Stat. Soc. Journ.\_ 1885). As

before, common sense would feel little doubt that such a difference

was significant, but it could give no numerical estimate of the

significance. Appealing to science, we see that this is an

illustration of the third of the above formulæ. What we really want to

know is the odds against the averages of two large batches differing

by an assigned amount: in this case by an amount equalling twenty-five

times the modulus of the variable quantity. The odds against this are

many billions to one.

21. The number of direct problems which will thus admit of solution is

very great, but we must confine ourselves here to the main inverse

problem to which the foregoing discussion is a preliminary. It is

this. Given \_a few only\_ of one of these groups of measurements or

observations; what can we do with these, in the way of determining

that mean about which they would ultimately be found to cluster?

Given a large number of them, they would betray the position of their

ultimate centre with constantly increasing certainty: but we are now

supposing that there are only a few of them at hand, say half a dozen,

and that we have no power at present to add to the number.

In other words,--expressing ourselves by the aid of graphical

illustration, which is perhaps the best method for the novice and for

the logical student,--in the direct problem we merely have to draw the

curve of frequency from a knowledge of its determining elements;

viz. the position of the centre, and the numerical value of the

modulus. In the inverse problem, on the other hand, we have three

elements at least, to determine. For not only must we, (1), as before,

determine whereabouts the centre may be assumed to lie; and (2), as

before, determine the value of the modulus or degree of dispersion

about this centre. This does not complete our knowledge. Since neither

of these two elements is assigned with certainty, we want what is

always required in the Theory of Chances, viz. some estimate of their

probable truth. That is, after making the best assignment we can as to

the value of these elements, we want also to assign numerically the

'probable error' committed in such assignment. Nothing more than this

can be attained in Probability, but nothing less than this should be

set before us.

22. (1) As regards the first of these questions, the answer is very

simple. Whether the number of measurements or observations be few or

many, we must make the assumption that their average \_is\_ the point we

want; that is, that the average of the few will coincide with the

ultimate average. This is the best, in fact the only assumption we can

make. We should adopt this plan, of course, in the extreme case of

there being only \_one\_ value before us, by just taking that one; and

our confidence increases slowly with the number of values before

us. The only difference therefore here between knowledge resting upon

such data, and knowledge resting upon complete data, lies not in the

result obtained but in the confidence with which we entertain it.

23. (2) As regards the second question, viz. the determination of the

modulus or degree of dispersion about the mean, much the same may be

said. That is, we adopt the same rule for the determination of the

E.M.S. (error of mean square) by which the modulus is assigned, as we

should adopt if we possessed full Information. Or rather we are

confined to \_one\_ of the rules given on p. 473, viz. the second, for

by supposition we have neither the \_à priori\_ knowledge which would be

able to supply the first, nor a sufficient number of observations to

justify the third. That is, we reckon the errors, measured from the

average, and calculate their mean square: twice this is equal to the

square of the modulus of the probable curve of facility.[8]

24. (3) The third question demands for its solution somewhat advanced

mathematics; but the results can be indicated without much

difficulty. A popular way of stating our requirement would be to say

that we want to know how likely it is that the mean of the few, which

we have thus accepted, shall coincide with the true mean. But this

would be to speak loosely, for the chances are of course indefinitely

great against such precise coincidence. What we really do is to assign

the 'probable error'; that is, to assign a limit which it is as likely

as not that the discrepancy between the inferred mean and the true

mean should exceed.[9] To take a numerical example: suppose we had

made several measurements of a wall with a tape, and that the average

of these was 150 feet. The scrupulous surveyor would give us this

result, with some such correction as this added,--'probable error 3

inches'. All that this means is that we may assume that the true value

is 150 feet, with a confidence that in half the cases (of this

description) in which we did so, we should really be within three

inches of the truth.

The expression for this probable error is a simple multiple of the

modulus: it is the modulus multiplied by 0.4769.... That it should be

some function of the modulus, or E.M.S., seems plausible enough; for

the greater the errors,--in other words the wider the observed

discrepancy amongst our measurements,--the less must be the confidence

we can feel in the accuracy of our determination of the mean. But, of

course, without mathematics we should be quite unable to attempt any

numerical assignment.

25. The general conclusion therefore is that the determination of the

curve of facility,--and therefore ultimately of every conclusion which

rests upon a knowledge of this curve,--where only a few observations

are available, is of just the same kind as where an infinity are

available. The rules for obtaining it are the same, but the confidence

with which it can be accepted is less.

The knowledge, therefore, obtainable by an average of a small number

of measurements of any kind, hardly differs except in degree from that

which would be attainable by an indefinitely extensive series of

them. We know the same sort of facts, only we are less certain about

them. But, on the other hand, the knowledge yielded by an average even

of a small number differs in kind from that which is yielded by a

single measurement. Revert to our marksman, whose bullseye is supposed

to have been afterwards removed. If he had fired only a single shot,

not only should we be less certain of the point he had aimed at, but

we should have no means whatever of guessing at the quality of his

shooting, or of inferring in consequence anything about the probable

remoteness of the next shot from that which had gone before. But

directly we have a plurality of shots before us, we not merely feel

more confident as to whereabouts the centre of aim was, but we also

gain some knowledge as to how the future shots will cluster about the

spot thus indicated. The quality of his shooting begins at once to be

betrayed by the results.

26. Thus far we have been supposing the Law of Facility to be of the

Binomial type. There are several reasons for discussing this at such

comparative length. For one thing it is the only type which,--or

something approximately resembling which,--is actually prevalent over

a wide range of phenomena. Then again, in spite of its apparent

intricacy, it is really one of the simplest to deal with; owing to the

fact that every curve of facility derived from it by taking averages

simply repeats the same type again. The curve of the average only

differs from that of the single elements in having a smaller modulus;

and its modulus is smaller in a ratio which is exceedingly easy to

give. If that of the one is c, that of the other (derived by averaging

n single elements) is c/sqrt{n}.

But for understanding the theory of averages we must consider other

cases as well. Take then one which is intrinsically as simple as it

possibly can be, viz. that in which all values within certain assigned

limits are equally probable. This is a case familiar enough in

abstract Probability, though, as just remarked, not so common in

natural phenomena. It is the state of things when we act at random

directly upon the objects of choice;[10] as when, for instance, we

choose digits at random out of a table of logarithms.

The reader who likes to do so can without much labour work out the

result of taking an average of two or three results by proceeding in

exactly the same way which we adopted on p. 476. The 'curve of

facility' with which we have to start in this case has become of

course simply a finite straight line. Treating the question as one of

simple combinations, we may divide the line into a number of equal

parts, by equidistant points; and then proceed to take these two and

two together in every possible way, as we did in the case discussed

some pages back.

If we did so, what we should find would be this. When an average of

\_two\_ is taken, the 'curve of facility' of the average becomes a

triangle with the initial straight line for base; so that the ultimate

mean or central point becomes the likeliest result even with this

commencement of the averaging process. If we were to take averages of

three, four, and so on, what we should find would be that the Binomial

law begins to display itself here. The familiar bell shape of the

exponential curve would be more and more closely approximated to,

until we obtained something quite indistinguishable from it.

27. The conclusion therefore is that when we are dealing with averages

involving a considerable number it is not necessary, in general, to

presuppose the binomial law of distribution in our original data. The

law of arrangement of what we may call the derived curve, viz. that

corresponding to the averages, will not be appreciably affected

thereby. Accordingly we seem to be justified in bringing to bear all

the same apparatus of calculation as in the former case. We take the

initial average as the probable position of the true centre or

ultimate average: we estimate the probability that we are within an

assignable distance of the truth in so doing by calculating the 'error

of mean square'; and we appeal to this same element to determine the

modulus, i.e. the amount of contraction or dispersion, of our derived

curve of facility.

The same general considerations will apply to most other kinds of Law

of Facility. Broadly speaking,--we shall come to the examination of

certain exceptions immediately,--whatever may have been the primitive

arrangement (i.e. that of the single results) the arrangement of the

derived results (i.e. that of the averages) will be more crowded up

towards the centre. This follows from the characteristic of

combinations already noticed, viz. that extreme values can only be got

at by a repetition of several extremes, whereas intermediate values

can be got at either by repetition of intermediates or through the

counteraction of opposite extremes. Provided the original

distribution be symmetrical about the centre, and provided the limits

of possible error be finite, or if infinite, that the falling off of

frequency as we recede from the mean be very rapid, then the results

of taking averages will be better than those of trusting to single

results.

28. We will now take notice of an exceptional case. We shall do so,

not because it is one which can often actually occur, but because the

consideration of it will force us to ask ourselves with some

minuteness what we mean in the above instances by calling the results

of the averages 'better' than those of the individual values. A

diagram will bring home to us the point of the difficulty better than

any verbal or symbolic description.

[Figure: Distribution for two samples from a non-Gaussian distribution.]

The black line represents a Law of Error easily stated in words, and

one which, as we shall subsequently see, can be conceived as occurring

in practice. It represents a state of things under which up to a

certain distance from O, on each side, viz. to A and B, the

probability of an error diminishes uniformly with the distance from O;

whilst beyond these points, up to E and F, the probability of error

remains constant. The dotted line represents the resultant Law of

Error obtained by taking the average of the former two and two

together. Now is the latter 'better' than the former? Under it,

certainly, great errors are less frequent and intermediate ones more

frequent; but then on the other hand the \_small\_ errors are less

frequent: is this state of things on the whole an improvement or not?

This requires us to reconsider the whole question.

29. In all the cases discussed in the previous sections the

superiority of the curve of averages over that of the single results

showed itself at every point. The big errors were scarcer and the

small errors were commoner; it was only just at one intermediate point

that the two were on terms of equality, and this point was not

supposed to possess any particular significance or

importance. Accordingly we had no occasion to analyse the various

cases included under the general relation. It was enough to say that

one was better than the other, and it was sufficient for all purposes

to take the 'modulus' as the measure of this superiority. In fact we

are quite safe in simply saying that the \_average\_ of those average

results is better than that of the individual ones.

When however we proceed in what Hume calls "the sifting humour," and

enquire \_why\_ it is sufficient thus to trust to the average; we find,

in addition to the considerations hitherto advanced, that some

postulate was required as to the \_consequences\_ of the errors we

incur. It involved an estimate of what is sometimes called the

'detriment' of an error. It seemed to take for granted that large and

small errors all stand upon the same general footing of being

mischievous in their consequences, but that their evil effects

increase in a greater ratio than that of their own magnitude.

30. Suppose, for comparison, a case in which the importance of an

error is directly proportional to its magnitude (of course we suppose

positive and negative errors to balance each other in the long run):

it does not appear that any advantage would be gained by taking

averages. Something of this sort may be considered to prevail in cases

of mere purchase and sale. Suppose that any one had to buy a very

large number of yards of cloth at a constant price per yard: that he

had to do this, say, five times a day for many days in succession. And

conceive that the measurement of the cloth was roughly estimated on

each separate occasion, with resultant errors which are as likely to

be in excess as in defect. Would it make the slightest difference to

him whether he paid separately for each piece; or whether the five

estimated lengths were added together, their average taken, and he

were charged with this average price for each piece? In the latter

case the errors which will be made in the estimation of each piece

will of course be less in the long run than they would be in the

former: will this be of any consequence? The answer surely is that it

will not make the slightest difference to either party in the

bargain. In the long run, since the same parties are concerned, it

will not matter whether the intermediate errors have been small or

large.

Of course nothing of this sort can be regarded as the general rule. In

almost every case in which we have to make measurements we shall find

that large errors are much more mischievous than small ones, that is,

mischievous in a greater ratio than that of their mere magnitude. Even

in purchase and sale, where \_different\_ purchasers are concerned, this

must be so, for the pleasure of him who is overserved will hardly

equal the pain of him who is underserved. And in many cases of

scientific measurement large errors may be simply fatal, in the sense

that if there were no reasonable prospect of avoiding them we should

not care to undertake the measurement at all.

31. If we were only concerned with practical considerations we might

stop at this point; but if we want to realize the full logical import

of average-taking as a means to this particular end, viz. of

estimating some assigned magnitude, we must look more closely into

such an exceptional case as that which was indicated in the figure on

p. 493. What we there assumed was a state of things in reference to

which extremely small errors were very frequent, but that when once we

got beyond a certain small range all other errors, within considerable

limits, were equally likely.

It is not difficult to imagine an example which will aptly illustrate

the case in point: at worst it may seem a little far-fetched.

Conceive then that some firm in England received a hurried order to

supply a portion of a machine, say a steam-engine, to customers at a

distant place; and that it was absolutely essential that the work

should be true to the tenth of an inch for it to be of any use. But

conceive also that two specifications had been sent, resting on

different measurements, in one of which the length of the requisite

piece was described as sixty and in the other sixty-one inches. On the

assumption of any ordinary law of error, whether of the binomial type

or not, there can be no doubt that the firm would make the best of a

very bad job by constructing a piece of 60 inches and a half:

i.e. they would have a better chance of being within the requisite

tenth of an inch by so doing, than by taking either of the two

specifications at random and constructing it accurately to this. But

if the law were of the kind indicated in our diagram,[11] then it

seems equally certain that they would be less likely to be within the

requisite narrow margin by so doing. As a mere question of

probability,--that is, if such estimates were acted upon again and

again,--there would be fewer failures encountered by simply choosing

one of the conflicting measurements at random and working exactly to

this, than by trusting to the average of the two.

This suggests some further reflections as to the taking of

averages. We will turn now to another exceptional case,

but one involving somewhat different considerations than

those which have been just discussed. As before, it may be

most conveniently introduced by commencing with an example.

32. Suppose then that two scouts were sent to take the calibre of a

gun in a hostile fort,--we may conceive that the fort was to be

occupied next day, and used against the enemy, and that it was

important to have a supply of shot or shell,--and that the result is

that one of them reports the calibre to be 8 inches and the other 9.

Would it be wise to assume that the mean of these two, viz. 8-1/2

inches, was a likelier value than either separately?

The answer seems to be this. If we have reason to suppose that the

possible calibres partake of the nature of a \_continuous\_

magnitude,--i.e. that all values, with certain limits, are to be

considered as admissible, (an assumption which we always make in our

ordinary inverse step from an observation or magnitude to the thing

observed or measured)--then we should be justified in selecting the

average as the likelier value. But if, on the other hand, we had

reason to suppose that \_whole\_ inches are always or generally

preferred, as is in fact the case now with heavy guns, we should do

better to take, even at hazard, one of the two estimates set before

us, and trust this alone instead of taking an average of the two.

33. The principle upon which we act here may be stated thus. Just as

in the direct process of calculating or displaying the 'errors',

whether in an algebraic formula or in a diagram, we generally assume

that their possibility is continuous, i.e. that all intermediate

values are possible; so, in the inverse process of determining the

probable position of the original from the known value of two or more

errors, we assume that that position is capable of falling at any

point whatever between certain limits. In such an example as the

above, where we know or suspect a discontinuity of that possibility of

position, the value of the average may be entirely destroyed.

In the above example we were supposed to know that the calibre of the

guns was likely to run in English inches or in some other recognized

units. But if the battery were in China or Japan, and we knew nothing

of the standards of length in use there, we could no longer appeal to

this principle. It is doubtless highly probable that those calibres

are not of the nature of continuously varying magnitudes; but in an

entire ignorance of the standards actually adopted, we are to all

intents and purposes in the same position as if they were of that

continuous nature. When this is so the objections to trusting to the

average would no longer hold good, and if we had only one opportunity,

or a very few opportunities, we should do best to adhere to the

customary practice.

34. When however we are able to collect and compare a large number of

measurements of various objects, this consideration of the probable

discontinuity of the objects we thus measure,--that is, their tendency

to assume some one or other of a finite number of distinct magnitudes,

instead of showing an equal readiness to adapt themselves to all

intermediate values,--again assumes importance. In fact, given a

sufficient number of measurable objects, we can actually deduce with

much probability the standard according to which the things in

question were made.

This is the problem which Mr Flinders Petrie has attacked with so much

acuteness and industry in his work on \_Inductive Metrology\_, a work

which, merely on the ground of its speculative interest, may well be

commended to the student of Probability. The main principles on which

the reasoning is based are these two:--(1) that all artificers are

prone to construct their works according to round numbers, or simple

fractions, of their units of measurement; and (2) that, aiming to

secure this, they will stray from it in tolerable accordance with the

law of error. The result of these two assumptions is that if we

collect a very large number of measurements of the different parts and

proportions of some ancient building,--say an Egyptian temple,--whilst

no assignable length is likely to be permanently unrepresented, yet we

find a marked tendency for the measurements to cluster about certain

determinate points in our own, or any other standard scale of

measurement. These points mark the length of the standard, or of some

multiple or submultiple of the standard, employed by the old

builders. It need hardly be said that there are a multitude of

practical considerations to be taken into account before this method

can be expected to give trustworthy results, but the leading

principles upon which it rests are comparatively simple.

35. The case just considered is really nothing else than the

recurrence, under a different application, of one which occupied our

attention at a very early stage. We noticed (Chap. II.) the

possibility of a curve of facility which instead of having a single

vertex like that corresponding to the common law of error, should

display two humps or vertices. It can readily be shown that this

problem of the measurements of ancient buildings, is nothing more than

the reopening of the same question, in a slightly more complex form,

in reference to the question of the functions of an average.

Take a simple example. Suppose an instance in which great errors, of a

certain approximate magnitude, are distinctly more likely to be

committed than small ones, so that the curve of facility, instead of

rising into one peak towards the centre, as in that of the familiar

law of error, shows a depression or valley there. Imagine, in fact,

two binomial curves, with a short interval between their centres. Now

if we were to calculate the result of taking averages here we should

find that this at once tends to fill up the valley; and if we went on

long enough, that is, if we kept on taking averages of sufficiently

large numbers, a peak would begin to arise in the centre. In fact the

familiar single binomial curve would begin to make its appearance.

36. The question then at once suggests itself, ought we to do this?

Shall we give the average free play to perform its allotted function

of thus crowding things up towards the centre? To answer this question

we must introduce a distinction. If that peculiar double-peaked curve

had been, as it conceivably might, a true error-curve,--that is, if it

had represented the divergences actually made in aiming at the real

centre,--the result would be just what we should want. It would

furnish an instance of the advantages to be gained by taking averages

even in circumstances which were originally unfavourable. It is not

difficult to suggest an appropriate illustration. Suppose a man firing

at a mark from some sheltered spot, but such that the range crossed a

broad exposed valley up or down which a strong wind was generally

blowing. If the shot-marks were observed we should find them

clustering about two centres to the right and left of the

bullseye. And if the results were plotted out in a curve they would

yield such a double-peaked curve as we have described. But if the

winds were equally strong and prevalent in opposite directions, we

should find that the averaging process redressed the consequent

disturbance.

If however the curve represented, as it is decidedly more likely to

do, some outcome of natural phenomena in which there was, so to say, a

real double aim on the part of nature, it would be otherwise. Take,

for instance, the results of measuring a large number of people who

belonged to two very heterogeneous races. The curve of facility would

here be of the kind indicated on p. 45, and if the numbers of the two

commingled races were equal it would display a pair of twin

peaks. Again the question arises, 'ought' we to involve the whole

range within the scope of a single average? The answer is that the

obligation depends upon the purpose we have in view. If we want to

compare that heterogeneous race, as a whole, with some other, or with

itself at some other time, we shall do well to average without

analysis. All statistics of population, as we have already seen

(v. p. 47), are forced to neglect a multitude of discriminating

characteristics of the kind in question. But if our object were to

interpret the causes of this abnormal error-curve we should do well to

break up the statistics into corresponding parts, and subject these to

analysis separately.

Similarly with the measurements of the ancient buildings. In this

case if all our various 'errors' were thrown together into one group

of statistics we should find that the resultant curve of facility

displayed, not two peaks only, but a succession of them; and these of

various magnitudes, corresponding to the frequency of occurrence of

each particular measurement. We \_might\_ take an average of the whole,

but hardly any rational purpose could be subserved in so doing;

whereas each separate point of maximum frequency of occurrence has

something significant to teach us.

37. One other peculiar case may be noticed in conclusion. Suppose a

distinctly asymmetrical, or lop-sided curve of facility, such as

this:--

[Figure: An asymmetric (lop-sided) distribution.]

Laws of error, of which this is a graphical representation, are, I

apprehend, far from uncommon. The curve in question, is, in fact, but

a slight exaggeration of that of barometrical heights as referred to

in the last chapter; when it was explained that in such cases the

mean, the median, and the maximum ordinate would show a mutual

divergence. The doubt here is not, as in the preceding instances,

whether or not a single average should be taken, but rather what kind

of average should be selected. As before, the answer must depend upon

the special purpose we have in view. For all ordinary purposes of

comparison between one time or place and another, any average will

answer, and we should therefore naturally take the arithmetical, as

the most familiar, or the median, as the simplest.

38. Cases might however arise under which other kinds of average could

justify themselves, with a momentary notice of which we may now

conclude. Suppose, for instance, that the question involved here were

one of desirability of climate. The ordinary mean, depending as it

does so largely upon the number and magnitude of extreme values, might

very reasonably be considered a less appropriate test than that of

judging simply by the relatively most frequent value: in other words,

by the maximum ordinate. And various other points of view can be

suggested in respect of which this particular value would be the most

suitable and significant.

In the foregoing case, viz. that of the weather curve, there was no

objective or 'true' value aimed at. But a curve closely resembling

this would be representative of that particular class of estimates

indicated by Mr Galton, and for which, as he has pointed out, the

\_geometrical\_ mean becomes the only appropriate one. In this case the

curve of facility ends abruptly at O: it resembles a much

foreshortened modification of the common exponential form. Its

characteristics have been discussed in the paper by Dr Macalister

already referred to, but any attempt to examine its properties here

would lead us into far too intricate details.

39. The general conclusion from all this seems quite in accordance

with the nature and functions of an average as pointed out in the last

chapter. Every average, it was urged, is but a single representative

intermediate value substituted for a plurality of actual values. It

must accordingly let slip the bulk of the information involved in

these latter. Occasionally, as in most ordinary measurements, the one

thing which it represents is obviously the thing we are in want of;

and then the only question can be, which mean will most accord with

the 'true' value we are seeking. But when, as may happen in most of

the common applications of statistics, there is really no 'true value'

of an objective kind behind the phenomena, the problem may branch out

in various directions. We may have a variety of purposes to work out,

and these may demand some discrimination as regards the average most

appropriate for them. Whenever therefore we have any doubt whether the

familiar arithmetical average is suitable for the purpose in hand we

must first decide precisely what that purpose is.

1. Mr Mansfield Merriman published in 1877 (\_Trans. of the Connecticut

Acad.\_) a list of 408 writings on the subject of Least Squares.

2. In other words, we are to take the "centre of gravity" of the

shot-marks, regarding them as all of equal weight. This is, in

reality, the 'average' of all the marks, as the elementary

geometrical construction for obtaining the centre of gravity of a

system of points will show; but it is not familiarly so regarded. Of

course, when we are dealing with such cases as occur in Mensuration,

where we have to combine or reconcile three or more inconsistent

equations, some such rule as that of Least Squares becomes

imperative. No taking of an average will get us out of the

difficulty.

3. The only reason for supposing this exceptional shape is to secure

simplicity. The ordinary target, allowing errors in two dimensions,

would yield slightly more complicated results.

4. When first referred to, the \_general\_ form of this equation was

given (v. p. 29). The special form here assigned, in which

(h/sqrt{π}) is substituted for A, is commonly employed in

Probability, because the integral of y dx, between +infinity and

-infinity, becomes equal to unity. That is, the sum of all the

mutually exclusive possibilities is represented, as usual, by

unity. In this form of expression h is a quantity of the order

x^{-1}; for hx is to be a numerical quantity, standing as it does as

an index. The modulus, being the reciprocal of this, is of the same

order of quantities as the errors themselves. In fact, if we

multiply it by 0.4769... we have the so-called 'probable error.'

5. See, for the explanation of this, and of the graphical method of

illustrating it, the note on p. 29.

6. Broadly speaking, we may say that the above remarks hold good of

any law of frequency of error in which there are actual limits,

however wide, to the possible magnitude of an error. If there are no

limits to the possible errors, this characteristic of an average to

heap its results up towards the centre will depend upon

circumstances. When, as in the exponential curve, the approximation

to the base, as asymptote, is exceedingly rapid,--that is, when the

extreme errors are relatively very few,--it still holds good. But if

we were to take as our law of facility such an equation as

y = π/(1 + x^{2}), (as hinted by De Morgan and noted by Mr Edgeworth:

\_Camb. Phil. Trans.\_ vol. X. p. 184, and vol. XIV. p. 160) it does

not hold good. The result of averaging is to \_diminish\_ the tendency

to cluster towards the centre.

7. The reader will find the proofs of these and other similar formulæ

in Galloway \_on Probability\_, and in Airy \_on Errors\_.

8. The formula commonly used for the E.M.S. in this case is

(sum e^{2})/(n - 1) and not (sum e^{2})/n. The difference is trifling,

unless n be small; the justification has been offered for it that

since the sum of the squares measured from the true centre is a

minimum (that centre being the ultimate arithmetical mean) the sum

of the squares measured from the somewhat incorrectly assigned

centre will be somewhat larger.

9. It appears to me that in strict logical propriety we should like to

know the probable error committed in \_both\_ the assignments of the

preceding two sections. But the profound mathematicians who have

discussed this question, and who alone are competent to treat it,

have mostly written with the practical wants of Astronomy in view;

and for this purpose it is sufficient to take account of the one

great desideratum, viz. the true values sought. Accordingly the only

rules commonly given refer to the probable error of the mean.

10. i.e. as distinguished from acting upon them indirectly. This

latter proceeding, as explained in the chapter on Randomness, may

result in giving a non-uniform distribution.

11. There is no difficulty in conceiving circumstances under which a

law very closely resembling this would prevail. Suppose, e.g., that

one of the two measurements had been made by a careful and skilled

mechanic, and the other by a man who to save himself trouble had put

in the estimate at random (within certain limits),--the firm having

a knowledge of this fact but being of course unable to assign the

two to their authors,--we should get very much such a Law of Error

as is supposed above.

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