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The Development of the Concept of the Best Mean of a Set of Measurements [1]
from Antiquity to the Present Day

by

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1. INTRODUCTION

Presidents of the American Statistical Association in their

presidential addresses commonly review the state of the Association or,

at least, of some particular aspect(s) of the Association at the time of

their presidencies, and then go on to predict future trends. I shall

depart from this custom. I have chosen to talk not about the present

and the future, but rather about how we got to the present from the past.

In particular, I shall consider a central problem of statistical inference,

the choice of the "best" mean of a set of measurements of a single quan-

tity, and shall review practice and theory in this regard, from the dim

past to the confused present. I am taking this opportunity to communicate

to you some of the findings of research that I have been pursuing as a

student of the history of statistics of measurement for a little over a decade.

In its simplest form the problem of choosing the "best" mean is

this:

> Given a number of "equally good" measurements of a single fixed
> quantity, what mean of their values should be taken as the
> best value of the unknown magnitude of this quantity afforded
> by these measurements?

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[1] Presidential Address, 131st Annual Meeting of the American Statistical
Association, Colorado State University, Fort Collins, Colorado,
24 August 1971.

A more general problem is:

> What mean should be taken when the measurements are NOT
> all "equally good"?

I shall be concerned principally with the simpler, more restricted, of

these two questions; but will take up the broader problem from time to

time.

There is an odd peculiarity of much historical writing and speaking

that some of you may have noticed: The length of an historical account,

or the definiteness of an historical statement, often tend to be related

inversely to the amount of solid information available. /   Thus,

I shall devote far more time and words to the early history of my topic

than to more recent developments. This is justified in this instance,

I believe, because many of you have participated in the creation or

sharpening of the most recent developments, which are "well known" to

you and others today; whereas, as I have discovered, the early history

of my topic is not easy to come by, and requires care in interpretation

besides.

2. PRELIMINARY CONSIDERATIONS

2.1 Some General Remarks.

Before we begin our historical journey together, let me set the stage, so to speak, by placing before you two quotations, the substance of which we shall do well to keep in mind as we proceed. The first is a statement by Professor Henry Guerlac of the Department of History, Cornell University, on an irremediable shortcoming of man's historical record:

> To a greater extent than we often realise, what we can know about the past is what our ancestors--the participants in events or those who came soon after--determined that we should know. They placed in the intentional record--in annals, memoirs and commemorative inscriptions--those men and events which appeared to them as exceptional, striking and wholly outside the ordinary dull routine of private existence. In the main, they singled out for preservation in the collective memory those events which they saw to have markedly affected the way of life, the thoughts and actions, of the larger social groups and political entities: a tribe, a city-state, a nation or an empire. So it is that the main scaffolding and framework of our view of history consists of those deeds, thoughts and productions which others besides ourselves deemed worthy of preservation because of their effect upon man in society.

> — Henry GUERLAC [1963], p. 798

The second, attributed to George Bernard Shaw by a speaker at the 50th National Conference on Weights and Measures, elucidates a characteristic of ourselves that handicaps our interpretation of man's historical record:

> The fashion in which we think changes like the fashion of our clothes, and it is difficult, if not impossible, for most people to think otherwise than in the fashion of their own period.

> -- George Bernard Shaw (?)

[M. D. SMITH 1966], p. 145. Thus far I have not succeeded in finding the precise location of this statement in Shaw's writings, or in verifying that Shaw is its author.

Efforts "to think otherwise than in the fashion" of our own time are made all the more difficult by changes in the meanings and usages of particular words. The etymologies of the terms "mean" and "average", which are especially relevant to our present historical journey, provide excellent illustrations. | Today there is a tendency to use these terms more or less interchangeably, but their original meanings were very different.

Another obstacle to sure interpretation of European scientific and technical writings up to the 19th century is the lack of articles ("a" and "the") in Latin. This often makes it impossible to decide for sure whether a stated summary value is "a mean" or "the mean" of the corresponding set of measurements or observations, which are described but not given individually. If only "a mean", it may be simply some subjectively chosen value between the extremes of the set, or it may be some unspecified weighted mean of the individual observation; whereas, if "the mean", it is likely, early in our historical journey, to be the midrange (i.e., the "arithmetic mean" between the extremes), or, in more recent times, the arithmetic mean of all of the observations.

One more general comment. Some wag has remarked: "History is something that never happened, written by someone who was not there". This mischievous remark should, at least, remind us to beware of second- and third-hand accounts. In a study such as ours, we can in effect "be there" when we are able to examine original documents (or facsimile reproductions). But there remain, of course, the possibility of inaccuracy or incompleteness of the record; and of imputing a modern meaning or insight long before its time.

2.2 Means and Averages [a]

The English economist and logician, William Stanley Jevons (1835-1882), complained a century ago that "Much confusion exists in the popular, or even the scientific employment of the terms mean and average, and they are commonly taken as synonymous" ([JEVONS], p. 360). He went on to recommend (pp. 362-363) that in scientific work the term mean be used when referring to an arithmetic mean of a set of measurements of a fixed quantity used as an approximation to the unknown value of this quantity; and the word average, when referring to a "fictitious mean" such as [b] the arithmetic mean of the heights of the houses on a particular street, which serves to give an idea of their heights but may not be the height of any particular house. In both of these cases the expression "arithmetic mean" signifies, of course, the sum of the several individual measurements or heights involved divided by their number. In a few moments I shall point out that this is a comparatively recent extension of the original meaning of "arithmetic mean".

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[a] This subsection should be skimmed or perhaps skipped on first reading. It brings together in one place various facts about means and averages mentioned only briefly or merely alluded to at various points in the oral presentation, together with additional relevant information needed to complete the story.

[b] I am giving here the example used by Adolphe Quetelet (1796-1874) to make the same distinction ([QUETELET 1849], p. 42), because it is much simpler than Jevons's example involving the mean density of the earth. Quetelet elected to reserve the word mean for the first case; and to employ the full expression arithmetic mean in the second.

Dictionaries today seem to be in general agreement that the English word mean derived, through the Middle French meien and moien (antecedent of the modern French moyen), from the Latin medianus ("that is in the middle"), obvious ancestor of the English word, and statistical term, median. As Jevons notes, the English words mean and medium are equivalent. They were often used interchangeably in the 18th century. Thus, James Bradley (1693-1762), the third Astronomer Royal of Great Britain (from 1742), sometimes used "medium" (e.g., [BRADLEY 1832], pp. 151-163) and sometimes "mean" (e.g., pp. 163-168) to designate the arithmetic means of his sets of observations taken at Kew in 1725-1726 -- observations that provided the basis of his discovery (1729) of the aberration of light from stars due to the Earth's motion. The English noun and adjective medium stem directly from the Latin medium, which is both a noun ("the middle") and the neuter form of the adjective medius ("middle", "midmost"); and Greek and Latin dictionaries indicate that medius is "connected with" the Greek adjective μεσος ("middle", "in the middle"), which in turn, Jevons says, "etymologists believe" is connected with the preposition μετα ("in the midst of", "among", "between"), ancestor "of the German mitte, and the true English mid or middle". (Also derived from the Latin medius is the English word mediocre (from the Latin mediocris, which is from medius, "middle", and ocris, "peak"), signifying initially simply "middle of the run", and hence moderate, ordinary, commonplace; but acquiring ultimately a tinge of "sub-ordinary".)

Jevons says little about the history of the English word average, but the Oxford English Dictionary, the first section of which was published two years after his death, gives "c 1500" as the date of

its first appearance in English writings; states that its "derivation is uncertain"; and traces its probable origin back to around 1200 A.D. The corresponding word in French is _avarie_; in German, _havarie_; in Spanish, Portugese, and Italian, _avaria_ -- with some variants of each of these -- all words which made their appearance in the maritime commerce, ordinances, and records of the Mediterranean around 1200 A.D., to signify "a duty, tax, or impost charged upon goods; a customs-duty, or the like". By the time of Columbus's discovery of the New World, these terms had taken on the signification of "any charge over and above the freight incurred in the shipment of goods, and payable by their owner". (This sense is preserved today, in maritime law, in the term _petty average_.) By the time of the coronation of Elizabeth I of England (1558), these terms (_avarie_, etc.) had taken on the meaning of "any expense or loss to owners, arising from damage at sea to the ship or cargo"; and by the time of her death (1603) these terms were used also to refer to "the _equitable distribution_ of expense or loss, when of general incidence, _among all parties concerned, in proportion to their several interests_" (emphasis added). / By 1735 the continental terms and the English _average_ had acquired the transferred meaning: "the distribution of the aggregate inequalities

(in quantity, quality, intensity, etc.) of a series of things among
all members of the series, so as to equalize them, and ascertain their
common or mean quantity, etc., when so treated; a determination or
statement of an <u>arithmetical mean</u>; a <u>medial estimate</u>." (Emphasis added.)
And in Noah Webster's A Compendious Dictionary of the English Language
(first edition, 1806; facsimile reproduction, Bounty Books, a Division
of Crown Publishers, Inc., New York 1970) we find the term <u>average</u>
defined as "a mean proportion, a medium"; and the term <u>medium</u> in turn
defined as "a mean, a middle state or place, a kind of printing paper,
average state".

Thus we see how it came to pass that "the average" in ordinary
usage commonly signifies the arithmetic mean. The signification of
an <u>average</u> "as a single number representing, summarizing, or typifying
a generally prevailing magnitude of a set of numbers of which it is a
function"<u>/</u> -- and thus including the <u>median</u> and the <u>mode</u> of a frequency
distribution, terms coined by Francis Galton in 1883 and Karl Pearson
in 1894, respectively -- is largely a development of the present century.

As further background for our historical study of "mean taking" it
will be well to review briefly the early history, and changes in the
concept of a <u>mean</u> in mathematics.

In the classical Theory of Means of Pythagoras $\underline{\text{(c. 580 - c. 497 B.C.)}}$/and his followers,
an essential feature of a "mean" ("μεσοτης") was <u>intermediacy</u>. But

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<u>/</u>Glenn James and Robert C. James, <u>Mathematics Dictionary</u>, 3rd edition.
Princeton, New Jersey: D. Van Nostrand Company, Inc., 1968.

mere intermediacy was not the whole story: the middle term of a three-term series of numbers arranged in order of magnitude was called a "mean" when, and only when, there was some equal relation between the first two and the second two terms; and the precise form of this "equal relation" characterized the mean involved. Thus, if a, b, and c were three positive numbers with $a > b > c$, the middle number b was said to be the _arithmetic mean_ between a and c, if and only if

(1) $\qquad a - b = b - c.$

In this event the three numbers a, b, c form a (descending) _arithmetic progression_. The Pythagoreans were well aware that (1) implies that

(2) $\qquad b = \frac{1}{2} (a + c) \; ;$

but (1), not (2), was the definition of the arithmetic mean. Similarly, for $a > b > c > 0$, the _geometric mean_ was defined by the relation

$$\frac{a}{b} = \frac{b}{c} \, ,$$

not by the formula $b = \sqrt{ac}$; and in this event the three numbers a, b, c form a (descending) _geometric progression_. (For documentation, and for the original definition of the _harmonic mean_, the third classical mean, see [COHEN and DRABKIN], pp. 6-7, or [HEATH 1963], pp. 51-52.)

Seven additional means were added to the first three by the Pythagoreans from time to time -- three by Eudoxus (408-355 B.C.) or his contemporaries, and four more by "later" Pythagoreans -- making ten in all. All ten can be defined by variants of a single equation, as explained in [HEATH], pp. 52-53. The details of these additional means are of no importance to us, inasmuch as only the original three have survived to the present day. What is important is (a) that a fundamental

characteristic of all of these early "means" is <u>intermediacy</u> between a

"leading" or "<u>a</u>ntecedent" term and a "following" or "<u>c</u>onsequent" term [/] .

of a numerical series -- the exact nature of the "intermediacy" defining

the particular "mean" involved -- and (b), that a "mean" was NOT thought

of as being in any sense <u>representative</u> of, nor as a <u>substitute</u> for,

these other two numbers.

Sometime early in the 16th century -- perhaps earlier, but not much

earlier -- it was recognized that the explicit formula (2) for the

<u>arithmetic mean</u> can be extended readily to the case of <u>n</u> positive

numbers a_1, a_2, ... a_n (positive or negative) in a "natural" way, and

the number m_A defined by this extended definition,

$$(3) \qquad m_A = \frac{1}{n} (a_1 + a_2 + \ldots a_n) = \frac{1}{n} \sum_1^n a_i$$

was called the <u>arithmetic mean</u> OF the <u>n</u> numbers a_1, ..., a_n. It.can be

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[/] Their designation as "extremes" came later.

shown that

$$a_{min} \leq m_A \leq a_{max}$$

where a_{min} and a_{max} denote the smallest and the largest of the a's, respectively, with the equalities holding only when the a's are all equal. Thus <u>intermediacy</u> between the "extremes" is retained by this generalization of the <u>arithmetic mean</u>; [/] and in due course -- I do not know just when -- m_A came to be regarded as <u>representing</u> or <u>typifying</u> the n numbers (usually not all equal) of which it is function, and usable as a <u>substitute</u> for them, individually and collectively, in further calculations. In other words, the generalized <u>arithmetic mean</u>, m_A, defined by (3), became recognized as an <u>average</u> in the modern (mathematical and statistical) sense of the term.

Although computations of the type embodied in (3) are involved in the theory of balances and levers and in determination of <u>centers of gravity</u> (or, <u>centers of mass</u>), which date back to Archimedes (c. 287-212 B.C.) /., as Quetelet ([1849], p. 78 and Note, p. 270) and Jevons ([1958], p. 363) note, <ins>and which are</ins> the archetype of <u>representative</u> or <u>substitutive</u> <u>means</u>, I have never found the expression "arithmetic mean", or even "mean" (or the Greek or Latin equivalents), associated with classical

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[/]But the intimate connection with <u>arithmetic progressions</u> has been lost. Indeed, there is no longer any "progression" involved, unless one imagines the n given numbers a_1, a_2, ..., a_n replaced conceptually by n new numbers α_1, α_2, ..., α_n in arithmetic progression, with $\alpha_1 = a_{min}$ and $\alpha_n = a_{max}$. Such a construct is, however, probably lurking in the background when one divides the elapsed time between an initial occurrence and the nth recurrence of a periodic phenomenon by n and terms the result (a determination of) the <u>mean period.</u>

or medieval discussions of centers of gravity or related topics. On the other hand, as we shall see presently, the expression "arithmetic(al) mean" was certainly used in the generalized sense (3) early in the 17th century, probably in the 16th century. That's why I said above that this generalization (3) dates, I believe, from "sometime early in the 16th century -- perhaps earlier, but not much earlier".

As is well known, the classical explicit formulae for the <u>geometric mean</u> and the reciprocal of the <u>harmonic mean</u> can be generalized similarly to <u>n positive</u> numbers a_1, ..., a_n; and these generalizations (m_A, m_G, and m_H) of the classical arithmetic, geometric, and harmonic means likewise conform to the inequalities

$$a_{min} \leq m_H \leq m_G \leq m_A \leq a_{max} ,$$

where a_{min} and a_{max} denote the smallest and the largest of the a's, respectively, with the equalities holding only when the a's are all equal. Furthermore, for <u>positive</u> numbers a_1, a_2, ..., a_n, these three can be shown to be special cases ($m_A = M_1$, $m_G = \lim_{p \to 0} M_p$, $m_H = M_{-1}$) of the general "power means", $M_p(a_1, a_2, ..., a_n) = [(a_1^p + a_2^p + ... + a_n^p)/n]^{1/p}$, which increase monotonically from $M_{-\infty} = a_{min}$ to $M_{+\infty} = a_{max}$.[/]

Gauss studied [GAUSS 1816] the statistical properties of the power means, $M_p(|e_1|, |e_2|, ..., |e_n|)$, (p = 1, 2, ..., 6), of <u>absolute values of</u> errors, e_1, e_2, ..., e_n -- e.g., the <u>mean absolute error</u> (p = 1) and the <u>root-mean-square-error</u> (p = 2) -- which, always being taken positive, are <u>no longer always intermediate</u> between the extreme errors, one of which will usually be positive and the other negative. These, therefore,

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[/]See, e.g., [HARDY <u>et al</u>], Chapter II, a fact with which Carl Friedrich Gauss (1777-1855) was evidently familiar ([GAUSS 1809], art. 186, 4th para.

constitute a further extension of the concept of a "mean". Accordingly, the English mathematician and logician, Augustus De Morgan (1806-1871), in his article "Mean" in volume 15 (1839) of the Penny Cyclopaedia, dropped the requirement of intermediacy and proposed:

> Generally, let there be a number of quantities
> x_1, x_2, x_3, &c., and let $\phi(x_1, x_2, x_3,$ &c.$)$ be a
> function of them which is symmetrical, that is,
> which is not altered when any two of them are
> interchanged; then if y be found from the equation
> $$\phi(y, y, y, \text{&c.}) = \phi(x_1, x_2, x_3, \text{&c.}),$$
> y may be called a species of mean.
>
> <div align="right">-- Augustus De Morgan, [   ], p. 35</div>

To this the Italian mathematician, Oscar Chisini (1889-   ) added [CHISINI 1929] the "natural" requirement that $\phi(y, y, y,$ &c.$) = y$ -- "natural" because, if our observations should all, by luck, be exactly equal to the "true" or "target" value, we would "naturally" want whatever "mean" we took to yield this same value; and this requirement also insures that the "mean" will be of the same dimensions as the x's. Edward L. Dodd (1875-1943) has provided a convenient summary [DODD 1940] of developments in the mathematical theory (to 1940) of the resulting class of representative or substitutive means, which clearly includes all of the customary statistical means or averages.

This brings the story of means and averages essentially up to the present. Individuals who wish to pursue the matter further will find all -- probably, more than -- they want to know in Le Medie [1958] by the late Corrado Gini (1884-1965).

3. THE STATE OF AFFAIRS FROM ANTIQUITY TO THE 17TH CENTURY A.D.

The taking of some definite mean -- arithmetic mean, mode, median, midrange -- of several measurements of a single$_\wedge$quantity to obtain a
fixed

better value for its magnitude than was afforded by one or another of

the individual measurements does not seem to have become a common

practice until the 17th century A.D., and first appears in the latter

half of the 16th, when a number of examples of the use of the arithmetic

mean to this end can be found. Before the 17th century the picture is

very fragmentary.

3.1 Absence of "mean taking" in ancient science and technology.

When I first began studying this matter, I expected to find a great

many examples in antiquity. I had been brought up on the view that

"astronomy [was] the most important force in the development of science

since its origin sometime around 500 B.C. to the days of Laplace,

Lagrange, and Gauss" ([NEUGEBAUER 1952], p. 2). So I fully expected that

I would find some good examples of mean taking in ancient astronomy;

and, perhaps, also in ancient physics. I have not found any. And I now

believe that no such examples will be found   in ancient science.

The reason is that quantitative science in antiquity was to a

large extent mathematics, and rested little on precise measurement.

Great attention was devoted to mathematical details, and by the Greeks

to mathematical elegance; but the observations involved were usually
—/
quite crude, though very adroitly chosen.

—/ Footnote appears on page 14a.

This important fact had some-
how escaped my attention in my earlier reading. Professors Aaboe and
Price at Yale have brought it out forcefully in a fascinating article
[AABOE and PRICE 1964] subtitled "The derivation of accurate parameters
from crude but crucial observations". They demonstrate how amazingly
accurate numerical values for astronomical parameters could be derived
from single crude but crucial observations corresponding to special
or extreme circumstances that were especially favorable or decisive;
and point out that much ingenuity was employed in the choice of just
what to observe, in the application of the mathematical machinery, and
in the solution of the basically mathematical problems involved.

The general practice in antiquity was to deduce a lot from very few data. For example, Professor Neugebauer has pointed out that tables "for the phenomena of Jupiter" from the late Babylonian period, say 240 to 40 B.C., which were computed several decades ahead, were nonetheless "based on a single observational element, the rest being derived therefrom in a strictly mathematical fashion". "This," he adds, "conforms to a conscious tendency of the ancients to reduce the empirical data to the barest minimum, because they were well aware of the great insecurity of direct observation, especially for such major problems as the date of the first visible crescent or the reappearance of planets. All these phenomena are located near the horizon, where climatic and optical disturbances exercise a most pernicious influence" ([NEUGEBAUER 1954] p. 801) .

And in an earlier article on "Mathematical methods in ancient astronomy" he remarks: "In short, we can say that kinematics and spherical astronomy play a much greater role than empirical observations. The ancient astronomers were fully aware of the fact that the low accuracy of their instruments had to be supplemented by a mathematical theory of the greatest possible refinement. Observations are more qualitative than quantitative: 'when angles are equal' may be

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<u>/</u>Footnote appears on page 16.

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Such practices were not restricted to antiquity. The late Professor

Eva G. R. Taylor (1880-1966) mentions ([TAYLOR 1956], p. 136-137) a

tide-table for "Flod at London", dating from early in the 13th century

A.D. and associated with Matthew Paris (c. 1200-1259 A.D.), which

states that "at new Moon high tide is said to be at 3 hours 48 minutes,

on the second day 4 hours 36 minutes, on the third 5 hours 24 minutes,

and so on". Miss Taylor comments: "It is in fact mechanically built

up from a single observation according to the rule accepted by astronomers

that the daily retardation was 48 minutes. It is a scholar's, not a

sailor's table. When John Flamsteed drew up his tide-table for London

Bridge in 1676 he found the figure highly variable, the retardation

sometimes under 30 minutes, sometimes an hour or more ... theory rather

than observation was still the rule in the learned world of Matthew

Paris's day."

decided fairly well on an instrument but not 'how large' are the angles',

says Ptolemy with respect to lunar and solar diameter (Almagest V, 14 .....).

Consequently, period relations over long intervals of time and lunar

eclipses are the main foundations so far as empirical material is concerned;

all the rest is mathematical theory. ... this holds for Greek as well

as for Babylonian astronomy". ([NEUGEBAUER 1948], p. 1015)

    The procedure. just alluded to that Ptolemy (Claudius Ptolemaeus,

fl. c. 150 A.D.) employed to obtain accurate values for the "apparent

diameters" of the sun and moon provides a helpful illustration of the

technique of supplementing crude observations with "mathematical theory

of the greatest possible refinement". Ptolemy says:

> But constructing ourself the four-cubit rod dioptra described
> by Hipparchus [fl. 161-126 B.C.], and making observations
> with it, we find the sun's diameter everywhere contained by
> very nearly the same angle with no variation worthy of mention
> resulting from its distances. But we find the moon's diameter
> contained by the same angle as the sun's ... only when during
> the full moons it is at its great distance from the earth ...
> it was easy to see when each of the diameters subtends the
> same angle ... But how large they were seemed very doubtful
> to us ... But once the moon at its greatest distance appeared
> to make an angle at the eye equal to the sun's, by means of
> the lunar eclipses observed at that distance we calculated the
> angle subtended by the moon, and immediately we had that of the
> sun also.

> > — Almagest V, 14; English
> > translation from [PTOLEMY (151[+])
> > 1952], pp. 171-172 .

In carrying out the step "by means of the lunar eclipses observed at

that distance we calculated", Ptolemy utilized Babylonian records of

lunar eclipses "at that distance" that took place several centuries

before his time and the whole intricate mathematical machinery of the

then-current theory of the moon's motion; and reached the conclusion

that

> the whole diameter of the moon subtends an arc of a
> great circle amounting to 0°31 1/3' (op. cit., p. 173)

This agrees remarkably well with what I was taught in college, namely,

that "the moon's apparent diameter ranges from 33'30", when nearest,

to 29'21", when most remote" ([RUSSELL, DUGAN, and STEWART 1926, p. 163).

Ptolemy's Almagest is the main source for our knowledge of ancient

astronomy. Ptolemy quotes observations of his own ranging from 127 to

151 A.D. and relies heavily on observations and methods of his prede-

cessors, especially Hipparchus, who flourished almost three centuries

before him, and whose works are lost, possibly partly the result of the

fact that Ptolemy's great book superseded them and made them superfluous.

([NEUGEBAUER 1948, p. 1013; [SARTON 1954]   , p. 41-42)

An extensive and intensive analysis of the quality of the solar

and lunar observations, and of the accuracy of parameters derived

therefrom, in Ptolemy's Almagest has been carried out by John Phillips

Britton under the guidance of Professors Asger Aaboe and Bernard R.

Goldstein of Yale University's Department of History of Science and

Medicine. His findings are reported in detail in his doctoral

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—/The original Greek title translates as Mathematical Syntaxis, or
Mathematical Collection. To distinguish it from lesser astronomical
treatises of other authors, later commentators dubbed it the "Great
Collection", which passed into Arabic as "Al-magisti", i.e., "The
greatest". In time this became "Almagest", by which name it has been
known ever since. ([HEATH (1931) 1963, pp. 402-403)

dissertation [BRITTON 1967], which consists of five chapters and two appendices, plus a Preface and an extensive Bibliography. In the Preface he remarks: "..what is troublesome is that all of Ptolemy's lunar parameters are quite accurate, while the observations from which he derives them are often imprecise and inaccurately reduced. Thus we may ask whether Ptolemy's lunar parameters were derived solely from the observations which he reports or whether some other explanation for the accuracy of these parameters must be found" (p. vii). In Chapter III he investigates the quality of the lunar observations that Ptolemy reports and finds that: "On the whole the errors of the observations agreed well with what we would expect from careful observations made with the techniques available in antiquity. Furthermore, the errors were well distributed with regard to sign and showed no systematic deviation from the modern computations". (pp. vii-viii) In Chapter IV he compares the parameters of Ptolemy's lunar model with their modern equivalents, to determine the errors in Ptolemy's values; and then compares these errors with those that one would expect from the average errors in Ptolemy's reductions of his observations and from the procedures by which he derives his values of these parameters. "For each of the eight parameters so tested [he] found ... Ptolemy's value [to be] significantly more accurate than we would expect". (p. vii). Britton concludes from this "that Ptolemy was not entirely candid in describing the procedures by which he determined his parameters" (p. viii); that he "must have used more sophisticated analyses of observations to obtain his parameter than those which he describes" (Abstract); and adds "the only plausible explanation for the accuracy

of these parameters that I can think of is that they were the result of some form of average of many determinations from a much larger number of observations than Ptolemy describes" (p. ix).

In answer to the question of why Ptolemy described procedures for determining his parameters that were "less sound than those which he actually employed", Britton comments: "... it is my feeling that the Almagest was not intended to be an historical account, but is in many places primarily pedagogic. Furthermore, Ptolemy generally takes great care to make his demonstrations and determinations conform as nearly as possible to the standards of logical rigor encountered in Greek geometry. Hence he might reasonably have concluded that the interests of clarity and rigor were better served by examples of how his results were obtained than by a lengthy, and necessarily non-rigorous, discussion of his procedures for obtaining parameters from discordant observations. One corollary to this conclusion is that Ptolemy almost certainly selected the observations which he reports because they yielded just the values of parameters which he wished to demonstrate. This is not to say that Ptolemy either tampered with the reports of the observations or made intentional errors in their reduction and analysis. Indeed, he would have had no need to do so, since among a large number of determinations a few could be expected to illustrate almost any desired value for a parameter, as long as this value was approximately correct" (pp. ix-x).

In contrast to Britton's study of Ptolemy's lunar and solar observations, where Ptolemy's values for parameters of lunar and solar motion are found to be more accurate than can be accounted for by the

data he presents, is the earlier critique by A. Pannekoek [PANNEKOEK 1955]
of Ptolemy's discussion (<u>Almagest</u> VII, 3) of the precession of the
equinoxes, in which the full set of observations that Ptolemy presents
is shown to be more in keeping with the modern value of the precession
constant than with the value that Ptolemy gives. Pannekoek's comments
on this "problem of Ptolemy's value for the precession" are instructive.
He says: "... the question may be asked: when this set of observations
is so consistent with the modern true data, how could Ptolemy find
therein a confirmation of his far too small constant of precession? The
answer is given by ... the six stars selected by Ptolemy. ... There can
be no doubt that Ptolemy selected these six stars <u>because</u> they were
favourable to his assumed value of the precession and could be quoted
as confirmations, and that the other stars were omitted because they
did not confirm his assumption. Yet we cannot speak of an attempt to
deceive his readers; he presents to them the full material with the
unfavourable cases also. It comes down to saying: 'my result is
confirmed by a number of data; the other data which do not confirm it
do not count'." ([PANNEKOEK 1955], p. 64)

Pannekoek proceeds to give another example of data selection by
Ptolemy, and then comments: "Selection of data in this way is, of
course, strictly condemned by modern scientific standards. In condemning
Ptolemy we should not forget, however, that the principle of selecting
data and rejecting deviating results as unreliable was followed up to
almost modern times; not until the seventeenth and eighteenth century
did it become habitual to derive and use the average of all observed data.
Even in the nineteenth century, scientists felt themselves warranted

in excluding strongly deviating values, and they established an exact criterion [sic] for exclusion." (p. 65).

Britton evidently feels that in some instances Ptolemy used "some form of average of many determinations from a larger number of observations", e.g., the arithmetic mean or midrange of some internal group or "cluster" of observations. Whether he did, and if so, what he did, we shall never know because he did not choose to tell us. My feeling is that if in some instances he did use a "mean" of some sort, this "mean" was not the result of a formal computation such as I indicated above, but rather of selection of a favored value, perhaps, but not necessarily, in the midst of the "bulk" or inner "core" of the observations, and not necessarily one of the observations of the set in hand, and was a "mean" only in that it was between the extremes, and an "average" only in that it was considered to be "typical" or exemplary in some sense.

To this point the discussion has been exclusively in terms of astronomy. This concentration on ancient astronomy stems not from any predilection on my part for astronomy, but simply from the fact that ancient astronomy is where numerical data are to be found. In contrast, the records of ancient physics that have come down to us are devoid of numerical measurements or observations as such. There can be no doubt that the Pythagoreans in the 6th and 5th centuries B.C. (and quite possibly the Babylonians before them ([van der WAERDEN], pp. 94-95) experimented with vibrating strings to establish the relationship between the lengths of the strings and the pitches of the tones emitted by them; that Aristotle,

in the 4th, and Archimedes, in the 3rd century B.C., experimented with levers (and Archimedes, with objects floating or sinking in water), and so forth; but all that has come down to us is the resulting mathematical theory -- see, e.g., the lengthy chapter on Physics (pp. 182-351) in Cohen and Drabkin's Source Book in Greek Science [   ].

At one time some historians of science thought there was a notable exception: the values of the angles of refraction (r) corresponding to angles of incidence (i), at ten-degree intervals from 10 to 80°, for light passing from air to water, from air to glass, and from water to glass, given in Book V of a treatise on Optics attributed (but not with certainty) to the same Ptolemy. These were believed to be actual experimental results. (See Sa rton [1927], p. 274; and his confession of error, [SARTON 1959], p. 57.) But Gilberto Govi, the editor of the first printed edition (1885), noted in his "Introduzione" that the second differences of the reported angles of refraction are constant / and equal to minus one-half degree in all three cases.

/An annotated English translation (based on the Govi edition) of the portions of Book V of interest to us here is available in [COHEN and DRABKIN 1948], pp. 271-281.

To summarize, it needs to be appreciated that observation and theory were differently related to each other in antiquity than they are today. Theory, the creation of the mind, reigned supreme, and observations were not understood as specifying "the facts" to which theory must conform, but rather as particular instances that were useful as illustrative examples in the explanation of theory, or as indicators serving as guideposts in the formulation of theory. (Compare [PANNEKOEK 1961], p. 150; and [SARTON 1959], p. 57.)

To repeat, I have not found in ancient science any example of the computation of a mean of two or more measurements of a single magnitude to obtain a more secure value; and I do not expect that any will be found. Nor have I found any in the records that have come down to us of day-to-day practical affairs.

—[1]/For a while I believed that one had been found. Julian Lowell Coolidge (1873-1954), in his History of Geometrical Methods [COOLIDGE 1940], refers, without precise identification, to a "set" of problems in a 1935 paper of Francois Thureau-Dangin (1872-1944) and comments: "Here a field is divided into rectangles, right triangles and rectangular trapezoids, as described above. The total area is calculated from two different sets of measures, and the divergent results averaged. It is interesting to note that the Babylonians realized that there must be slight errors of observation, and sought means to obviate them" (p. 5). To date, however, I (and a number of others much more expert in such matters than I) have been unable to find any such case as he described. It is certainly not in the paper he cited [THUREAU-DANGIN 1935], which is entirely about evaluation of "volumes". Nor did we find it in Thureau-Dangin's later book [THUREAU-DANGIN 1938]. We also searched without success in the "MKT" and "MCT" collections of Professor Neugebauer [NEUGEBAUER 1935-1937; NEUGEBAUER and SACHS 1945]. We found a number of examples of the evaluation of quadrilateral areas from the product of the mean lengths of opposite sides, i.e., from $\left(\dfrac{a + a'}{2}\right)\left(\dfrac{b + b'}{2}\right)$, which is correct only in the case of a rectangle,

and in other cases gives a value in excess of the correct value; and examples of obtaining a closer approximation to a quantity sought by taking the arithmetic mean between an approximation that is manifestly too large and another that is manifestly too small; but none that seemed to fit Professor Coolidge's description. Professor Coolidge may have been misled by some combination of the above-mentioned procedures.

3.2 An instance of use of the MODE in antiquity.

About 20 years ago Professor W. Allen Wallis

had the good fortune to discover an instance of the use of

the _mode_ of repeated counts in a measurement situation in the Pelo-

ponnesian War between Athens and Sparta (431-404 B.C.), as related by

Thucydides (c. 460-c. 400 B.C.), one of the Athenian commanders:-

> During the same winter [428 B.C.] the Plataeans, who were
> still being besieged by the Peloponnesians and the Boeotians,
> began to be distressed by failure of their supply of food,
> and since there was no hope of aid from Athens nor any other
> means of safety in sight, they and the Athenians who were
> besieged with them planned to leave the city and climb over
> the enemy's walls in the hope that they might be able to
> force a passage. ... They made ladders equal in height to
> the enemy's wall, getting the measure by counting the layers
> of bricks at a point where the enemy's wall on the side facing
> Plataea happened not to have been whitewashed. Many counted
> the layers at the same time, and while some were sure to make
> a mistake, the majority were likely to hit the true count,
> especially since they counted time and again, and, besides,
> were at no great distance, and the part of the wall they wished
> to see was easily visible. The measurement of the ladders,
> then, they got at in this way, reckoning the measure from the
> thickness of the bricks.

> Thucydides, History of the Peloponnesian War, Book III,
> para. 20. English translation by Charles Forster Smith
> [   ], pp. 31, 33; Greek text, pp. 30, 32.

In his elementary textbook written jointly with Professor Harry V.

Roberts [WALLIS and ROBERTS 1956] the Richard Crawley translation (1876)

of this passage is quoted (p. 215) from the Modern Library edition

([   ], pp. 155-156), with the comment:

> The everyday, contemporary version of this application of the
> mode is in checking calculations. If a calculation is repeated
> several times, the value accepted is that which occurs most
> often, not the median, mean, or any other figure. Even in these
> cases, a majority, or some more overwhelming preponderance,
> rather than merely a mode, is usually required for a satisfactory
> decision.

I have quoted a different translation to facilitate comparison and thereby reveal that the essential details are the same in both, although worded slightly differently. _/

This remarkable passage not only describes the earliest known instance of use of the _mode_ of enumerations of the same aggregate by different individuals, or of repeated enumerations by the same individual(s), but also appears to be unique.

_/ Subsequent to my presentation (August 1971) of this address, a third translation, from the Penguin edition (1954), was published (December 1971) by Ernest Rubin in his "Quantitative commentary on Thucydides" ([RUBIN 1971], p. 53); and he mentions additional translations. This is the fifth of a series of articles in The American Statistician in which Dr. Rubin has scrutinized other famous works from the viewpoint of a statistician: Adam Smith's Wealth of Nations (April 1959), Malthus's Essay on Population (Feb. 1960), The Histories of Herodotus (Feb. 1968), and the first volume of Marx's Das Kapital (Apr. 1968). It would be helpful if more classical and other famous works were searched systematically this way for items of interest to statisticians, and the findings in a particular volume or set of volumes published together in a single article, as in these two cases, rather than leaving such items to be discovered and published piecemeal only as they are seen to be relevant to discussion of some particular topic.

During the remainder of 1963 and 1964, I corresponded or talked
with a great many individuals $\underline{a/}$ who might be aware of another (or, other)
recorded instance(s) in antiquity of <u>multiple</u> or <u>repeated counting</u>
followed by adoption of the <u>mode</u>. None could recall another instance,
except in connection with <u>voting</u>, which, of course, involves <u>counting</u>
(and, occasionally, <u>recounts</u>) $\underline{b/}$ and adoption of the <u>mode</u>.

There is thus the nagging inference that this Peloponnesian War
instance of "many" counting, and recounting, the layers of brick, and
adoption of the finding of the majority, i.e., the <u>mode</u>, may be an
instance of the application in an unusual situation of the then-
prevailing rules of conduct in Athens and Sparta which "required that

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$\underline{a/}$ Professors Asger Aaboe (Yale University; ancient and medieval mathe-
matics and astronomy), A.Rupert Hall (Imperial College, London;
history of science and technology), A. H. M. Jones (Cambridge Univer-
sity; ancient history), Tom Bard Jones (University of Minnesota; ancient
history), E. S. Kennedy (American University of Beirut; Arabic mathe-
matics and science), Samuel Noah Kramer (University of Pennyslvania;
Sumerian literature), Richard A. Parker (Brown University; Babylonian
and Egyptian history, science, and mathematics), David Pingree (Oriental
Institute, University of Chicago; mathematics and science in the Near
East and India), Derek J. de Solla Price (Yale University, ancient and
medieval science and technology), Chester G. Starr (University of
Illinois; military history), B. L. van der Waerden (University of Zurich;
Babylonian, Egyptian, and Greek mathematics); and Harry Woolf (Johns
Hopkins University; history of science).

$\underline{b/}$ Professor A. H. M. Jones commented that inasmuch as "the Athenians
voted by a show of hands, ... there was not exactly a recount as
people could change their votes"; and he cited an instance of a second
vote taken in the Athenian Assembly in 406 B.C., described by
Xeonophon (c. 430 - c. 350 B.C.) in his <u>Hellenica</u> (Book I, Chapter
VII, para. 34; see, e.g., [   ], p. 83), in which the second vote,
which reversed the decision, was taken after an objection was made to
the legality of the motion adopted in the first vote. Perhaps some
reader will be able to send me an earlier and "cleaner" example.

almost every important act be directed by a formal vote".$^{/}$ In this

connection, Professor Tom Bard Jones of the Department of History,

University of Minnesota, has drawn my attention to the accounts by

Polybius (c. 205 - 125 B.C.) and Livy (Titus Livius, 59B.C. - 17 A.D.)

of the decisive tactics of the Romans in taking Syracuse in 212 B.C.,

in which ladder-length for scaling the wall was determined from an

estimate of its height based on "counting the courses" -- but, while

Livy's account suggests that there may have been multiple or even

repeated counting, there is absolutely no mention of adoption of the

mode -- quite the contrary! In Polybius's account (Histories, Book

VIII, 37; e.g., [   ], p. 537) it is the Roman general, Marcellus

, who "counted the

courses", and later, on learning that the citizens of Syracuse were

drunk with wine from a three-day celebration of the feast of Artemis

(Diana), "recollected his estimate of the height of the wall at its

lowest point" whereas, according to Livy:-

> ... As they came there repeatedly, one of the Romans, observing
> the wall from near at hand, by counting the courses and making

---

$^{/}$"In ancient Greece and Italy the institution of suffrage already
existed in a rudimentary form at the outset of the historical
period. In the primitive monarchies it was customary for the king
to invite pronouncements of his folk on matters in which it was
prudent to secure its assent beforehand. In these assemblies the
people recorded their opinion by clamouring (a method which survived
in Sparta as late as the 4th century B.C.), or by the clashing of
spears on shields. ... the word suffragium meaning literally a
responsive crash. ... in the days of their full political develop-
ment the communities of these countries [e.g., Athens in the Age of
Pericles, 443-429 B.C.] had firmly established the principle of
government according to the will of majorities, and their constitu-
tions required almost every important act to be directed by a formal
vote."

— Encyclopedia Brittanica, 11th edition, "Vote and Voting",
vol. 28, p. 216.

his own estimate of the height of each on its face, measured
the height of the wall as nearly as he could by guesswork. And
thinking it considerably lower than his own previous estimate
of it and that of all the rest, and that it could be scaled by
ladders even of moderate length, he reported to Marcellus. ...

> — Livy, Annals of the Roman People, Book XXV, 23. English
> translation by Frank Gardner Moore [   ], p. 431;
> Latin text, p. 430.

The comment "and that of all the rest" certainly implies that others

had made estimates of the wall's height too, but not necessarily "by

counting the courses"; and the word "that" certainly suggests a single

value agreed upon by "the rest", but not necessarily the mode of their

estimates -- maybe just a compromise value.

I am not sure what we can conclude from all this beyond the

following: there may well have been other instances in antiquity of

multiple or repeated counting followed by taking the mode as the "true"

value; but, in view of Professor Guerlac's comments quoted earlier, such

instances, if any, were not likely to be recorded for posterity unless

some great good or evil ensued that made the event worthy of notice --

in the above two instances, the "side" telling the story "succeeded"!

3.3 The MIDRANGE as the predecessor to the arithmetic mean.

If there was a predecessor to the arithmetic mean as "best mean"
-- there certainly was no commonly employed predecessor -- but if there
was a predecessor at all,   it must have been the
midrange.

<u>Al-Bīrunī (11th century A.D.)</u>. Al-Bīrunī (Abū Rayhān Muhammud
ibn Ahmad al-Bīrunī, 973-1050[+] A.D.), perhaps the greatest scientist
_a/_
of his day, and one of the greatest of all time, refers to computation
of the midrange of a set of measurements as if this were the customary
"rule" in his day. In one instance he attributes this "rule" to
Ptolemy:--

> In the first treatise of the Almagest, Ptolemy stated that,
> for several years in succession, he observed [the sun's zenith
> distance on the meridian at the times of the solstices] ....
> At all times, he found it (the arc between the two solstices)
> to be forty-seven degrees, and more than two thirds, but less
> than three quarters of a degree. He assumed that this amounts
> approximately to what Eratosthenes had said, which was accepted
> by Hipparchus. <u>He said that because the rule is - for such a
> range with an upper limit and a lower one - to take the average
> amount between them.</u> Hence the amount (MS 78) given by Ptolemy
> is 47;42,30°; its half is 23;51,15°, but he constructed the
> tables of declination on the basis of 23;51,20°, in agreement
> with that assumed by Hipparchus and Eratosthenes, for if their
> third parts are rounded off, the declination comes to be this
> amount.
>
>> -- al-Bīrunī, <u>Tahdīd ... al-Amākin</u>, Chapter III (1018 A.D.);
>>   translation by Jamil Ali [   ], pp. 59-60.
>>   Emphasis added.

Ptolemy did not mention any such rule, or "give" the value quoted.

All that Ptolemy said was:--

_a/_ Al-Bīrunī ("The Master") wrote important treatises on astronomy,
  geodesy, mathematics, mechanics, mineralogy, and pharmacology, among
  others. For a full and interesting biography, see [E. S. KENNEDY 1970].

Now from such observations [of the sun's zenith distance on the meridian] and especially from those made by us over several periods while the sun was near the tropics ... we found the arc from the northernmost to the southernmost limit ... to be always more than 47°40' but less than 47°45'. And with this results nearly the same ratio as that of Eratosthenes and as that which Hipparchus used. For the arc between the tropics turns out to be very nearly 11 of the meridian's 83 parts.

> -- Ptolemy, Almagest I, 12; Taliaferro translation [   ], p. 26.

Thus we see that al-Bīrunī "put words into the mouth" of Ptolemy that the latter never uttered. $\underline{a/}$

The midrange, or "average amount between" the limits that Ptolemy gives, is 47°42'30", as stated by al-Bīrunī. Also 11/83 of 360°, the value of the double obliquity (2e) that Ptolemy says was given by Eratosthenes (c. 273 - 192 B.C.) and adopted by Hipparchus (c. 162 - c. 127 B.C.), is equivalent to 47°42'39"2"' = 2(23°51'19"31"'); and Ptolemy actually used 23°51'20" (one-half of the foregoing rounded up) in the construction of his Table of Obliquity (Almagest I, 15; e.g., [   ], p. 31, entry in Table for 90°), thereby saying, in effect, that by his own observations he has confirmed this esteemed or traditional

---

$\underline{a/}$
Professor Neugebauer told me in the spring of 1966 that he knew of no edition of the Almagest that contains the rule statement that al-Bīrunī attributes to him; but that it is possible that there once existed, and may still exist somewhere, an Arabic version of the Almagest containing such a statement, because the Arabic scribes and scientists were prone to "improving" manuscripts they copied. Professor Neugebauer was quite positive that there was no such "rule" in use in Ptolemy's day. Inasmuch as it was evidently in vogue, or at least recognized, in al-Bīrunī's time, an interesting unresolved question is: when and where did this rule originate during the intervening 900 years?   I understand that there are a number of extant Arabic manuscript copies of the Almagest that date from this intermediate period, so a partial answer to this question may be possible.

value. Britton has provided a fresh translation of the above passage

([   ], pp. 1-2), and has subjected Ptolemy's determination of the

obliquity of the ecliptic to a detailed critique ([   ], Chapter One).

Britton notes that Simon Newcomb's analysis (1895) of the decrease of

the obliquity from antiquity to modern times gives 23°40'40" as the

value of the obliquity (e) in Ptolemy's day, so that the value Ptolemy

used was too large by 21'20", and his error would have been reduced,

but only slightly (by only 5"), had he used one-half of the midrange

value for the double obliquity, namely, e = 23°51'15".

In Chapter V of his Tahdīd... , al-Bīrunī again states the midrange

rule and explains its purpose:--

> As to the halving of the interval between the two times, it
> is a rule of procedure which has been adopted by calculators
> for the purpose of minimizing errors of observation so that
> the time calculated will be between the upper and lower bounds.
>   -- al-Bīrunī, Tahdīd ..., Chapter V (before 1025 A.D.);
>   translation from [   ], p. 168.

This statement constitutes al-Bīrunī's explanation of an instruction

in a work on determination of longitudes by Habash al-Hasib ("Habash

the Computer"), who flourished around 860 A.D.

Al-Bīrunī mentions the "rule" again in Treatise VI, Chapter 2

of his astronomical encyclopedia, al-Qānūn al-Masūdī (Canon of Mas'ud), [a]/

so-called because al-Bīrunī dedicated it to Sultan Mas'ud of Ghazna (now

Ghazni, in east central Afghanistan) on his accession to the sultanate

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[a]/ The Arabic text of the entire Qānūn was published recently in three
volumes [   ]. A German translation of Chapter 2 (of the 6th
Treatise), by C. Schoy, appeared in 1923 ([   ], pp. 56-64); and an
English translation by J. H. Kramers, in 1951 ([   ], pp. 179-185).

in 1031 A.D. following the death of his father, Sultan Mahmūd of

Ghazna (971? - 1030), one of the greatest Muslim conquerors, and

founder of the Ghaznavid dynasty. In sections (8) - (10) of Chapter 2,

al-Bīrunī determines the longitude of Ghazna by traverse from Shiraz

(in southwestern Iran today); and in sections (10) - (13), by traverse

from "al-Jurjānīya in Khwārizm" (i.e., from the modern city of Kunya-

Urgench, northwest of Khīva, in Turkmen S.S.R.). He then says:--

> So the longitude of Ghazna, according to this computation,
> is 98°44'2". The result of the computation from the side
> of Shiraz was 94°54'26". Half the sum of both these figures,
> according to the rule (rasm) of the arithmeticians, is
> 94°19'14".
>
> > -- al-Bīrunī, Qānūn VI, 2, (13); English translation from
> > [   ], p. 182; corresponding German translation in
> > [   ], p. 61.

Although computation of "half the sum of both these figures" would

usually be regarded today as evaluation of the arithmetic mean of the

two given values, we must not forget that the early Greeks, and probably

also scientists of al-Bīrunī's day, thought of it as evaluation of the

arithmetic mean between the two given values , which, when there are

only two values given, are the "upper and lower bounds". The above

quotation from the Qānūn, and the second ("Chapter V") quotation from

the Tahdīd, each by itself might easily be construed as evidence of

use of an arithmetic-mean rule in a degenerate case of only two

observations, but, taken in conjunction with the first ("Chapter III")

quotation from the Tahdīd, must be interpreted, I believe, as additional

evidence for the existence of a MIDRANGE RULE in the 9th - 11th centuries A.D. __/

Al-Bīrunī does not mention the "rule" in his treatise on specific

gravities of metals and precious stones[_/], but it seems, from his

wording and/the/numerical values given that he did use <u>midranges</u> in

summarizing some but not all of his specific gravity determinations.

It will suffice to quote what he says for the first three metals only:--

> First: Gold. I purified it by means of sharp drug agents five
> times until its melting was difficult, its solidification faster,
> and its adhesion to the touchstone was little. Then I probed it
> ten times with different weights testing some according to others
> by converting them [i.e., each one] to a single standard which
> is one hundred <u>mithqals</u>. The check with water differed with
> the highest precision in the work. All of them fell between
> five <u>mithqals</u>, one <u>daniq</u>, and one <u>tasuj</u>, and five <u>mithqals</u>, and
> one <u>daniq</u> and one <u>tasuj</u> and five-sixths of a <u>tasuj</u> and one-half of
> one-sixth of a <u>tasuj</u>. It was wise, of necessity, that one be
> guided between the two limits - five <u>mithqals</u>, one <u>daniq</u> and two
> <u>tasujes</u>. Consideration is taken of the moisture, on the spout of
> the instrument, which did not drip down.

[_/]My interest in this treatise was aroused -- and I was introduced to
al-Bīrunī -- by the description of the apparatus used by al-Bīrunī and
the summary of results obtained therewith on pages 55-56 of the partial
English translation [al-KHAZINI 1865] of the "Book of the Balance of
Wisdom" (<u>Kitāb mīzān al-hikma</u>) by al-Khāzinī of Merv, written in 1121-
1122 A.D., to which my attention had been drawn by an article by
H. J. J. Winter [1956]. Professor E. S. Kennedy suggested that it
would be a good idea to take a look at al-Bīrunī's <u>Maquala fi'l-nisab</u>
<u>allati bayn al-filizzat wa'l-jawahir fi'l-hajm</u> (Treatise on the ratios
between the volumes of metals and jewels), that is available to us
today only through photographs, made in 1912 of an Arabic manuscript
copy, which are now included in the Arabic Manuscript Collection MS 223
of the Bibliothèque Orientale, Université St. Joseph, Beirut, Lebanon.
With his assistance I was able to obtain a microfilm copy of the MS 223
collection, which also contains an incomplete Arabic manuscript copy
of al-Khāzinī's "Book ...". A printed edition of the latter in Arabic,
with text collated from several manuscripts, was published in 1940 by
the Osmania Oriental Publications Bureau, Osmania University, Hyderabad-
Dn., India. The late Professor Martin Levey (1913-1970) kindly pro-
vided me with an English translation of the relevant portions of the
Osmania edition of the al-Khāzinī "Book ..."; and a comparison in
parallel columns of the corresponding portions of the al-Bīrunī and
al-Khāzinī texts on the microfilm. I hope to make more of this material
available in some later publication.

Second: Mercury. [He purified it by passing it through several thicknesses of cloth.] Then I put it in the instrument several times and I converted [i.e., adjusted] the quantities to one hundred. The first of the bounds of its water was seven mithqals, one daniq, and one tasuj and one-quarter of a tasuj. The last was seven mithqals, two daniqs, two tasujes and five-sixths of a tasuj. Most of them are agreeable as to seven mithqals, two daniqs, and a tasuj. So we took it.

Third: Lead. [He describes how he purified it.] The first of the bounds of the water for the hundred was eight mithqals, four daniqs and a tasuj, and the last, nine mithqals. So you take a value between them which is eight mithqals and five daniqs.

Using the relation 1 mithqal = 6 daniqs and 1 daniq = 4 tasujes, the midranges in these three cases are found to be --

gold: 5m, 1d, $1\frac{11}{24}$ t

mercury: 7m, 2d, $\frac{1}{24}$t

lead: 8m, 5d, $\frac{1}{2}$t

In the cases of gold and lead, both the phraseology and the numbers indicated seem to support the view that the result given is the midrange. (In the gold case he gives his reason for rounding up.) In contrast, the phraseology of the last sentence in the mercury case suggests that a preponderance of the (unstated number of) results clustered around the chosen summary value, which is NOT the midrange of all, but may be the midrange of an inner cluster, or, perhaps, the mode.

Assaying, to 1600.

Only single determinations are mentioned in the discussion of coin assaying in the so-called Dialogus de Scaccario ([ ], pp. 36-43), an account, by Richard Fitzneale (? - 1198), treasurer of England and Bishop of London, of the procedures followed by the Exchequer in his day, in which he insists that the coin samples chosen for assay be well mixed so that they may "answer to the weight". Mention is made of duplicate and triplicate assays in the recently published 13th century English Mint Documents from the Red Book of the Exchequer ([ORESME 1956], pp. 50-96) but no midrange or mean taking -- rather "judgement should always be given for the assay which weighs the heaviest ... because silver can easily be lost and can never be gained in the fire, so that judgement must be given where most silver is found" (p. 82). No relevant details of assaying procedure are to be found in De Moneta [ ], the treatise on money and coinage by the great French philsopher and scientist, Nicole Oresme (1323 - 1382). In the Probierbuchlein (1520[+]), "the first printed work on any aspect of metallurgy" ([ ], p. 8), it is recommended that assays always be run in duplicate or triplicate "in order to be safe" ([ ]), para 45) -- one assay may fail, or even two, but hopefully not all three (cf. [Agricola], p. 241). In the

Pyrotechnia (1540) of Vannoccio Biringuccio (1480-1539), the wording

([   ], p. 139) suggests that assays are to be made at least in

duplicate. But nothing is said in either of these works about what is

to be done if duplicate (or triplicate) results do not (all) agree.

The implication is that, if the assays are successfully carried out,

the resulting "beads" of pure silver will weigh exactly the same. This

is explicitly stated in De Re Metallica (1556) of Georgius Agricola

(Georg Bauer, 1494-1555): "If neither [bead] depresses the pan of the

balance in which it is placed, but their weight is equal, the assay has

been free from error; but if one bead depresses its pan, then there is

an error, for which reason the assay must be repeated" ([AGRICOLA 1912],

p. 252). A similar statement appears in the Beschreibung allerfürnemsten

mineralischen Ertzt unnd Berckwercksarten (1574) of Lazarus Ercker

(? - 1593) -- see [   ], p. 52 -- who, except in a special circum-

stance to be noted later, advises against averaging: "If, however, one

of the assays contained much of the thin [penny] pieces and the other

one much of the thick ones, the beads will not balance. ... Some assayers

do not consider this very important and when the beads do not balance

they use the average [mittel]. It is better, however, to be careful

in weighing out the assays and to make an effort to have the beads

balance nicely" ([   ], pp. 61-62). In short, except as just noted,

I have not found any evidence of a midrange (arithmetic or median) practice

or rule in writings on assaying dating from the 12th through the 16th

centuries A.D.

Astronomy, to 1600. To date I have not examined any European works on astronomy prior to the great De Revolutionibus Orbium Coelestium ("On the Revolutions of the Celestial Orbs", 1543) by Nicolaus Copernicus (1473-1543), canon of the cathedral at Frauenberg in East Prussia (Poland), which changed the course of astronomy. I have looked through the "Great Books" English translation [C 1952] several times without finding any instance or mention of taking a "mean" of any kind (midrange, arithmetic mean, etc.) of two or more determinations of a single quantity. At one point in Book VI, Chapter 7 ([C 1952], p. 287) Copernicus does recommend using the midvalue between the extremes of a variable astronomical quantity, to minimize computational labor and simplify exposition, "where indeed the extremes would not have made any manifest difference" ("ubi enim extrema non fecerint apertam differentiam" [? 1949], p. 430); but this, of course, is not the same as taking the midrange of multiple determinations of a single fixed quantity in order to obtain a more secure value.$\frac{a/}{}$

$\frac{2/}{}$ In his calculation of the angle of obliquation of Venus (loc. cit.), Copernicus adopts Ptolemy's values (Almagest XIII, 4; [?], p. 449) for Venus's distances at apogee and perigee, but in transformed units, so that "the greater distance, at apogee, is 10,208", "and the lesser, at perigee, is 9792", "and also the mean distance is 10,000" ("atque inter has media partium 10,000"), and then comments ([?], fol. 205 verso; with spelling clarified and punctuation added, [?], p. 430, and [?], p. 390):

> ... quam assumi in hanc demonstrationem placuit Ptolemaeo volenti consulere difficultati et sectanti, quantum licit, compendia, ubi enim extrema non fecerint apertam differentiam, tutius erat medium sequi.

The "Great Books" English translation of this is ([?], p. 827):

> ... which Ptolemy decided to assume in this demonstration, as he wished to avoid labour and difficulty and to make an epitome, for where the extremes do not cause any great difference, it is better to use the mean.

(Footnote continued below text on next page.)

Before leaving Copernicus, let me note that he also wrote a treatise on coinage, _Monete Cudende Ratio_ (1526; first printed in 1816), at the invitation of the king of Poland to serve as a basis for currency reform in the Prussian states of Poland. I have examined a French translation [℃ /$ ⅝4] and found nothing on assaying procedure and no mention of "mean taking" of any kind.

To my great disappointment, I have not to date found in astronomical writings any clear-cut (or even probable) examples of the use of the midrange of more than two measurements of a single quantity as a more secure value, from al-Bīrunī's day up to 1650 A.D., say, by which time the arithmetic mean was unquestionably in use.[a/] But I have found some later (at least probable) examples:

While this is not a strictly accurate translation, it is in no way misleading. In contrast, the Russian translation (Moscow, 1964) quoted by O. B. Sheynin ([1965c] sec. 4, third para.; and [1966b], third para.) renders "extrema" as "extreme limits of measurements" ("kraynimi predelami izmereniy"; emphasis added). This, especially when read out of context, gives the mistaken impression that Copernicus recommended use of midranges of measurements.

There is one more point of interest here: while Ptolemy uses the middle value (60^p in his units) in his "demonstration", he does so without comment. The explanation is all Copernicus's own addition.

[a/] I am deferring to the next section discussion of the "mean taking" practices of Tycho Brahe (1546-1601), "the prince of astronomical observers" ([MORE], p. 270), because some of his data and associated summary values that I have examined fall among the ambiguous cases that I have come upon in the period 1550-1650 -- "ambiguous" because the arithmetic mean is NOT mentioned explicitly and the summary value given, in consequence of being recorded more coarsely than the individual observations, could be a rounded value of more than one of the simple "averages" (arithmetic mean, median, midrange, or mode).

a. <u>1671 (Flamsteed)</u>. Here is a statement by John Flamsteed (1749-1719), written only a few years before he became the first Astronomer Royal (1675-1719) of Great Britain:

> ... by several observations made yesterday morn, and
> the morning and night preceding, [the moon's] semidiameter
> exceeded not 16'53", nor was less than 16'47", so that for
> diverse very good reasons, too many to be rendered here,
> I determine 16'50", which I intend to compare with ...

> From letter dated Nov. 8, 1671 from John Flamsteed to
> John Collins, secretary of the Royal Society (quoted
> from [RIGAUD & RIGAUD], Vol. II, p. 125).

Francis Baily (1774-1844), Flamsteed's official biographer, remarked that Flamsteed "does not appear to have taken the <u>mean</u> of several observations for a more correct result: since we find that, where more than one observation of a star has been reduced, he has generally assumed that result which seemed to him most satisfactory at the time, without regard to the rest" ([BAILY 1835], p. 376), and went on to support this statement in great detail, <u>a/</u> Nonetheless, the fact that 16'50" is exactly the midvalue between 16'47" and 16'53", <u>and</u> his somewhat evasive wording, together indicate/that , I believe, in this instance Flamsteed adopted the value 16'50" because it is the <u>midrange</u>. On the other hand, I cannot exclude the possibility that 16'50" may have been one of the actual observations and that he selected it because it "seemed to him [the] most satisfactory at the time". The "evasive

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<u>a/</u> Baily is extremely positive about this, but Plackett ([1958], p. 133) has given an extract from Flamsteed's posthumously published <u>Historia Coelestis Britannica</u>, Vol. 3 (1725), in which a result stated to be the "Media inter has Differentia" is exactly the <u>arithmetic mean</u> of the three preceding observations. Unless it can be shown that the "mean taking" is ascribable to the editors, this constitutes a clear-cut counter-example to Baily's "general" dictum.

wording" could also be construed to support this inference. If the

"several observations" mentioned but not given in the letter are extant,

the lack of a 16'50" among them would support my inference;

and confirm it, if neither the arithmetic mean, median, or mode is

equal to 16'50". Otherwise, the issue is irresolvable.

b. <u>1738 (Whiston)</u>. A similar example is provided by the following

excerpt from a book by William Whiston (1667-1752), who served as the

deputy of Sir Isaac Newton (1642-1727) in the Lucasian chair of mathe-

matics at Cambridge University from 1699, and in 1703 succeeded Newton

as Lucasian Professor.

> Now if we suppose, with Mr. <u>Flamsteed</u>, and Sir <u>Isaac</u>
> <u>Newton</u> formerly, that the Sun's Parallax is no more
> than 10", the Earth's distance from the Sun will be
> about 81,000,000 measured Miles, and ... But if, with
> Sir <u>Isaac Newton</u> at last, we take that Parallax to be
> 10 1/2" (which is the mean between Mr. Pound's many
> and most accurate Observations, which always proved to
> be between 9" and 12"), the Earth's distance from the
> Sun will be but 77,000,000, and ...

> Whiston, <u>The Longitude Discovered</u> (1738; [   ], p. 16)

The wording here is not at all evasive, but rather quite specific: it

says to me that Mr. Whiston believes (or knows) that Sir Isaac in his

latest work adopted the value 10 1/2" because it is the midvalue between

the extremes of "Mr. Pound's many and most accurate Observations".<u>a/</u>

<u>a/</u> "Mr. Pound" is the Reverend James Pound (1669-1724), rector of Wanstead
in Essex, and one of the best astronomical observers of the time, who
in 1719, made "many ... accurate Observations" with a long telescope
having an objective lens of 123-foot focal length loaned by the Royal
Society of London, and mounted in Wanstead Park on a maypole that Sir
Isaac had procured for this purpose. In the third edition (1726) of
his <u>Principia</u>, Newton incorporated a number of revisions based on
Mr. Pound's many and most accurate Observations" -- see, e.g.,
[NEWTON 1934], p. 637, Note 9, and p. 662, Note 39.

Navigation to 1600. Practically everything that is known about
the history of navigation among Mediterranean and Western European
peoples from antiquity to the close of the 18th century is contained
in The Haven-Finding Art [TAYLOR 1956] by the late E. G. R. Taylor,[a]
"a history of navigation 'from Odysseus to Captain Cook', which in both
scope and authority is perhaps the most definitive book of its kind".
A useful adjunct is The Art of Navigation in England in Elizabethan
and Early Stuart Times [WATERS 1958] by D. W. Waters,[b] by virtue of its
precise notes and references, and detailed appendices.[c] From a study
of the early chapters of these volumes it is clear that navigational
practices on the part of Mediterranean and Western European seamen were
completely non-numerical up to only a few years before Christopher
Columbus's first voyages to America. Consequently I consider it prac-
tically certain that no "mean taking" of any kind was used by these
seamen before the close of the 15th century.

It was the far-flung ocean voyages of the Portuguese in the 15th
century that focused attention on the need for dependable methods for

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[a] Eva Germaine Rimington Taylor (1879-1966), Emeritus Professor of
Geography, University of London.

[b] David Watkin Waters, Lt. Cdr., R.N., and British Admiralty Historian.

[c] A concise yet remarkably complete chronological summary of the prin-
cipal elements of the "Three Ages of Pilotry" ("primitive", "quanti-
tative", and "mathematical" navigation) in "the western part of the
Old World" has been provided by Joseph Needham in Volume 4, Part III
of his monumental treatise Science and Civilization in China
(NEEDHAM 1971], pp. 554-560).

determining a ship's position at sea.[a/] The first step toward mathe-
matization of navigation was astronomical but non-numerical. As the sea
captains of Portugal's Prince Henry the Navigator (1394-1460) sailed
south along the West Coast of Africa, they saw the North Star drop lower
and lower in the northern sky behind them and used its altitude as an
indication of their distance south of Lisbon. Prince Henry's learned
advisors formalized this procedure and introduced the new navigational
technique of _instrumental observation_ of the _altitude_ ("altura") of the
North Star.[b/] "But ... at first, as pilots did not know how to use a
scale of 'degrees', and had never thought in terms of 'latitude',
quadrants were marked with important place names against particular parts
of the scale, and the pilot could thus recognize his position by the
fall of the plumb line alone." ([TAYLOR 1956], p. 159).[c/]

[a/] The exact location of a ship at sea is uniquely determined by its
latitude and _longitude_. Its _latitude_ is its angular distance north
or south of the _terrestrial equator_; its _longitude_, its angular
distance east or west of a prime meridian (or "zeroth" meridian)
through some particular place. As we shall see, the Portuguese
pioneers of the 15th century solved the "latitude problem"; but a
general solution to the technically much more difficult "longitude
problem" was not found until the latter half of the 18th century --
see, e.g., [BROWN 1956].

[b/] The _latitude_ (angular distance north or south of the equator) of a
place on the earth is equal to the _altitude_ (angular distance above
the horizon) of the northern, or southern, celestial pole as seen
by an observer at the place. Inasmuch as the North Star (_Polaris_,
α of the Lesser Bear) revolved around the northern celestial pole
in a circle of about 3.5 degrees in the late 15th century, the
latitude of a ship north of the equator was indicated to a very good
first approximation by the altitude of the North Star.

[c/] For a sketch of a marine quadrant of 1492, and a vivid description of
the manner and difficulties of its use at sea, see [MORISON 1942],
p. 185.

It was the dropping out of sight of the North Star as the Portuguese seafarers approached and crossed the equator, and continued their explorations southward, that forced the shift from <u>place names</u> on quadrants to <u>latitudes</u> in <u>degrees</u>, and thus began the arithmetization of navigation. The requisite <u>Rules</u>, or <u>Regiment</u>, <u>of the Sun</u> -- for translating observed altitudes of the noonday sun (in degrees) into (degrees of) latitude north or south of the equator -- were prepared about 1485 by a commission of astronomers and mathematicians appointed by Prince Henry's grandnephew, King John II of Portugal. The commission also simplified the astronomer's or astrologer's planispheric astrolabe, "by leaving out the parts not absolutely needed and produced a plain astrolabe of wood and iron" ([PRESTAGE], p. 319) for measuring the altitude of the Sun or North Star, and provided a set of corrections between +3.5° and -3.5° to permit more accurate translation of observed altitudes of the North Star in degrees of latitude north of the equator. A copy of these <u>Rules</u> for converting altitudes of the Sun or North Star into degrees of latitude was apparently used by Christopher Columbus on his voyages to America (1492-1504) and by Vasco da Gama on his voyage to India around South Africa's Cape of Good Hope in 1497-1498.

The rolling and pitching motions of a ship made accurate measurements of the altitude of the sun or a star with a quadrant or astrolabe

difficult, and often impossible, on shipboard.^{a/} Consequently, seamen

went ashore to take altitude observations, if possible. But when a

need for knowledge of latitude arose at sea, especially when the safety

of the ship and cargo and every man aboard was in jeopardy, they

simply had to do as best they could. Therefore I had hoped to come

upon some 16th century instances of "mean taking" at sea in order to

make the best of a set of discordant observations. But, to my great

disappointment, I have not to date found any clear-cut examples of

"mean taking" of any kind in writings on navigation until the last

decade of the 16th century, where I have found two. The first consists

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^{a/} Thus Columbus, homeward bound on his first voyage to America, noted
in his Journal entry for 3 February 1493:

> The North Star appeared very high, as on Cape St. Vincent; but
> couldn't take the altitude with the astrolabe or quadrant,
> because the rolling wouldn't permit it. ([MORISON 1963], p. 160)

And, "Master John of Galicia, a physician" ([TAYLOR 1956], p. 166),
who served as astronomer on the voyage of Pedro Alvarez Cabral
(c. 1460 - c. 1526) to Brazil in 1500, wrote in a letter from Brazil
on 1 May 1500 to King Manuel of Portugal:

> It seems to me almost impossible to take the height of any
> star at sea, for I labour much at it and however little the ship
> rolls, there are mistakes of 4 or 5 degrees, so that it can only
> be done ashore. ... at sea it is better to be guided by the
> height of the sun than by the stars, and it is better to use the
> astrolabe than the quadrant or any other instruments. ([PRESTAGE],
> p. 321)

The difficulties and errors of altitude measurement on shipboard were
not entirely overcome until the advent of the reflecting quadrant
(later, octant, and sextant), invented independently in 1730 by John
Hadley (1682-1744) in England and Thomas Godfrey (1704-1749) in
Philadelphia. (See, e.g., [COTTER], pp. 77-81, or [TAYLOR 1956],
pp. 256-258; and [TAYLOR 1966], Biographies 56 and 262.)

of a set of recommendations written by Thomas Harriot a/ about 1595,

which Professor Taylor mentions but does not spell out in detail in

her book ([TAYLOR 1956], p. 220); and in a letter to me dated February 21,

1964, she remarked: "Hariot is the first man I know to suggest taking

the mean of extensive swings of an instrument aboard ship to get a

reading." The second, by Edward Wright b/, published in 1599, I discovered

during my visit to the National Maritime Museum, Greenwich, during my

visit in September 1969.

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a/ Thomas Har(r)iot (1560-1621), "one of the founders of algebra as we
know the science today" ([SMITH 1928], p. 388), entered the service
of Sir Walter Raleigh in 1579 as mathematical and scientific advisor,
especially to solve mathematical problems arising in navigation and
to correct errors in current navigational practice. By 1584 he had
written a new navigation manual, entitled Arcticon, unfortunately no
longer extant, which he seems to have used "for instructing Raleigh,
and the sea-captains and masters in Raleigh's service in the refine-
ments in the art of navigation resulting from his researches" ([WATERS],
p. 584). In 1585, he was sent by Raleigh to accompany Raleigh's
colonists to Virginia under the command of Sir Richard Grenville,
"obtaining practical experience on the voyage ..., when he was able
to observe the seaman's difficulties and the errors to which he was
prone" ([TAYLOR 1952], p. 345); and "must have learned how wide a
gap there lay between [himself] and the practising sea-masters, a
difference not only in mathematical knowledge and capacity, but in
habit of mind" ([TAYLOR 1956], pp. 215-216). It seems that Raleigh
asked for a "refresher course" before his departure for Guiana in
February 1595, and these "Instructions ...", teaching notes in
Harriot's own hand, lay neglected in the British Museum until
Professor E. G. R. Taylor examined them shortly after World War II,
leading to her account [TAYLOR 1952] of their content. A fuller account
[PEPPER 1967] by Jon V. Pepper of the Royal Naval College, Greenwich,
appeared in 1967, where references to other studies of Harriot's life
and unpublished papers are to be found.

b/ Edward Wright (1558-1615) "was a Fellow (1587-96) of Caius College,
Cambridge, who was induced the the Earl of Cumberland about 1589 to apply
his mathematical studies particularly to navigation ... He designed,
described and made mathematical instruments of many types [and he] gave
William Gilbert (1540-1603] considerable assistance in his great work
De Magnete [1600]. During the last few years of his life, he delivered
lectures on navigation on behalf of East India Company, and dedicated to
them his last book", an English translation, approved by John Napier
1550-1617), of Napier's original work on logarithms, together with a
graphical method of interpolation of his own design (also approved by
Napier). ([TAYLOR 1954], Biography 53; see also, pp. 45-48 of text.)

a. <u>1595 (Harriot)</u>. Thomas Harriot suggested in his "Instructions for Raleigh's Voyage to Guiana, 1595", which date from late 1594 or early 1595, that

when taking the altitude of the sun on shipboard with an astrolabe

a/

or sea ring, the midpoint between the extreme readings, i.e., the

<u>midrange</u>, be taken as the true reading. These recommendations are to

be found in a section captioned "Some remembrances of taking the

altitude of the sonne by the Astrolabe and Sea Ring" (British Museum,

b/

Additional Manuscript 6788, folio 485) . This is what he said with

respect to the astrolabe:

> The Astrolabe hath ben most ancient & it is used comonly &
> only for the sonne; and serveth the seamans turne most specially
> when the sonne is hy ... And when the sea is rough it is very
> hard to make any observation by it ...; because of his agitation
> & unquiet hanging. But however when there is need you must do as
> well as you may. And therefore when you have your Astrolabe
> hanging as quietly as the time wil permit with his side toward
> the sonne according to the usuall order, you are to fit the Index
> by mouing it so long up & down till the sonne shine thourough the
> holes of both the sightes of the same. Or you finding by reason
> of his agitation that the sonne will not passe iustly thourough
> the lower sights but be sometime higher and sometime lower. When
> you finde the light of the sighte to move as much over as under:
> then your Index standeth as precisely as if the Astrolabe had hong
> quietly; & sheweth the true altitude of the center of the sonne.

> But if you doubt of the true hanging of the Astrolabe you may move
> your Index quickly to the same degree on the other side & hold it
> towarde the sonne. If you find the sonne shine thourough as
> before your Astrolabe hangeth well. Otherwise you are also to
> move the Index, till you have also the altitude on that side.
> Which had, compare with the other, & note the difference. The
> half of that difference adde to the lesse altitude or subtracte
> from the greater; And you have the altitude of the sonne as
> exacte as if your Astrolabe had hong truly upright.

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a/ See, e.g., [COTTER], Fig. 2, p. 63.

b/ I am indebted to Jon V. Pepper (now Head of the Mathematics Department, North East London Polytechnic) for providing me with a copy of a "draft typed version". Otherwise Harriot's actual wording of these recommendations might never have surfaced, inasmuch as their mathematical content is negligible.

And with respect to the <u>sea ring</u>:

> The other instrument called the sea Ring is of late yeares
> in great use with the Portingalles & Spaniardes, the making
> whereof & use they had about 40 yeares past of a contry man
> of there owne a learned Mathematician called Petrus Nonius
> [Pedro Nunes, 15C2-1578] ... And is only for taking the
> altitude of the sonne as the Astrolabe. But for ease and
> speed it much excelleth it as also for exactnes. For the
> degrees are as large agayne as in an Astrolabe of the same
> bignes. And in the use, there is no troublesome moving up &
> downe any Index as in an Astrolabe; but it hanging as the
> Astrolabe, is that side which hath a small hole being holden
> towardes the sonne; the light passing thourough presently
> sheweth upon the degres noted within the ring the altitude
> of the sonne you desire. You are to note that the middle of
> the light be it round or long is your true marke; And if the
> light play by reason of his unquiet hanging; then the middle
> of the play is the sight.

A few special features of the <u>midpoints</u> or <u>midranges</u> involved in
these recommendations should be noticed. The sea-ring procedure calls
for reading "the middle of the play" directly; and the first of the
two astrolabe procedures calls for adjusting the Index (i.e., the
alidade) until the oscillation of the light spot (from the light through
the upper sight) is centered on the hole of the lower sight, and then
reading the location of the Index points on the scale. In both of
these cases the <u>midpoint</u> or <u>midrange</u> is "observed" directly, without
computation from the values of the "extremes". In contrast, the second
astrolabe procedure involves finding and reading the "extremes" and then
computing the <u>midpoint</u> or <u>midrange</u>. Finally, in all three cases, the
"sample size", i.e., the total number of "individual" momentary positions
of the light spot or Index at least implicitly involved, is indefinite.

b. <u>1599 (Wright)</u>. Inasmuch as Harriot's contributions to navigation, developed "for the exclusive use of Raleigh's navigators" ([WATERS], p. 590), were never published, I was pleased to find a somewhat similar recommendation in <u>Certaine Errors in Navigation ...</u> <u>detected and corrected</u> (1599) by Edward Wright, "the most influential and oft-quoted treatise on nautical practice of the era" ([TAYLOR 1954], p. 45). The pronouncement of interest to us occurs in connection with Wright's discussion of measurement of the <u>variation of the compass</u> as a navigational tool.—/ If, as he believed, compass <u>variation</u> could be used in combination with <u>latitude</u> determinations to indicate a ship's postion at sea, by comparing observed with tabulated values, it was clearly important that determinations of compass variation on shipboard be carried out as accurately as possible. Therefore, in order "that others that shall go about hereafter to observe the variation (at sea especially) may bee the more circumspect to foresee and preuent all causes of error herein", he advised:

> Exact trueth amongst the vnconstant waues of the sea is not
> to bee looked for, though good instruments bee neuer so well
> applyed. Yet with heedfull diligence we may come so neare
> the trueth as the nature of the sea, our sight and instruments

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—/Footnote appears on p. 3.3-21*.

During the 15th century it became recognized that a magnetic needle
does not, in general, point directly to the north, but deviates from
the true meridian by a small angle that varies from place to place.
This variation of the compass (as it was then called; or magnetic
declination, as it is now termed) was easterly over Western Europe.
Then "the voyages to the Americas and to India between 1492 and 1500
brought ... [the news] that after a ship had passed [west of] the
Azores, or alternatively had rounded the south of Africa [to the east],
there was a change from 'north-easting' to an increasing 'north-
westing'" ([TAYLOR 1956], p. 173). This marked variation of the
variation led to the belief, found, e.g., in a manuscript by João de
Lisboa dating from 1514 ([CRONE], p. 393), that measurement of compass
variation provided a means of solving or by-passing the longitude
problem; and in conjunction with determination of latitude would
enable a ship to determine its position at sea. Thus, the measurement
of compass variation and its use in combination with latitude deter-
minations to find any port or place at sea was the theme of
De Havenvinding (1599) by the distinguished Dutch mathematician, Simon
Stevin (1548-1620), which was prescribed by Prince Maurice of Nassau
for use by all ships under his jurisdiction and published simultaneously
in Dutch, French, Latin and English. The English translation [STEVIN
1599b] was prepared by Wright, and its full title should be noted.
A fresh English translation [STEVIN 1961] has recently become available.

> will suffer vs. Neither if there be disagreement betwixt
> obseruations, are they all by & by to be reiected; but as
> when many arrows are shot at a marke, and the marke after-
> wards taken away, hee may bee thought to worke according
> to reason, who to find out the place where the marke stood,
> shall seeke out the middle place amongst all the arrowes:
> so amongst many different obser ations, the middlemost is
> likest to come nearest the truth.

-- Edward Wright [1599], verso of ℓN1.

Unfortunately, Wright does not give any numerical examples of
the application of this dictum in either his original (1599) or expanded
(1610) edition [WRIGHT 1657]. Consequently, it is not possible to be
absolutely certain that he is recommending use of the median, as he
appears to be. However, his admonition that "neither are [the observa-
tions] all by & by to be reiected" seems to me to support the inference
that he is recommending the "middlemost" observation, i.e., the median,
and not the "middle place", i.e., the midrange: correct identification
of the median requires retention either of all of the observations, or
of sufficient central observations to be on the safe side as more
observations are taken and the number of outliers discarded in each
direction; whereas computation of the midrange requires knowledge of
only the largest and smallest observations throughout the period of
observation.

I did come upon a number of earlier possible instances of "mean
taking" on 16th century voyages of discovery, but they were of a purely
conjectural nature. Thus, Admiral Samuel Eliot Morison, in his latest
best seller [MORISON 1971], quotes, interprets, and comments on the
amazingly accurate determination of the latitude of Newport, Rhode

Island, by Giovanni da Verrazzano in 1524, as follows:

> "This land is situated in the parallel of Rome, in 41 degrees
> and 2 terces", i.e. 41°40'N. The center of Newport is on
> 41°30'N, and the Vatican is on 41°54'N. ... He must have
> "taken" the sun and Polaris frequently and <u>averaged</u> them;
> Rome's latitude he could have obtained from any printed
> Ptolemy or rutter.
>
> <div align="right">-- [MORISON 1971], p. 307;
emphasis added.</div>

And Commander Waters suggests ([WATERS], p. 310) that the large number
of sand-glasses -- "18 hower glasses" ([WATERS], Appendix 10) --
carried by Martin Frobisher on his first voyage to North America in
1576 (see, e.g., [MORISON 1971], Chapter XV) may probably be accounted
for as follows: "in an effort to eliminate the various errors to which
the glasses were subject the prudent navigator turned two or three
simultaneously" and based the start of each new set on "a mean reading
of several glasses".

These examples are, of course, conjectural, but I have a feeling
that some clear-cut examples of "mean taking" in navigational settings
may well be lying "buried" in the logs and chronicles of one or another
of the 16th century voyages of exploration, awaiting discovery by alert
attuned eyes.

4. THE RISE AND FALL OF THE PRINCIPLE OF THE ARITHMETIC MEAN

By far the best known statement of this Principle is that which

Gauss gave as the starting point of his derivation of the (Normal) Law

of Error in art. 177 of his great astronomical treatise, Theoria Motus

Corporum Coelestium ... (1809). What he said, in English translation,

was this:

> It has been customary certainly to regard as an axiom the
> hypothesis that if any quantity has been determined by several
> direct observations, made under the same circumstances and with
> equal care, the arithmetical mean of the observed values affords
> the most probable value, if not rigorously, yet very nearly at
> least, so that it is always most safe to adhere to it.[/]

Gauss clearly makes no claim to having originated this "hypothesis"

that he adopts as an "axiom". Quite the contrary; he regards it as

traditional. And rightly so, because, as we shall see, the practice

of adopting the arithmetic mean of a number of "equally good" measure-

ments of some quantity as the "best" value of this quantity afforded by

these particular measurements certainly predates Gauss's birth (April 30,

1777) certainly by more than one, and, perhaps, by as much as two centuries.

[/] His exact words were:

> Axiomatis scilicet loco haberi solet hypothesis, si quae
> quantitas per plures observationes immediatas, sub aequalibus
> circumstantiis aequalique cura institutas, determinata fuerit,
> medium arithmeticum inter omnes valores observatos exhibere
> valorem maxime probabilem, si non absoluto rigore, tamen
> proxime saltem, ita ut semper tutissimum sit illi inhaerere.
> [GAUSS 1809], art. 177.

The English translation ([GAUSS (1809) 1857], p. 258) is by Charles
Henry Davis (1807-1877), first Superintendent of the American
Ephemeris and Nautical Almanac Office (1849-1856), and twice
Superintendent of the U.S. Naval Observatory (1865-1867, 1874-1877).

4.1 From no mention of "mean taking" of any kind to explicit

"taking of the Arithmeticall meane" "for the true Variation"

in the 16th-17th-century writings on magnetic declination.
_/
The detailed discussion by Gustav Hellmann _/ of early geomagnetic

observations ([HELLMANN 1899]), together with his facsimile reproductions

of practically all of the important writings on geomagnetic phenomena

up to 1635 ([HELLMANN 1898], [GELLIBRAND 1635]), have enabled me to

trace the evolution of "mean taking" in writings on magnetic declination

(or variation of the compass), from no mention of "mean taking" of any

kind early in the 16th century, through seeming adoption (without

explicit mention) of the arithmetic mean toward the close of the 16th

century, to explicit mention of "taking the Arithmeticall meane" in the

mid- and late 17th century. To date I have not succeeded in unfolding

the evolution of "mean taking" so neatly in any other area of science.

As I indicated earlier   (in footnote ___, p. 51), the discovery

of marked differences in the observed direction and magnitude of the

variation of the compass (or magnetic declination) in different parts

of the world toward the close of the 15th century led to the hope early

in the 16th century that the isogonic (Greek isogonios, "having or

pertaining to equal angles") lines of equal magnetic declination over

the surface of the earth would exhibit a stable and orderly pattern

which, in conjunction with the parallels of latitude, would enable a

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_/ (Johann Georg) Gustav Hellmann (1854-1939), Professor of Meteorology
   (from 1886) and Director (1907-1922) of the Meteorological Institute,
   University of Berlin.

ship to determine its precise position at sea from observation of declination and latitude alone, by-passing the need for direct determination of longitude. Therefore, measurement of magnetic declination was pursued with diligence throughout the 16th and the early part of the 17th centuries.

Many of the early determinations of magnetic declination, obtained merely by noting the deviation of the magnetic needle from "north" as determined simply by sighting the North Star (Polaris) over the compass dial, were subject to great uncertainty, especially in the case of observations made on shipboard. "That in this way no great accuracy could be attained is self-evident. It is also to be questioned whether the movement of Polaris ... about the North Pole [in] a circle of about 5 degrees in diameter ... was always taken into account". ([HELLMANN 1899], p. 81) Indeed, declination values obtained by different pilots at the same place not only agreed poorly with each other in magnitude, but often contradicted each other with respect to direction, so "that doubts of the correctness of the magnetic declination arose everywhere ..." (loc. cit.). Clearly a prerequisite to accurate measurement of the deviation of a compass needle from the north at any particular place is accurate knowledge of the direction of "north" at that place; and this is what was lacking, especially on shipboard.

The customary procedure for finding the north-south direction or meridian line at a particular place before the advent of the "north-pointing" magnetized needle was to bisect the angle between morning and afternoon shadows of equal length cast by a gnomon.$\underline{}\!\!/$ In 1525, Felipe Guillen, an apothecary of Seville, presented to King John III of Portugal a portable instrument ("brújula de variacón", i.e., "sea-compass for variation") that he had devised for measuring the magnetic declination at a particular place on land or at sea.   It consisted of a small sun-dial setup with a magnetic needle and an azimuth scale graduated from 0° to 180° clockwise (from N. through E. to S.) and counterclockwise (from N. through W. to S.).   The first to provide a

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$\underline{}\!\!/$ It is described clearly, for example, by the Roman architect Vitruvius (c. 88 - c. 26 B.C.) in his De Architectura Libri Decem (first printed at Rome c. 1486), in two places (Bk. I, Ch. 6, par. 6-7 and 12).   Thus

> Let A [be the locus of a gnomon at] the centre of a [level] plane surface, and B the point to which the shadow of the gnomon reaches in the morning. Taking A as the centre, open the compasses to the point B, which marks the shadow, and describe a circle. Put the gnomon back where it was before and wait for the shadow to lessen and grow again until in the afternoon it is equal to its length in the morning, touching the circumference at the point C. Then from the points B and C describe with the compasses two arcs intersecting at D. Next draw a line from the point of intersection D through the centre of the circle to the circumference and call it EF. This line will show where the south and north lie.
>
> -- Vitruvius, De Architectura ...
> Bk. I, Ch. 6, par. 12; English
> translation by M. H. Morgan from
> [V 1914], pp. 29-30.

Commonly known as the Indian circles method, this procedure has been traced back to an Indian astronomical work dating from about 400 B.C. ([KIELY], pp. 37, 61-62, 281-282). An older and more accurate method, but requiring a wide and absolutely level horizon and accurate sighting, is based on analogous bisection of the angle between the points of rising and setting of some particular star; and was used by the ancient Egyptians to fix the orientation of the temples and pyramids ([EDWARDS], pp. 258-260).

printed description of the use of such an instrument to measure magnetic declination was Francisco Falero, a Portuguese in the service of the Spanish navy, in the chapter on "The northeasting of the needles" (reproduced in [HELLMANN 1898]; English translation [FALERO]) in his treatise on navigation published in 1535. Falero describes three procedures for determining the magnetic declination at a given place with such an instrument: (1) by observation of the azimuth of the magnetic needle at noon, when the shadow coincides with the meridian, (2) by observation of the shadow azimuths corresponding to equal altitudes of the Sun at "one hour, or two, or three, etc., before noon ... and at similar times after noon ..."and (3) by observation of the azimuths of the Sun at sunrise and sunset. Falero favored procedure (2) because "it is a very good principle ... being true ... also it may serve more times per day than the others and there may be no error in it, if it is well observed" ([FALERO 1943], p. 83) -- but he says nothing on how to choose a "best value" when the values found are not all identical.

/ In procedure (1) the instrument is held level with the 0°-180° axis lying in the meridian indicated by the shadow, and the needle indicates the declination directly on the graduated scale. In procedure (2) and (3), the instrument is held level with the needle pointing to 0°; following the reading of the second (shadow or Sun) azimuth, the mean ("el medio") of the two azimuths will be the exact meridian ("sera el meridiano pciso"); and the angle that the needle then makes with this direction will be the declination sought.

In "The shadow instrument" (1537; facsimile reproduction [HELLMANN 1989(c)]; English translation [NUNES 1943]), Pedro Nunes (1502-1578), chief cosmographer to King John III of Portugal and tutor to the royal princes ([SMITH 1928], p. 349), describes (i) his improvement of the Guillen instrument by addition of a device for measurement of the altitude of the Sun, and (ii) a new method of determining latitude at any hour of the day. The importance of Nunes' instrument stems from the fact that King John's son, Prince Luiz, presented a copy of this instrument to his friend and fellow pupil of Nunes, John de Castro (1500-1548), chief pilot of the Portuguese India Fleet, and charged him to thoroughly test the instrument as well as Nunes' new method of latitude determination on his voyage from Lisbon to Goa on the west coast of India in 1538 ([HELLMANN 1898], p. 83; [TAYLOR 1956], p. 183).

De Castro executed this assignment very faithfully, making numerous determinations of magnetic declination -- often two, three or four in a single day by the morning-afternoon shadows method, and occasionally also one by the sunrise-sunset procedure -- on his voyage to India around the Cape of Good Hope in 1538; on his subsequent voyage along the west coast of India, 1538-1539; and again on his voyage through the Red Sea to Suez in 1541. / The 43 values of magnetic declination recorded ranged

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/ De Castro's logbooks, or "Roteiros", for these voyages lay for three centuries in the archives of Portugal practically unused until they were brought to light and published in 1882, 1843, and 1833, respectively -- for particulars, see [HELLMANN 1899], pp. 84-85, or p. 186 of the introduction to [CASTRO 1943]. Passages relating to geomagnetic determinations were reproduced in facsimile by Hellmann [1898(d)]. A new edition of the complete texts of all three of these logbooks (Roteiros de João de Castro, 2nd edition, 3 vols., with preface and annotations by Commander A. Fontoura da Costa. Lisbon: Divisão de Publicações e Biblioteca, Agencia Geral das Colónias, 1940) was utilized in preparing the English translation of selected extracts [CASTRO 1943].

from the extreme of 19-1/2° or 20° E in the South Atlantic at Lat.

31-1/2° S to the north-north-west of the Tristan da Cunha Islands;

through 0° at the first promontory of Natal northeast of the southern-

most tip of Africa; to 11° W on the coast of India ([CASTRO 1943],

p. 189; and [TAYLOR 1956], p. 183). The precision of recorded replicate

determinations made on the same day by the morning-afternoon shadows

procedure was remarkable: "the differences fluctuate only between 0

and 3/4°" and reflect not only the errors of observation but also "those

real differences ... caused by the progress of the ship [which] could

not be taken into account" ([HELLMANN 1899], p. 85)$^{/}$ However, in no

case (at least in the translated extracts [CASTRO 1943]) did de Castro

take any mean of, or otherwise select a "best" value from among,

replicate determinations made on the same day -- perhaps he considered

any such action unnecessary in view of the very close agreement of

determinations made on the same day in comparison with the great range

of values of magnetic declination encountered in the course of his

travels. His 43 values were nonetheless quite sufficient for him to

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$^{/}$De Castro says that "if the ship sails firmly and smoothly, whoever
possesses good estimating power cannot err above 1/2°" in reading a
shadow's azimuth, but "if the ship rolls strongly, it will lead us
easily to an error as large as 2°" ([CASTRO 1943], p. 188). Conse-
quently, azimuths of the style's morning and afternoon shadows at
equal altitudes of the Sun seem to have been recorded in the perma-
nent record only when they could be "estimated" to the nearest 1/2°,
and the derived value of the declination recorded to the nearest
1/4°. The largest "difference" between replicate determinations made
on the same day by the shadows method that I have found in the transla-
ted extracts [CASTRO 1943] is 1/2°, the range of the four determina-
tions given on p. 191.

conclude that "it is certain ... that there is no relation between the variation of the compass and the longitude of a place", a finding that seems to have discouraged further studies of magnetic declination by the Portuguese ([CRONE 1961], pp. 395-396).

Skipping over the next four items (which I have designated "(e)" through "(h)" in my reference entry for Hellmann's "Rara magnetica" [HELLMANN 1898]),/ we come to the facsimile reproduction of the 1596 reprint of a 1581 work [BOROUGH 1581] by William Borough which contains a set of replicate determinations and a pair of summary figures which I feel probably are <u>coarsely rounded</u> values of the <u>arithmetic mean.</u>

—/None of the four items skipped -- the Hartmann letter of 1544 announcing the discovery of <u>magnetic dip</u>, the Mercator letter of 1546 expressing and substantiating the view that the Earth has a <u>magnetic pole</u>, the excerpt from Cortez's book of 1551 containing the earliest exact description of a <u>marine compass</u> and its construction, or Norman's book of 1581, the first printed work purely on geomagnetism -- contains any multiple measurements of a single magnitude, and hence no opportunity for "mean taking" of any kind. English translations of the first three may be found in <u>Terrestrial Magnetism and Atmospheric Electricity</u>, Vol. 48 (1943), pp. 128-130, 201-202, and 84-91, respectively. Norman's book is in English.

<u>1581 (Borough)</u>. On October 16, 1580, William Borough

(1536-1599)^{a/} made 9 determinations of the magnetic declination at Lime-

house in London's East End, with a Guillen-Falero type instrument of

his own design,^{b/} using morning and afternoon shadows corresponding to

Sun altitudes 17, 18, ..., 24, and 25°. His shadow azimuth readings

(to the nearest minute) and the corresponding declination values (to

the nearest <u>half-minute</u>) are given in tabular form on the second page

of Chapter 3 of [BOROUGH 1581] and [HELLMANN 1898(i)], and reproduced

on page 8 of [GELLIBRAND 1635]. The 9 declination values are 11 <u>degrees</u>

and 17-1/2, 11-1/2, 30, 22-1/2, 22-1/2, 15, 20, 17, 14 <u>minutes</u> E,

respectively.

> ... and conferring them altogether, I doe finde the true
> variation of the Needle of Compas at Limehouse to be about
> 11 d. 1/4 or 11 d. 1/3, which is a point of the Compasse
> just [360°/32 = 11 1/4°] or little more.
>
> > -- BOROUGH [1581], Chapter 3, last
> > paragraph

For these 9 values I find:

mean = 11° 18 8/9'	median = 11° 17 1/2'
midrange = 11° 20 3/4'	mode = 11° 22 1/2'

Clearly the operation of "conferring them altogether" is not unequi-

vocally identified by "about 11 d. 1/4 [= 11° 15'] or 11 d. 1/3

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^{a/}Treasurer of the Queen's (i.e., Elizabeth's) Ships in 1582, and later
Comptroller of the Navy. Brief biographies of William Borough are to
be found toward the end of biographies of his older brother, Steven,
(or Stephen), in various editions of the <u>Encyclopaedia Britannica</u>.
For additional information, see [TAYLOR 1954], Biog. 26, Work 58, and
text pages 37-38.

^{b/}Portrayed in a figure on the second page of Chapter 1 (of [BOROUGH 1581]
or [HELLMANN 1898(i)]), which is clearly reproduced in [CRONE 1961],
p. 383.

[= 11° 20']". The median is exactly midway between these two summary values; the other three are all closer to the "11 d. 1/3". These facts certainly seem to favor the inference that the two summary figures are rounded values of the median. Nonetheless, the unusual expression "conferring them altogether", together with the explicit mention of the "Arithmeticall meane" in the later related instance that I shall spell out shortly, lead me to believe, albeit uneasily, that the two summary figures are rounded values of the arithmetic mean.

Forty-two years later, Edmund Gunter (1581-1626), Professor of Astronomy at Gresham College, London, finding the magnetic declination there by a new method / to "be only 6 gr. 15 m", i.e., nearly 5° less than Borough's smallest value, he "enquired after the place where Mr. Borough observed, and went to Limehouse with some friends ... and towards night the 13 of June 1622" made 8 determinations of the "variation" of the Needle (by the new method), which ranged from 5°40' to 6°13' ([GUNTER 1673), p. 279; his data table is reproduced in [GELLIBRAND], p. 15, and in [WATERS], p. 422). Gunter did not choose or deduce a "best" or summary value from his own data. And he refrained

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/ Some years earlier, Gunter had developed computational procedures for solving such problems as: Given the hour of the day in local time, and the Sun's altitude and declination (angular distance north or south of the celestial equator, as given in astronomical tables for the day of the year concerned), to find the Sun's azimuth (angular distance from the meridian). "Having these means to find the Sun's Azimuth, we may compare it with the [Sun's] Magnetical Azimuth [angular distance from the magnetic "north-south line" indicated by the compass needle], to find the variation of Needle" ([GUNTER 1673], The Cross-Staff, p. 278, where a worked example is given; see also [WATERS], pp. 358, 421-422).

from commenting on the large difference between his and Borough's values; perhaps, in deference to the authoritative pronouncement of William Gilbert (1540-1603) "variatio uniscujusque loci constans est" ("variation at any one place is constant"/); or he may have regarded the difference as casting doubt on the accuracy of Borough's values.

Whatever may have been Gunter's view, his successor as Professor of Astronomy at Gresham College, Henry Gellibrand (1597-1636), pursued the matter further, discovering the non-constancy of magnetic declination with respect to time at a given place, and incidentally providing us with the earliest <u>explicit</u> examples of "taking" the <u>arithmetic mean</u> that I have come upon to date.

<u>1635 (Gellibrand)</u>. As a check on the accuracy of the values that Borough obtained in 1580, Gellibrand selects the particular set of observations corresponding to 20°0' morning and afternoon <u>apparent</u> altitudes of the Sun. By an astronomical computation analogous to that mentioned in the fottnote to p. ___, he calculates the Sun's azimuth at each of these two instants; and from Borough's values of 45°0' and 20°15' for the angular deviations of the two shadows at these instants "from the North of the Needle" "to the westwards" and "to the East-ward", respectively ([GELLIBRAND], p. 8), he derives a declination of 11°0'0" from the morning data (p. 13), and 11°32'28" from the afternoon data (p. 14), for comparison with Borough's single value, 11°22'30". He quite clearly viewed his two values as substantiating the approximate

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/The title of Chapter III of Book IV of Gilbert's great book,
<u>De Magnete ...</u> (1600) -- see, for example, [GILBERT 1958], p. 240.

correctness of Borough's value; but considered the mean of his two

values as closer to the truth:

> So that if we take the Arithmeticall meane, we may probably
> conclude the variation [i.e., declination] answerable to his
> time to be about 11 gr. 16 min.
>
> -- GELLIBRAND [   ], p. 15;
> emphasis added.

Note that the value stated is a rounded down value of the exact

arithmetic mean, 11°16'14".

Accepting Gunter's 8 determinations in 1622 at face value,

Gellibrand concluded: "Thus have we prooved that for the Interstice

of 42 yeares, there hath beene an evident diminution of five degrees

variation" (pp. 15-16). He went on to confess: "... this great

discrepance moved some of us to be overhasty in casting an aspersion

of error on Mr. Burrows observations ... till an acquaintance of ours,

lately applying Mr. Gunters owne Needle ... could not finde the

variation so great as 6 gr. 15 min. [sic] formerly found ..." (p. 16).

Confirming by a number of preliminary observations of his own that the

"variation" had apparently decreased further, he "went to Diepford ...

to the very same place where Mr. Gunter heretofore had made observation

..." (p. 16) and made 11 definitive declination determinations on

12 June 1634 by the aforementioned procedure based on computation of

the Sun's azimuth from its observed altitude. The values obtained were

(pp. 17-18):

before noon: 4°6', 4°10', 4°1', 4°3' 3°55' ;

after noon: 4°7', 4°10', 4°12', 4°4', 4°0', 4°5' . _/

_/ arithmetic mean = 4°4 9/11'; median = 4°5';
midrange = 4°3 1/2'; mode = 4°10'.

He summarized these results thus:

> These Concordant Observations can not produce a variation
> greater than 4 gr. 12 min. nor less then [sic] 3 gr. 55 min.
> the <u>Arithmeticall meane</u> limiting it to 4 gr. and about 4
> minutes.
>
> <div align="right">-- Gellibrand [   ], p. 18;
emphasis added.</div>

In spite of the fact that the largest and smallest values are
stated explicitly and the midrange (4°3 1/2') is closer to 4°4' than
is the arithmetic mean (4° 4 9/11'), I believe that in view of the date
(1635) of this statement we are justified in interpreting the words
"the Arithmeticall meane" here as signifying the <u>arithmetic mean</u> OF
all 11 values, and not, as in the case of the 11th century al-Bīrūnī
(sec. 3.3), as the <u>arithmetic mean</u> BETWEEN the extremes -- see secs.
2.1-2.2. The fact that this interpretation involves a rounding down of
over 1/2 should not, I believe, cause us any concern because a similar
rounding down of over 1/2 occurs in the 1668 "D.B." example discussed
next, where the verdict is clear-cut.‾/

‾/ A month later, on 4 July 1634, Gellibrand made 13 additional
determinations at "Paul's Cray in Kent". The individual values
were (p. 19):

4°0', 3°55', 3°56', 3°55', 3°58', 3°58', 4°0', 3°58', 4°2', 4°0',
3°59', 3°59', 4°2'.

> mean = 3° 58 8/13'.   median = 3°59'.
> midrange = 3°58 1/2'.   Two modes (3 values) at 3°58' and 4°0'.

This time he does not mention "the Arithmeticall meane" at all, but
says simply: "So that its plaine, the observations made in this
place do all make the variation to fall neere upon 4 degrees";
and then sums up with "Hence therefore we may conclude that for the
space of 54 yeares (the difference of time betweene <u>Mr. Burrowes</u> and
these last observations of ours) there hath beene a <u>sensible</u>
diminution of 7 degrees or better" (p. 19).

Thus, Borough's (1581), Gunter's (1622), and Gellibrand's (1634) observations together revealed unequivocally: "that variation, far from being immutable at any place, as hitherto believed, changes with the passage of time. This discovery of 'the secular change of variation' was to give an added urgency to the problem of finding longitude astronomically. Not only did it show all earlier observations of variation in all parts of the world to be thoroughly unreliable for longitude-finding, but it showed how imperative it was to check the steering-compass frequently for variation, either by azimuth or amplitude observations [of the Sun]" ([WATERS], p. 153)

Although Gellibrand's discovery of the non-constancy of magnetic declination over time at a particular place destroyed the hope of using measurement of "variation of the compass" as a substitute for longitude measurement in navigation, Gilbert's De Magnete ... had launched the study of geomagnetism as a scientific discipline in its own right; and measurement of magnetic declination in various parts of the world continued, and continues today. Searching through early issues of the Philosophical Transactions [a/] of the Royal Society, I came upon the following clear-cut example of "taking" the arithmetic mean as the "best" approximation to the truth.

1668 (D.B.) The issue of the Philosophical Transactions, dated "Monday, July 13, 1668", contains "An Extract of a Letter, written by D. B. to the Publisher" [D.B. 1668] presenting a table of 5 magnetic declination measurements made near Bristol on June 13, 1666, by Capt. Samuel Sturmy [b/], "an experienced Seaman, and a Commander of a Merchant Ship for many years," who took them "in the presence of Mr. Staynred [c/], an ancient Mathematician, and others."

> In this Table, he [Capt. Sturmy] notes the greatest ...
> difference to be 14 minutes; and so taking the mean for the
> true Variation, he concludes it then and there to be just
> 1. deg. 27. min.
>
>   -- D. B. [ ], p. 726

[a/] Publication of which began in 1665, at first partly as a private profit-seeking venture of the Society's Secretary, Henry Oldenberg (c. 1615 - 1677).

[b/] 1633-1669. For further details see [TAYLOR 1954], Biog. 265a, Work 329.

[c/] Philip Staynred (Standridge), fl. c. 1621-1669. For further details see [TAYLOR 1954], Biog. 145 and Work 328.

The 5 individual determinations "in this <u>Table</u>" are:

1°22', 1°36', 1°34', 1°24', 1°23'.

The exact <u>arithmetic mean</u> of these 5 values is 1°27 4/5';

the <u>median</u> = 1°24' ; the <u>midrange</u> = 1°29' . The "mean" taken "for

the true Variation" was the <u>arithmetic mean</u> indubitably!