Measurement and Philosophy

Mensuração e Filosofia

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Abstract: Peirce earned his keep making measurements, mainly of gravity but also astronomical, and he made several contributions to the science of measurement. It has been said that his experience measuring had philosophical consequences: his adoption of fallibilism, his argument against necessitarianism, and his conception of inquiry as converging on the truth have all been mentioned. But not much attention has been paid to the curious episode of his making "the study of great men" part of a course in logic: students were asked to rank a long list of men by order of greatness. That was at Johns Hopkins in 1883. I shall argue that that study, together with his reflections on pre-instrumental estimates of stars" brightness, bears directly on the method of phaneroscopy formulated nearly two decades later. In each case, the problem is to show how objectivity is possible under conditions in which it must seem that objectivity is impossible.

Key-words: Measurement. Observation. Objectivity. Phaneroscopy. Peirce.

Resumo: *Peirce ganhou seu sustento fazendo mensurações, principalmente de gravidade, mas também mensurações astronômicas, dando várias contribuições à ciência da mensuração. Já se disse que sua experiência nessa área teve conseqüências filosóficas: sua adoção do falibilismo, seu argumento contra o necessitarismo e sua concepção da inquirição como convergência para a verdade foram todas mencionadas. Mas foi dada tanta atenção ao curioso episódio de ele ter feito de "o estudo dos grandes homens" uma parte de um curso de lógica: era pedido aos estudantes que classificassem uma longa lista de homens por ordem de grandeza. Isso aconteceu na Universidade Johns Hopkins em 1883. Defenderei que esse estudo, junto com suas reflexões sobre estimativas pré-instrumentais do brilho das estrelas, é diretamente relevante para o método da faneroscopia formulado aproximadamente duas décadas mais tarde. Em cada um dos casos, o problema é mostrar como a objetividade é possível sob condições em que é preciso parecer que a objetividade é impossível.*

Palavras-chave: *Mensuração. Observação. Objetividade. Faneroscopia. Peirce.*

I. No part of scientific work would seem further removed from philosophy than the tedious, painstaking labor of measuring. A glance at the long columns of numbers, in fine print, that surprisingly occupy many pages – over a hundred all told – of the chronological edition of Peirce's *Writings*[1] – and which are but a minute sampling of Peirce's empirical work – prove how time-consuming his labors must have been. The measurements made, of the force of gravity at various points in the United States, of the magnitude (relative brightness) of stars, and of phenomena observable only during solar eclipses, were of a precision not before attained and required instruments of corresponding delicacy. Much time was spent merely in traveling to places of observation and setting up the apparatus, most notably to Kentucky in 1869 and to Sicily in 1870 to observe the solar eclipses of those years. And Peirce had not only to make the measurements or, in later years, to direct others who made them, but also to make complex calculations from the raw data in order to produce the scientifically meaningful data that were published. But to say only this much is to scant the amount of original thought that goes into making careful measurements of novel kinds. Peirce adapted instruments to new purposes, devised mathematical techniques by which the resulting data could be applied to the questions asked, and found ingenious ways of checking the instruments themselves for accuracy. Most importantly, he found a systemic error in pendulum measurements of the force of gravity, due to a flexure in the apparatus, reported that finding at a meeting of geodesists in Paris and traveled to Germany to have a better instrument made. For a concise yet impressive summary of Peirce's scientific work, in geodesy as well as in astronomy, see Victor Lenzen's 1964 article, "Charles S. Peirce as Astronomer".

Among other things that may be learned from Lenzen is that Peirce's experience measuring and in making calculations from measurements led to work on the theory of measurement and on standards of measure (he was in charge of the U.S. Office of Weights and Measures for five months in 1884-5). As to the first: when Peirce was appointed to a permanent position in the Coast Survey in 1861, he worked as a "computer" (as in those days persons making computations were called) reducing observations of occultations of the Pleiades by the moon and thereby correcting earlier determination of longitudes (by triangulation from time-differences in such observations taken at different locations). This gave him experience with the method of least squares, based on probability theory, by which a best estimate of a quantity may be derived from a set of measurements of the same quantity, assuming that those measurements will tend to err symmetrically, on one side of the true value or the other[2]. Peirce showed, however, that error can also be introduced by this method – for example, when, as in determining the time of the emersion of a star from occultation, errors will be asymmetric. His paper "On the Theory of Errors of Observation", hereinafter TEO, appeared in 1873

1 E.g., W3: 419-72 and W6: 223-41 – seventy-three pages – contain only columns of numbers. I shall follow the usual conventions in citing Peirce's writings: "Wn: m" denotes p.m of vol. n of the chronological edition of Peirce's Writings, "EPn: m" denotes p.m of vol.n of the Essential Peirce, and "n. m" denotes paragraph n of v. m of the Collected Papers. See bibliography.

2 According to this method, the best estimate of a quantity, relative to a set of measurements actually made of it, is that number which is such that the sum of the squares of its differences from each actual measure is least.

(W3:114-60). As to the second: in 1867, Harvard observatory acquired a spectroscope (invented in 1859), which Peirce used in 1869 to measure the spectrum of an aurora. Later, in relation to his work in measuring gravity by the pendulum method, he used spectroscopy to determine the meter in units of a wave-length of light. A meter stick will vary in length, e.g., with temperature, whereas wave-lengths of light are not variable (a point deepened in 1905, with the publication of Einstein's special theory of relativity). He was the first to have used a wave-length of light as a unit of measure.

That, in brief, was how Peirce made his living during his ripest three decades, from age twenty-one to age fifty-two. It will be noted that, as his tasks were to make the most precise measurements possible of minute differences in quantity, he naturally focused on sources of error and on means of overcoming these. And yet his chief interests during all this time were philosophical, in logic for the most part but extending into metaphysics and cosmology. He was spinning the most abstruse theories with one hand while swinging a pendulum and writing numbers with his other hand – a high level of ambidexterity. What influence might Peirce's deep engagement with measurement have had on his philosophy? I shall argue that a new answer to this question sheds a surprising light on what may be his most important contribution to modern thought. First, however, there are some well-known answers that are also important and these I will discuss briefly, just for the sake of completeness.

Peirce's cosmological doctrine of indeterminism (or "absolute chance" or "tychism") is variously motivated but at the time of its creation depended for its plausibility on refuting the doctrine of determinism (or "necessity") then associated with Newtonian mechanics. That refutation depended on pointing out that our knowledge of physics" laws is empirical and, therefore, that these laws cannot be known to hold any more exactly than can be determined by observation and measurement. As we can make no measurements that are absolutely exact, it follows that, so far as we can know, the laws of physics may be no more than approximate (EP1: ch. 22). But if laws hold only approximately, then they may hold to varying degrees of precision, which supports not only tychism but also Peirce's synechism, the principle that one should look for continuities of gradation in place of philosophy's problematic dualisms, such as that of mind and body. Thus he could entertain the hypothesis that matter and mind differ only by the degree to which behavior is determined by law: behavior noticeably variable we attribute to "mind", that predictable we attribute to "matter" (EP1: ch. 21).

Imprecision is not the same thing as error; one can say without error that it is four miles, roughly, from here to town. However, from the inaccuracy of all measurement, it follows that, the more precisely we attempt to state a measure, the more likely we are to err. And if we try to get around that problem by specifying a measurement's margin of error, that opens up a new avenue for error. So also, attempts to calculate a best estimate from several inaccurate measurements, as by the method of least squares, can introduce error, as Peirce demonstrated in TEO. But that is not the end of the matter, for theoretical conclusions sometimes depend on measurements made of observed quantities. There are many reasons why we should be humble and admit that our most cherished beliefs may turn out some day to be mistaken; these reflections on measurement provide one such reason. In the 1890s, Peirce asserted the doctrine of fallibilism (presumably in reaction to the doctrine of Papal infallibility, declared at the Vatican Council of 1870), according to which we can never know enough to know which of our beliefs are exempt from eventual refutation.

Finally, Peirce identified truth with the opinion on which inquiry converges. That is a doctrine some have thought was modeled on the way additional measures of a quantity lead, by the method of least squares, toward successively better approximations to the exact truth. However, that makes the progress of science smoother and simpler than obviously it is. Measurement of a given quantity is one thing, construction and choice of theory is another. As alternative theories differ from one another in many dimensions, progress in theory is unlikely to be monotonic. Is it plausible that Peirce would have been guilty of so simplistic a view? It is clear from some passages that the convergence he had in mind is not at all that of successively more accurate measures of a single quantity: instead, he was referring to the convergence of independent lines of inquiry on the same conclusion – lines that begin from different bodies of evidence and proceed by different kinds of reasoning. To take a post-Peircean example, think of the surprisingly diverse forms of evidence (geological, biological, magnetic etc.) that led by various pathways to the same conclusion, that had at first seemed preposterous, that the continents have drifted. That sort of convergence is consistent with the divergence of inquiry into ever more numerous specializations, each addressed to different theoretical questions: an endless branching with each branch producing different kinds of fruit. This contradicts another mistaken simplification of Peirce's view, viz., that "the" final opinion is a single answer to all questions, one grand system in which all truth is contained. In this case, Peirce's experience with measurement has had more influence on his interpreters than on he himself, leading them to attribute to him a view obviously false and inconsistent with what he actually wrote. More plausible is Lenzen's suggestion that Peirce's probabilistic defense of induction was modeled on his experience with the method of least squares (for which he cites powerful textual evidence, viz., that one version of that defense is found in TEO). The validity of induction is of course essential to such convergence as scientific inquiry does exhibit, namely on answers to specific questions (including questions about which questions have answers).

II. The most bizarre entry in the new, chronological edition of Peirce's *Writings* is surely the 81 pages in the fifth volume collected under the title "Study of Great Men" (W5: 26-106). Those pages are taken up almost entirely with long lists of names (574 in all) of famous men (and a few women – Cleopatra, Mrs. Siddons, George Eliot) and enumerations of their characteristics (under some constant categories such as "shape of chin" and "sexuality, how shown?" but also including stray facts such as that Rabelais worked in bed and wrote while eating). There are an additional 27 pages of notes (W5: 426-33, 524-42) of the same nature. The data were compiled from questionnaires completed by students, after they had read biographies of the persons in question, in a logic course Peirce taught at Johns Hopkins in 1883-4.[3] Then the students were asked their "impressionistic" responses. The objective of the exercise was to rank a large number of individuals by their relative "greatness", but the study was never completed. What has any of this to do with logic?

[3] Or possibly as a voluntary project outside of a course proper: see FISCH and COPE 1952, p. 290-1.

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The editors of W5 tell us that Peirce called the study "comparative biography" and they mention that it was an application of statistics (W5: 426). Greater light is shed by Peirce himself in a manuscript written some sixteen years later and printed in the *Collected Papers*, 7.256-66. There he explained that the study of great men was indeed chosen as "a subject that might afford valuable training in […] inductive investigation …", but more particularly as one in which "no exact observations could be made". Had Peirce's purpose been to teach statistical analysis or the principles of inductive reasoning, he could not have chosen a worse example. Instead, he was aiming at something else: to "explode the ordinary notions that mathematical treatment is of no advantage when observations are devoid of precision and that no scientific use can be made of very inexact observations" (7.256).

What is this distinction between exact and inexact observation? Apparently using "precise" and "exact" as synonyms, Peirce continued: "… very little training is required in the purely observational part of the business of making observations that can be rendered extremely precise; while great training is requisite for the making of the very observations themselves when the observations are of the kind which can never be made precise." But what is the "observational part" of making observations, and what is the other part? The answer is clear from a pair of examples he used to illustrate the difference. In one example, colors are to be matched. Care must be taken that each sample be illuminated in exactly the same way, but the judgment that can then be made, as to whether they match, is easy. "On the other hand, an example of an observation that can never be rendered precise is that of saying which of two different colors, say a red and a blue, is the more luminous." Here, the difficulty lies within the observer, who must be trained to attend to luminosity while ignoring hue. The "observational part" of observation is therefore the observer's judgment, while the other part consists of the conditions under which that judgment is to be made. And what, in the context of 7.256, Peirce meant by "inexact observation" is an observation the accuracy of which depends on training the observer's judgment rather than on care in setting up the conditions of observation.

In observations of luminosity, Peirce remarked, "A great deal of training is required before a person can do this well enough to give any uniformity to his judgments." (Notice that the test of accuracy of observation is agreement – "uniformity of […] judgments". In this case, the uniformity sought is among judgments made by the same observer. We shall return to the importance of agreement, below.) Later in the same long paragraph, he added: "The exact sciences, physics and chemistry, do not teach observation of any kind, to speak of; but only manipulation and experimentation. The purely observational business is confined to such trifles as bisecting stars, putting crosswires on spectral lines, reading verniers and the like."

Before proceeding, I must object to Peirce's use (in 7.256, not necessarily elsewhere) of the terms "exact" and "precise" and their negatives. At least with respect to measurement, these terms as ordinarily used refer to something that varies by degree and can be represented numerically. If not erroneous, a measurement to three decimal places is more exact or more precise than is a measurement of the same quantity to two decimal places: the difference in exactness of measurements is itself measurable. As a temporary expedient, let us call this the quantitative sense of exactness. Peirce assumes this quantitative idea in his characterization of "inexact observation", namely, as being one of a type that can be made more exact – in the quantitative sense – by training the observer. But then there is nothing to prevent a so-called inexact observation from being quite exact, indeed, more exact than some so-called exact observations. (As we shall see in a moment, Peirce himself provided several examples of this.) To avoid confusion, I shall use “exact” and “precise” only in the quantitative sense when speaking either of measurement or of observation. That is the meaning these terms ordinarily have in such applications, though not in other applications.

Possibly, this sense of exactness does not apply to all observation. However, the sorts of observation Peirce mentions in the passages that concern us are closely connected with measurement, and “exact” and “precise” apply to them in the same quantitative sense. Suppose that we measure the length of a board by a yardstick: we lay the yardstick along the board and match the end of one to a line on the other. Matching is therefore part of measuring. Conversely, the matching can be exact or inexact: perhaps the board does not quite end where the nearest mark on the yardstick is. And that degree of exactness can also be measured, if only by using a finer-grained measure: “Ah, you´re off by two hairs.” Whether two colors match is also not a simple either/or question. Colors that may be said to match for one purpose might be judged not close enough for another purpose; the latter comparison is then said to be more exact. Again, ranking items in order, whether it is the brightness of stars or the greatness of men or the nearness in color of pairs of colors may be more or less exact. A system of three orders of brightness or greatness or nearness is not as precise as is a system of a dozen orders; the latter requires finer discriminations. And such ordering is also part of measuring: our smallest units of difference in rank-ordering become units by which the degree of a thing may be measured.

Repeatedly over many years, Peirce expressed interest in the fact that observation can be made more exact by training the observer. In TEO, an experiment is described, and its results tabulated, showing the distribution of errors made in observations like those of the emersion of a star from occultation. Subjects were asked to time the onset of an event, determined by a mechanism, occurring abruptly; they did so by striking an extremely sensitive telegraph key which then caused their “answers” to be automatically recorded. As the mechanism producing the effect was precisely timed, the true values were known and thus the observers’ errors could be measured and their distribution charted. The purpose was to make an empirical test of the method of least squares when that method is applied to observations of this kind. Incidentally, however, Peirce found that the size of observer error diminished dramatically with practice. “For the twenty-fourth day the probable error does not exceed one-eightieth of a second. I think that this clearly demonstrates the value of such practice in training the nerves for observation …”, and “... it is the general condition of the nerves which it is important to keep in training more than anything peculiar to this or that kind of observation” (W3:137). Some twenty-seven years later, Peirce illustrated the influence of “the general condition of the nerves” by the case of a person whose ability to pick out harmonics of a note struck on a piano was improved by training in color observation. That illustration occurs in the passage, 7.256, from which we have been quoting, as part of his comment on the “Great Men” study, sixteen years after that study was made. Considering the dramatic shifts Peirce’s philosophy underwent, his preoccupation over many decades with certain problems in measuring is remarkably constant.

One-eightieth of a second is in size well below any interval one might consciously notice. The accuracy attained by training therefore exceeds that of which one could be directly aware. This contradicts the assumption of British empiricism that all knowledge derives from direct examination of one's own ideas, i.e., the contents of one's consciousness. For the same reason, it undermines the method of British empiricism – Locke's "historical, plain method" – of establishing the sources of knowledge by reflection on our own mental processes. As it turns out, neurophysiology has more to teach us than introspection does about how we know things. To say so is of course to sap the foundations of foundationalism and to imply a "naturalization" of epistemology: theory of knowledge depends on scientific theory, rather than the reverse. Such conclusions were not drawn in TEO, but Peirce had implied them earlier in his critique of Descartes" assumption of immediate cognition, in his famous three articles of 1868-9, in the *Journal for Speculative Philosophy*.

In the same period that he conducted his students through a study of great men, Peirce also guided a single student, Joseph Jastrow, in setting up and carrying out an experiment that holds a place of importance in the history of psychology.[4] The resulting paper, "On Small Differences of Sensation", published in 1885, presents their experimental findings and argues on that basis against the assumption (by Fechner and others) of a "least perceptible difference" or *Unterschiedsschwelle* of nerve excitations (W5:122-35). A subject was asked to discriminate between minute differences of pressure on a finger-tip, even when he declared that he was quite uncertain and could only guess. As in the TEO experiment, the correct answer is known. It turned out that subjects who thought they were guessing judged correctly more often than would have been the case had they merely guessed. Perceptible differences therefore occur below the level of conscious discrimination. While the proportion of error increased as the size of the difference decreased, there appeared to be no lowest limit beneath which there was not some bias in judgment toward the truth. This prompted a skeptical curiosity about telepathy: Peirce thought it possible that one might respond to cues of which one was unaware and that this possibility is open to empirical investigation (W5: 135, 7.597-614).

Peirce's interest in the power of unaided observation to become more exact, especially through practice, was perhaps inspired in the 1870s when he was measuring the magnitudes of stars using an instrument (a Zöllner's Astrophotometer). For the same had been done without instrumental aid by the ancient astronomers; and Peirce, who examined their results and compared them to his own, was impressed by the surprisingly fine discriminations they were able to make. He reported on this in his 1878 book, *Photometric Researches*, and repeated the tale in his late comment on the "Great Men" study. He remarked (7.258) that the ancient astronomers divided visible stars into six orders of magnitude, that Ptolemy subdivided each of those into thirds, and that earlier in the 19thC they were subdivided into tenths. That makes sixty distinguishable degrees of brightness. Even Ptolemy's eighteen degrees is not something the casual stargazer could distinguish: it requires persistent practice. "Now when photometry came into use, it was found that there was a nearly constant ratio of light between the light of average stars of successive magnitudes [i.e., as previously determined]." "The old astronomers assigned successive numbers to stars which gave equal differences of sensation, and these [...] correspond to a geometrical progression of intensities of light measured physically ..."

4 See the Editor's note 1 to 7.21.

The old astronomers could be quite confident in their judgments because of the consistency with which each of them judged and because of the further consistency of the judgments of each with those of others. No doubt they assumed that there is some physical reality that both explains and is revealed by that consistency. Peirce discovered a further consistency, of their observations with instrumental readings, and that consistency, because of the physical theory of the instruments, tells us more about the cause of their agreement. Only, this further consistency is not a simple one, as it relates an arithmetical progression in sensory effect to a geometrical progression in physical stimulus.

We begin to see what Peirce meant by a mathematical treatment of observations that are, in his confusing use of the term, "inexact", i.e., that depend utterly on the judgment of observers unaided by yardsticks, crosswires, verniers, and the like. In fact, he modeled the study of great men on that of the early astronomer's non-instrumental observations of stars" magnitudes:

> The first thing we did was to make what I called an impressionist list of great men […] the admission to it of any name was to depend upon our pure observation of the impression of greatness which we received from the contemplation of his life and labors, while carefully abstaining from any analysis of greatness or the reason for the impression we felt … (7.257)

> The scale of star-magnitudes, having been found to lend itself perfectly to mathematical treatment, was imitated by us […] we marked the greatest man on our list 1 and the least 6. Some member of the class […] would read an account drawn up by him of one of the men, and each member would then secretly jot down in units and tenths, his evaluation of the impression produced upon him. (7.258)

III. That beauty is in the eye of the beholder is a peculiarly modern idea. One might suppose that impressions of greatness are also subjective in that sense and that the participants in the Study of Great Men were ranking only their own feelings. And yet their judgments cohered to a significant degree. On the 1 to 6 scale, there was normally a difference between extreme judgments of less than one unit. A few cases showed greater variance, but "I do not think that there was any man for whom the extreme estimates exceeded two magnitudes" (7.259). When using the method of least squares, one discards measures far off the median as due, probably, to some fluke: in this study, an out-of-line judgment of Julian was not rejected but noted as "probably influenced by religious bias" (7.258n4). Of course the participants in the study (including Peirce himself) were of roughly the same cultural background. "Since we were all students, no doubt we had some bias in favor men of intellect; but against this we were on our guard" (7.257); again, "… the marking could not escape a large 'subjective' percentage due to our common, but not thoroughly catholic, culture and environment" (7.259). Clearly, the intent was to evade cultural bias as far as possible. Shared bias, however, did not render it unnecessary for the participants to contemplate the biographical data on each person studied. Their agreement must therefore reflect something in the data contemplated, no matter how much it was also conditioned by their sharing a culture.

The concept of personal "greatness" is a cultural product; but so is any concept, such as those of length and weight. The initial division of starlight into six orders of magnitude is arbitrary, as is the division of length into inches or meters. Objectivity in application of concepts is compatible with their social origin and arbitrariness. Some concepts are identified with criteria of application. Philosophers tend to think only of these, as their work often consists in defining concepts, a fortiori by stating criteria of application. However, a criterion must itself employ concepts, and, if an infinite regress is to be avoided, there must ultimately be concepts applicable without criteria (thus Bertrand Russell's famous distinction between knowledge by acquaintance and knowledge by description). Debates about objectivity, e.g., of moral notions, sometimes turn on whether a choice of criterion is objective; I won't go into what that might mean, though in moral questions the contrast usually intended is to self-interest. But in the case of concepts applicable without criteria the same question of objectivity cannot be raised. It would seem that the only question is whether persons can be so trained that their judgments will agree. If there is agreement, then there must be something in the items observed that accounts for that agreement, and that is the feature said to be observed.

Now, greatness was clearly intended in this study to be without criterion: in a passage already quoted, Peirce wrote that "the impression of greatness" was to be formed "while carefully abstaining from any analysis of greatness or the reason for the impression we felt". That does not mean that no verbal instruction was germane: greatness had still to be distinguished from other qualities, such as moral goodness or attractiveness or interest to logicians, and one can imagine the students being admonished, "No, you are confusing approval with your impression of greatness; try again." Those are words I imagine Peirce having spoken. His own words, written later, are: "I think we had a human prejudice against monsters of iniquity" (7.265). Training in observing greatness was in that respect like learning to distinguish luminosity from hue. One has to sort out one's own impressions. But, once the right distinctions are made, then the observers" judgments are the only evidence there is of differences in degrees of greatness. Those judgments are not based on evidence, as by a criterion. They are the evidence. One cannot go behind them, as it were.

In a letter of 1905 to William James, Peirce wrote, "… I have myself preached the doctrine of immediate perception as you know; – and you can't find a place where I distinguish the objective and subjective sides of things" (8.261). As the context makes clear, the subjective and objective sides referred to are those assumed by philosophers who think that there is a problem in proceeding from one to the other. In their view, the subjective is one's own feelings, the objective is the world external to all feelings, and it is the subjective that one observes directly. Knowledge of the external world is thus made to seem problematic. By contrast, Peirce's view was that "It is the external world that we directly observe …" Therefore, when some five years earlier he wrote of the Johns Hopkins study that "… one of my main purposes was to train the men to nice observations of their own sensations, to show them that feelings are capable of direct evaluation with sufficient precision to serve a scientific purpose, and to admit of mathematical treatment …" (7.265), he did not mean to deny that the greatness felt actually qualified the persons studied, at least so far as they are accurately portrayed in the biographical data. He did not separate the content of sensation from the qualities sensed.

Peirce's talk of subjectivity and objectivity in judgment, in the passages of c.1900 from which I have been quoting, is therefore to be distinguished from the distinction between subjective sensation and objective quality that he rejected. Judgments are more objective or less objective depending on the conditions in which and care with which they are formed and their freedom from extraneous influences. Their objectivity is revealed in the uncoerced agreement of different observers: "… if what we mean by a judgment being 'objectively valid' is that all the world will agree in it, and after all Kant's discussion that is about what it comes to, then there was a satisfactory degree of 'objectivity' in the mean magnitudes we assigned, though they referred, not to the man as he really was, but to the man as he was presented in the account read to the class …" (7.259). "This objectivity," he remarked about his own judgments of greatness, "is, like Mercutio's wound, not as wide as a church door, yet twill serve" (7.266).

Peirce described the Study of Great Men as one that pertained to a class of especially difficult observations. But does it not pertain as well to the basis of all observation, even that which is most "exact" in the sense of employing sophisticated instruments that unsophisticated lab assistants can use effortlessly? Take even the simplest case. Before one can measure the length of a board, one must learn to distinguish length from other dimensions such as width and weight and texture, and one must acquire the skill of matching edges and lines, and also the skill of counting, that is, of repeating the numerals in order while attention shifts from one line on the measuring stick to the next. Objectivity is not something achieved merely by avoiding prejudice. It is hard won: something made, not found. Sensitivities have to be formed, arbitrary units chosen, and so on. It begins in infancy with the formation of skills that cannot be taught by a teacher's imparting definitions and rules and criteria. Verbal instruction presupposes skills and a world already well-organized in perception coördinated with action. And objectivity is built up in successive layers: non-instrumental observations are employed in using instruments; instruments are used in calibrating finer instruments and checking their reliability; entities postulated to explain agreement in observation prove to be measurable indirectly by yet further instruments and in terms of newly invented units arbitrarily chosen; and so on.

To uncover the bases of objectivity, one might try to put aside all that one knows and is habitual and recover the mindlessness of infancy and then the way in which movements of head and eye gradually coördinate in response to sensation so as to garner additional sensations organized into images – images keyed to emotional and physical responses, so that one smiles in response to mother's smile and grabs her finger. Our first observations are of persons and things, before we can learn the words for them. But lost innocence is n"er recovered. So, Peirce led his students through a study that was to the same effect, by its being so unusual. Attempting to distinguish degrees of personal greatness puts an adult on a par with an infant learning to read faces. Being unusual, the study of great men seems to us bizarre. But it is really not much different from wine-tasting, which seems less bizarre only because in our culture it is so familiar, while at the same time it has become fashionable to denigrate greatness, especially of men. It has often been noted with amusement that Peirce once hired a sommelier to tutor him in Medoc wines. But the same problems, of distinguishing dimensions of difference and establishing their reality by agreement of judgment among different observers, or tasters, arises in that case as in the others we have been considering. It is therefore permissible to suppose that Peirce's interest in the subject was scientific, indeed, that of a logician. There was probably a reason why he could not have made wine-tasting part of a course in logic, in Baltimore in 1883, and, so, turned to a study of great men, instead.

IV. Many times, Peirce referred to his 1867 paper, "On a New List of Categories" in the highest terms, e.g., as "my one contribution to philosophy" (8.213, c. 1905). To the consternation of some, I have argued that the praise was due entirely to this essay having been the first expression of his list of three categories and not at all due to the way in which he there derived them (SHORT 2007, ch. 2, §2; ch. 3, §1). The derivation in 1867 was a transcendental deduction expressly in the Kantian style. There was in subsequent years no further echo of Kantian "deductions". In fact, there is much evidence that Peirce foreswore a priori philosophizing altogether. The very reason some give for assigning the "New List" pride of place, viz., that it is the only essay in which he attempted to "justify" the categories a priori, is instead the reason why that essay is superseded in his later writings. Such assessments of the "New List" ignore the revolution in philosophy that Peirce's pragmatism is. When he returned to his categories some eighteen years later, he re-founded them on an empirical basis, in a new kind of analysis of experience which he at first named "phenomenology" but after 1904 named "phaneroscopy"; I will use the later term. Phaneroscopy is the most fundamental part of Peirce's attempt to reconstruct philosophy as an empirical science, or set of empirical sciences. As with science in general (as science in the modern era had become), philosophy as scientific is a stream of specialist inquiries (special does not always mean narrow), the conclusions of which are always tentative. The worth, or justification, of any conclusion lies in the future, as it depends on what can be done with it, whether practically or as leading to further discoveries of theoretical interest. That is what pragmatism entails. Its roots are in Kant's writings, but it is profoundly anti-Kantian.

To be sure, Peirce described phaneroscopic analysis as verging on the apodictic: it eschews judgments about whether what is experienced is physically real or what its explanation might be; it limits itself to categorizing the most universal features of experience; and it aims to achieve a formal analysis that will hold for all possible experience. However, the method is nonetheless observational and there is room for error; there can never be a guarantee that the aim of universality and necessity has been attained. "There is nothing quite so directly open to observation as phanerons" (1.286), meaning by "phaneron" "the collective total of all that is in any way or in any sense present to mind, quite regardless of whether it corresponds to any real thing or not" (1.284).[5] The three categories were re-founded (and renamed) on the basis of applying relational concepts to observation of the

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5 Peirce refers here not to reality in general, defined as that which is independent of any person's opinion about it, but to physical reality, defined as that which exists independently of experience (but not necessarily as being other than it is experienced to be, when experienced): see 6.327-8, where the term "external" is used in lieu of "physical". The distinction is crucial in the present context since, as a science, phaneroscopy is indeed the study of something real, albeit not necessarily physical. The test of something"s being real, whether physical or not, is that inquiry converges on an opinion about it. That opinion is not any person"s; it might in fact never be reached by anyone.

phaneron, identifying elements of it that are respectively monadic, dyadic, and triadic. Phaneroscopy is that observation and analysis: "So far as I have developed this science of phaneroscopy, it is occupied with the formal elements of the phaneron" (1.284). The phaneroscopic categories are 1stness, 2ndness, and 3rdness.

The phaneroscopic revision of the categories began in 1885 (W5: 235-47), a year after the Study of Great Men ended without being finished. The thesis of the present essay is that, in method, the two studies are much alike: the Study of Great Men anticipates, explains, and justifies those aspects of phaneroscopic method that are the most difficult to grasp and seem the most open to criticism. I will not attempt a full-scale exposition of phaneroscopic method here (for brief exposition, see SHORT 2007, ch. 3, §2-3), but will conclude by listing the points of comparison that I see between it and Peirce's curious exercise in logic at Johns Hopkins.

In both studies, one is to observe one's own feelings, but without any assumption being made that feelings are private episodes wholly contained within an individual's subjectivity. As indicated above, in phaneroscopy the question of physical reality, i.e., reality independent of experience, is not even raised. In the same passage, Peirce added that he had never "entertained a doubt that those features of the phaneron that I have found in my mind are present at all times and to all minds" (1.284). Notice that he does not doubt the reality of persons. While physical reality is not considered within phaneroscopic analysis, physically realist assumptions are made as one sets about performing that analysis. For one thing, one assumes that there are other persons to whom one can communicate one's results. And to do that, one must be able to cite real things experienced: e.g., "Consider a color sample. Now, are you able, as I think I am, to distinguish the color itself from the fact of its presence in this patch of cloth?" Entertaining no doubt that others' experiences are like one's own is undogmatic: for it is checked by the fact of agreement or disagreement, and the fact is, there is a lot of agreement. Peirce found in the study of great men that impressions of greatness "do not, for the most part, differ extravagantly among different persons in the same environment" (7.265).

In both studies, observation is difficult because it cannot employ instruments or criteria and therefore depends wholly on training the observer in nicety of judgment. And, because there are no instrumental checks or evidence related by criteria to the qualities observed, judgment can be corrected and improved only by comparison to other judgments. While "every reader can control the accuracy of what I am going to say about them [phanerons]" (1.286), this can be done only if the reader repeats "the author's observations for himself" (1.287). "Indeed, he must actually repeat my observations and experiments for himself, or else I shall more utterly fail to convey my meaning than if I were to discourse of effects of chromatic decoration to a man congenitally blind" (1.286). This last follows from their being no criteria, hence, no definitions, of what is observed. Phaneroscopy has an additional difficulty: "These observations escape the untrained eye precisely because they permeate our whole lives," (1.241; the reference is to the philosophical sciences in general, but of these phaneroscopy is the basic science). Nevertheless, agreement testifies to the reality of what is observed about experience. Here again, we can cite the agreement that was achieved in the study of great men.

To both studies, mathematical analysis is essential; it provides the grounds of agreement or disagreement in judgment. And agreement, in turn, is measured mathematically. In the study of great men, impressions of greatness yield rank ordering of differences in greatness; to these, numbers can be assigned and thus the impressions of different observers can be compared quantitatively and found to agree or disagree. A major point of the study was to apply statistical analysis to those numbers, finding mean values of several observers' estimates of the relative greatness of a given individual and computing variances of individual estimates from those means. The variances being small, the agreement is seen as correspondingly great. And thus objectivity is achieved even in this matter seemingly so subtle and seemingly so subjective. In phaneroscopy, application of concepts drawn from the algebra of relations provides a common language within which to describe and classify features of experience. It is essential that this language is from pure mathematics, as thus it imports no physically realist assumptions. If phaneroscopists can agree about which features are monadic, which dyadic, and so on, then that attests to the reality of the features they descry.

Beyond the fact that Peirce founded a new empirical science, one that he designed to play a basic role in philosophy, there is a further philosophical consequence of his experience in measuring and his investigation of the art of measuring. That consequence bears on what we find most difficult and baffling in much of his writing – his late essay on "A Neglected Argument for the Reality of God" (EP2: ch. 29) is an extreme example – namely, its making much of mere feeling. We are stunned, for example, that he should define science itself neither by its theories nor by its methods but by the "spirit" that animates its practitioners (6.428). Surely something as nebulous as "spirit" cannot serve to distinguish the hard-headed practice of science from, say, what it is that theologians do. We expect someone trained in mathematics and chemistry, who earned his living making precise measurements in geodesy and astronomy, and who contributed fundamentally to the development of modern formal logic, to come up with crisper definitions than that. Those expectations, I suggest, are based on our not having had Peirce's own direct acquaintance with scientific measurement united with his powers of logical analysis. We are captives of a caricature of the scientist; many scientists are captives of it, too. Peirce knew from logical analysis of his own experience measuring, that nuances of feeling are the spring from which the stream of objective inquiry flows.

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