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INDEX

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FORNER WASHINGTON AND SCHOOL STREET, BOSTON

BOSTON, Jan. 10, 1888.

Ninth meeting (annual) of the Society.

Two hundred and fifty persons present. Professor H. P. Bowditch in the chair.

Record of the previous meeting (Nov. 29, 1887) read and approved.

List of persons elected Associates by the Council since the previous meeting of the Society was read.

In the absence of the Treasurer, the Chairman called upon the Assistant Treasurer to make an informal statement as to the financial position of the Society.

Mr. W. L. Parker and Dr. J. W. Warren were appointed auditors of the financial statement to be made by the Treasurer.

The following were elected to the Council, to hold office till 1891:—

Dr. W. S. BIGELOW,	Prof. W. JAMES,
Mr. C. B. CORY,	Prof. S. P. LANGLEY,
Prof. G. S. FULLERTON,	Prof. E. C. PICKERING,
Mr. R. PEARSALL SMITH.	

Professor C. S. Minot read the report on the diagram returns received by the Committee on Experimental Psychology.

Professor Royce read the report on the narratives received by the Committee on Phantasms and Presentiments.

Professor James reported on some cases of "Automatic Writing." The meeting then adjourned.

RICHARD HODGSON, *Secretary*.

#### REMARKS ON MR. PEIRCE'S REJOINDER.

BY EDMUND GURNEY.

I WILL endeavor to make the present reply as short as possible, my object being, not so much to make controversial points, as to ensure, as far as possible, that Mr. Peirce's treatment of the evidence and argument for telepathy shall not prevent his readers from studying them at length and at first-hand. Consequently I shall say little or nothing on matters where I believe that an impartial study of what has been said in "Phantasms of the Living," or in my previous reply, obviates the necessity of further explanation and defence, nor shall I attempt to put what I have to say in connected literary form. It will be enough to state the points which need stating, one after another, with references to the pages in Mr. Peirce's last paper.

Mr. Peirce's treatment of the question of general probabilities (p. 182) seems to me completely fallacious. My argument was, that a particular series of events, of the sort known as coincidences, could not, according to the doctrine of probabilities, be due to chance. My calculation made out the degree of probability against chance, as the cause of these coincidences, to be enormous; and Mr. Peirce objected to my figures, on the ground that in mundane affairs probabilities never really ran so high. I accordingly gave a simple practical instance where they ran higher, — an instance yielding a probability of almost incalculable magnitude that a particular series of events was not due to chance. The instance was that of a pair of dice turning up sixes a hundred times running, from which we should unhesitatingly conclude that the dice were loaded. Here was a case of an enormous *a posteriori* probability against a chance causation, exactly parallel to my case of the coincidences. Mr. Peirce, not being able to directly deny the legitimacy of the illustration, confuses the question by introducing a case of *a priori* probability, totally irrelevant to the matter in hand. He supposes the throw of a single die, which we should ordinarily regard as certain to turn up one or another of its faces, but in respect of which there is an appreciable possibility that it "may rest on its vertex, or fly up to heaven, or vanish altogether, or that, before it reaches the table, earth and heaven shall be annihilated." It would be easy, but it is unnecessary, to demur to this statement on its own account. The question is not of the appreciable possibility that one new and extraordinary event will occur at all; but of the appreciable possibility that a series of events, similar in character, but no one of them new or extraordinary, has occurred by chance. Mr. Peirce may hold, if he likes, that the probability, which plain men would describe as certainty, that his die will not accidentally fly up to heaven, rests on "assumptions," and "refers to an imaginary state of things;" it is enough for me that the probability which plain men would describe as certainty, that my dice did not accidentally turn up sixes a hundred times running, rests on no assumptions, and refers to the actually existing state of things. In what way, when estimating such a probability numerically in the analogous case of the coincidences, I can be held to "admit" that the number has no real significance, I am at a loss to conceive.

As regards Mr. Peirce's remarks on p. 182, bottom, and p. 183, top, it seems enough to refer to my former reply, p. 176, bottom, and 177. I do not "suppose that hallucinations are experiences particularly well remembered" (p. 183), in the sense which Mr. Peirce implies. I hold them to be neither better nor worse remembered than other equally rare and striking experiences.

Mr. Peirce cannot, I think, have given much time or care to the subject of hallucinations, or he could not have put forward (p. 184), as the one type of "genuine hallucination," "the product of an overwrought brain, which is preceded by great depression, accompanied by faintness, and followed by an access of terror." Such hallucinations are very rare, and are no more "genuine" than numbers of others. (See "Phantasms of the Living," Chap. XI., on "Transient Hallucinations of the Sane.")

So far from there being "no good reason for limiting the census-question to a period of twelve years" (p. 184), there were two very good reasons: (1) the imperfection of human memory, of which Mr. Peirce supplies an instance, since he describes a hallucination of his own, of thirty years ago, as having all but escaped his recollection; and (2) the fact that most of the best established coincidental cases, with which the non-coincidental cases had to be compared, fell within the assigned period.

The census inquiry was not limited, as Mr. Peirce represents, to hallucinations presenting persons really alive, but to hallucinations presenting persons who, as in the coincidental cases, were believed by the percipient to be alive; and so far from this involving a "fallacy," a fallacy would have been involved in reasoning conducted on any other basis.

The error in Mr. Peirce's argument at the top of p. 185 may be best shown by an illustration. Suppose I put an advertisement in the papers, asking persons who have had small-pox, though vaccinated in childhood, to communicate with me; and suppose my appeal to reach a circle of two hundred and fifty thousand people, strangers to me, of whom five take the trouble to write and tell me that they have had the experience in question. And suppose that I address inquiries on the same subject to the one thousand people most nearly connected with me and with my few intimate friends, and find that five out of the one thousand have been similarly affected. Mr. Peirce would apparently conclude that the one thousand form a class two hundred and fifty times as "fertile" in cases of small-pox as the general population. Most other people, I fancy, would conclude that only a very small proportion of the newspaper-readers who had had the experience had answered my appeal. As regards my alleged *petitio principii* (p. 185), I can but refer once more to the sentences from the opening of Chap. XIII. of "Phantasms of the Living," quoted in my last paper, p. 176.

P. 186. Mr. Peirce says: "Persons who, from the percipient's stand-point, appear particularly likely to die, are, we find, particularly apt to appear in hallucinations." I suppose that this statement is founded on those cases in "Phantasms of the Living"—an extremely

small proportion of the whole number—where the so-called "agent" was known by the percipient to be seriously ill. But even if such hallucinations were numerous enough to justify Mr. Peirce's assertion, at least two strong objections may be urged to his conclusion, that they must have been due to the percipient's knowledge of the illness.

(1.) By what right does he assume the correctness of the evidence for the fact and the circumstances of the hallucination, in these particular cases, while disputing it in the far more numerous cases where the "agent" was supposed by the percipient to be in normal health? The evidence must surely be judged, throughout, on its own account, and not be picked to suit a particular hypothesis. And of two rival hypotheses, that which covers all the facts, as telepathy does here, is naturally to be preferred to one which only covers a small, arbitrarily-selected group of the facts.

(2.) How does he account for the close correspondence, in time, of the hallucination with the death, in the cases—of which the small class in question chiefly consist—where the more or less serious condition of the "agent's" health had been equally well known to the percipient for weeks, and even months, before?

Mr. Peirce's next sentence (p. 186) reproduces his gratuitous and erroneous view of "genuine hallucinations," already sufficiently noticed (p. 288). The "peculiar terror" is an extremely rare concomitant.

To the two pages 187-9 ("In the discussion of each story"—"destructive of sound judgment") I can give no better reply than is already given in the "General Criticism of the Evidence" ("Phantasms of the Living," Vol. I., Chap. IV., pp. 161-72). I hope that Mr. Peirce's readers will consult that chapter before accepting his sweeping statement that telepathy is opposed to "some of the fundamental elements of the general conception of nature," and to "the main principles of science."<sup>1</sup> Even less defensible is the view, by which much of the remainder of his case is vitiated, that it is sufficient to suggest "an explanation for each story more probable than the telepathic explanation." This, of course, entirely ignores the quintessential point of the telepathic argument—the *cumulation* of similar instances. A single illustration—that of the Alice—will again serve. If the dice turn up sixes once, by far the most probable explanation is, that they did so by chance, and no sane person will conclude that they are loaded; but if they turn up sixes a hundred times running, no sane person will conclude anything else.

<sup>1</sup> As to the alleged *rarity* of telepathic effects "we must not be too positive that the telepathic action is confined to the well-marked or extensive instances on which the *proof* of it has to depend." (See "Phantasms of the Living," Vol. I., p. 97.)

P. 189. I have never admitted that I had "the bias of an advocate;" what I admitted was some slight (very slight) justification for Mr. Peirce, if he chose to regard me as an advocate. I approached the subject quite as sceptically as he did; and to this day I agree with him in professing "a legitimate and well-founded prejudice against the supernatural."

A little lower, Mr. Peirce's bare assertion that one of his old objections is "logically sound" is less persuasive than would be some reply to the passage (pp. 158-9) in which I have proved it, as I conceive, to be the reverse. But, as he withdraws the objection, I need say no more about it.

Mr. Peirce has certainly not added to the force of his third objection. The hallucinations in the coincidental cases of the class under debate were recognized as representing particular persons. It is of hallucinations of this class, and of no other, that account has to be taken in estimating the comparative frequency of non-coincidental cases. Whether a recorded hallucination was of the "recognized" class was one of the details as to which inquiry was made after the more general census question had been answered in the affirmative. (See "Phantasms of the Living," Vol. II., p. 7, note.) Mr. Peirce's argument here is curiously suicidal; for, even if it were the case that persons who have had occasional impressions of a quite different kind were "abnormally subject" to this particular type of hallucination, they would be more, and not less, likely than other people to recall instances, which is just what not one of the percipients in the cases to which Mr. Peirce objects has been able to do.

P. 190. Objection 6. In his first comment Mr. Peirce seems to have missed my point. Once more let me repeat, what had to be done was to make a numerical comparison of certain coincidental hallucinations with non-coincidental cases similar in kind. For a non-coincidental case to be included in the statistics used it would be sufficient that the percipient *believed himself* to be awake at the time of his experience. I should not have been justified in rejecting a case merely because I had not conclusive proof that he *was* awake; and the coincidental cases had, of course, to be treated on the same principle. I may add that the belief in question is itself a very strong proof of its own truth, since it very rarely happens that after waking from a dream we continue to believe that it was a piece of real waking experience. To Mr. Peirce's second comment I can allow no weight. There is absolutely nothing in the fact of the coincidence to lead the percipient to conclude that he had been awake rather than asleep at the time of his experience. Rather is the tendency of percipients, shown in several cases, to persuade themselves, as time goes on, that what was

clearly recognized at the moment as a rare thing, viz., a waking hallucination, had been no more than a common thing, viz., a vivid dream, likely to be increased by the fact of the coincidence; which is clearly easier to explain by the natural hypothesis of accident, if the percipient's experience belonged to a *common* class, than if it belonged to a *rare* class.

Objection 12. "The percipient may have been intoxicated," etc. So equally may the subjects of the non-coincidental cases have been. So equally, of course, ~~when~~ they not.

Objection 15. A. "He may be lying." The improbability of cumulative and concordant lying is ignored, like the whole of the cumulative argument. B. The hallucinations have in most cases been quite unlike "ordinary indistinct vision," or "dreams." They have been clear and definite. C. The memory of the hallucination has located it definitely in time and space, which entirely differentiates the cases from the common vague impression of having been in the same situation before.

Objection 16. I have nothing to add to the concluding sections of Chap. IV. of "Phantasms," already referred to. I will just repeat that "it is very important to avoid confounding the natural growths on the margin (so to speak) of a telepathic record with the vital point at its centre; or concluding that the latter is as likely to be unconsciously invented as the former."

Objection 17. I must maintain that the clearly-stated, adorned, and corroborated piece of evidence which Mr. Peirce condemns as "meagre," differs completely from the natives which one "may hear in an endless flood," by frequenting the company of marvel-mongers, or even in ordinary society, where unscientific credulity is often the prevailing temper. Whether or not such a piece of evidence "*must*" go for nothing, it certainly *will* not go for nothing, in the eyes of any impartial reader, in whose eyes I am not thoroughly incompetent for my work.

Case 26. I have nothing to add to my remarks on pp. 164-5. I, of course, "use the case as a premise from which to draw a conclusion" of the high degree of probability which has so offended Mr. Peirce, just as I should use each of the hundred throws of sixes to support a similar highly probable conclusion that the dice were loaded.

Case 27. I dissent from Mr. Peirce's remarks, but am quite content to leave the question to the reader; merely protesting against the monstrous assumption "that the probability that this deceiver would be represented in any hallucination that the percipient might have at this time was four-fifths." A little study of the subject of hallucinations would have taught Mr. Peirce that the hallucination was every

bit as likely to represent the percipient's wife, or a servant in the house; and far more likely to represent one or another member of this daily-seen class.

Case 28. Again dissenting from Mr. Peirce's treatment, I am quite content to let the reader form his own opinion. It is amusing, by the way, to find Mr. Peirce driven by the momentum of his argument into eulogizing the judgment and observation of one witness, of whom all that appears is — that he believed in telepathic communications on insufficient evidence! Mr. Peirce concludes his comment with a similar monstrous assumption to that noticed in the last case.

Case 29. The percipient's testimony as to his health is this: "I never felt better in my life; there was nothing in the least amiss with me." In the original account he says that while peering forward, for a special purpose, he "slightly stumbled on a hassock of grass, and looked at my feet for a moment only." On the strength of this sentence Mr. Peirce describes the man as "stumbling about the churchyard," — a very characteristic piece of misrepresentation, small in appearance, but eminently calculated to prejudice the reader. He proceeds to adduce as a suspicious circumstance that, "when the percipient got home, he half thought what he had seen must have been his fancy." I go further, for I have hardly a doubt that it was "his fancy," — in other words, that what he saw was a hallucination. How does that affect the improbability that this fancy, and others of the same sort, would, by chance, closely coincide with the death of the person represented? A little further on, the "monstrous assumption" — as to the probability that this particular person would be the object of the hallucination — duly reappears; partly based in this instance on another — "as the news of the death reached the town the next morning, it is fair to assume that the gardener was aware of the illness of the decedent." This is a specimen of the assumptions which Mr. Peirce regards as "fair." The contrary of what he supposes seems sufficiently implied in the account; but the evidence is certainly improved by the following explicit statement: "I had no knowledge that Mrs. de F. was ill, and was not even aware that she was away from Hinxton. Alfred Bard."

Mr. Peirce says, "If we had a better acquaintance with the witness than is conveyed by the vicar's banal certificate to the man's character." The vicar's certificate may be "banal," but it is at any rate explicit and based on thorough knowledge. But "we" have "a better acquaintance," in so far as first-hand acquaintance is better than second-hand; for Mr. Myers and Mr. Hodgson<sup>1</sup> have seen and carefully examined the witness.

<sup>1</sup> Mr. Bard was interviewed, I think in the summer of 1883, by Mr. Myers and myself, and we questioned him closely concerning his experience. R. H.

P. 197, top. Referring to a remark of mine on p. 170, line 8, I much regret the misprint, whereby a repetition of the last five letters of the word *through* has produced an interpolation of the adjective "rough" before "inadvertence." When I found, too late, that I had overlooked this word in the proof, I hoped that (apart from manners) its fatuity in point of style would suggest the nature of the error; but most authors have had occasion to mourn the baselessness of such hopes.<sup>1</sup>

Case 172. I dissent from Mr. Peirce's objections, and I, on my side, should be glad to know, (1) why he calls an apparition seen with the eyes open, and after the percipient had started up in bed and looked round, a dream; (2) how he would support his assertion that it was "practically certain that the dream would relate to the decedent," which implies, of course, that the witness could not dream of any human being except this particular friend during all the months of the friend's illness.

Case 173. Mr. Peirce's "explanation" involves, besides several assumptions, the conjunction of at least two improbabilities, — the production of a vivid dream by the mention of a name outside the cabin, and the continuance, in waking hours, of the belief that the dream had been a piece of waking experience.

Case 174. Beyond noting once more the monstrous assumption of "an antecedent probability of nine-tenths" that the person who appeared would be the object of any hallucination which the percipient might have at the time, I need only refer to my former remarks, pp. 160-1, 174-5.

Case 175. I willingly leave to the reader the decision as to whether Mr. Peirce is justified in dismissing as a dream an experience of which the percipient uses such expressions as these: "I thought I saw him there after dreaming. I arose and rested on my right elbow, looking at him in the dusky light. I am sure that as the figure disappeared I was as wide awake as now." The percipient's former purely subjective dream-experiences, which he expressly distinguishes from the present case, were, as I rightly say, "not hallucinations at all in the sense in which I throughout employ the word." Mr. Peirce's assertion that they were "hallucinations, according to the definition of the census-question," is quite without foundation; for the census-question related expressly to waking experiences.

Case 182, with the corroboration of the percipient's cousin, given in my last paper, reduces Mr. Peirce to rather desperate straits; and as the "wonderful hypnotoid sensitiveness," leading up, weeks

<sup>1</sup> I may note here another misprint, which occurs on p. 175, line 27. The "case which happened a few months earlier" than case 109 is No. 500.

afterwards, to an accurately-timed hallucination, is in my view as groundless an assumption as the "important suppression or falsification of the testimony." I must continue to think the case a very strong one. Our old friend, the "monstrous assumption," reappears, in a particularly monstrous form, in the supposition that the antecedent probability that the hallucinations would relate to the decedent was one-half.

Case 184. In saying that the words "a vivid impression of seeing a human being" define what we have to understand by a hallucination, for the purposes of the present argument, Mr. Peirce has made a serious error. He does not seem to have observed — what is stated in "Phantasms of the Living," Vol. II., p. 7 — that the details of the hallucinations mentioned in answer to the original census question were "a matter of subsequent inquiry." One of these details, as I have said, was whether the figure seen (or the voice heard) was recognized as that of a person known to the percipient. And I must, it seems, once more point out the obvious fact that the only hallucinations which could properly be included in my estimate are those of the same character as the coincidental group which I present as properly telepathic. — i.e., they must be of the "recognized" class. It is worth noting that had I made the mistake which Mr. Peirce, it seems, would defend, of including *unrecognized* non-coincidental hallucinations in the reckoning, his own "monstrous assumption" of an immensely high probability, sometimes even of certainty, that any hallucination that befell the percipient in the coincidental cases would represent the person whom it did represent, would become more monstrous still, since it is only a minority of visual hallucinations that represent recognized figures at all.

Mr. Peirce's proof that the hallucination was determined by a state of anxiety on the part of the percipient is surely not one that he can reflect on with much satisfaction. He says: "The decedent was a child of his [the percipient's], five years old, who had been removed from his parents, and from Paris to London, on account of an outburst of small-pox." He omits to add that the removal took place in December, while the apparition did not take place till the 24th of January, and that in the course of the month's interval several letters had been received giving an excellent account of the little boy's health. More than this: the hallucination, which conveyed the impression of a happy laughing child, left the percipient saying to himself, "Thank God, little Isidore is happy as always;" and he describes the ensuing day as one of peculiar brightness and cheerfulness. The assumption that it was "antecedently practically certain that any hallucination at that time would relate to the decedent" is a robust specimen of its class.

Case 195. As to the supposed anxiety, I may simply refer to the remarks in my former paper, on p. 161. Mr. Peirce's point, that the percipient "ought to know her own imagination better than Mr. Gurney can do," has no force; for she has had no other hallucination, and therefore has no claim (such as some abnormally vivid visualizers might have) to speak with authority on the power of her imagination to conjure up fictitious sensory experiences. But of course the attribution of a sensory hallucination to "a strong imagination" would be a very natural and defensible hypothesis, even for a coincidental case, if the case stood alone; it is the accumulation of coincidental cases, of which the percipients themselves knew nothing, that justifies us in rejecting the hypothesis of a purely subjective origin for all of them. The matter is one of statistics, where the collector is an authority, and the contributor, as such, is none at all.

Case 197. I have here to admit a piece of inadvertence. When giving the additional evidence under head 16 (pp. 172-3), I did not recollect that it affected my remarks under head 8 (p. 165). The retention of those remarks is, however, of no importance, for in my summary (p. 175) I say, "As regards closeness of coincidence, the recent information as to case 197, though improving the quality of the evidence, removes it from this particular death-list."

Mr. Peirce's paragraph (p. 203), beginning "Mr. Gurney admits the coincidence of time is not proved to be within twelve hours," shows a curious misunderstanding of my meaning. I never dreamt of taking advantage of the fact that the twelve hours' limit was arbitrary, to include in a particular estimate, based on a twelve hours' limit, cases where that limit was known to have been exceeded, and I should not have thought that my remarks on page 165 could have been so interpreted. For the purpose of the estimate, the inclusion, with "due warning," of "two or three cases" where the chances are about even that the twelve hours' limit *was* or *was not* slightly exceeded, seems the more defensible in view of the large number of included cases where the coincidence was *much closer* than the said limit.

Mr. Peirce is so fond of assuming it as a certainty that the person actually represented would be the object of any hallucination that the percipient might have at the time, that we ought to be grateful for the probability of nine-tenths that he substitutes in this case, and which is, perhaps, not more than fifty times too large.

Lastly, unless Mr. Peirce could show how the words, "the coincidence cannot have been as close as Mrs. Bishop imagines," implied that Mrs. Bishop had imagined it to have been exact, he should not have labelled my perfectly true statement as a "mistake," in order to father his own upon it.

Case 199. I cannot conceive what Mr. Peirce finds *independent* of the vision to mark the day of the week on which the vision fell. The words which he quotes relate to the vision, and to nothing else.

His remark about changing the limit to thirteen years, "for the sake of including a known instance," is quite wide of the mark. Any limit of years that was selected would have included a certain number of "known instances;" and what is there "unwarrantable" in my true statement, that, had thirteen years been selected instead of twelve, "the numerical argument would not have suffered appreciably, if at all"?

Case 201. I willingly leave the case to the reader, merely drawing attention to the misleading brevity of Mr. Peirce's assertion that the percipient "suspected she might have been asleep." Her words, which he compels me to re-quote, are, "I tested myself as to whether I had been sleeping, seeing that it was ten minutes since I lay down. I said to myself what I thought I had read, began my chapter [of Kingsley's *Miscellanies*] again, and in ten minutes I had reached the same point."

Mr. Peirce says, "Mr. Gurney gives up the case, and I am not inclined to give it any weight." I concede its omission from this particular calculation, owing to the uncertainty as to the degree of closeness of the coincidence, but I continue to give it great weight.

On case 202 I have nothing to add.

Case 214. I do not understand Mr. Peirce's probability of one in one hundred, but suppose that he means it as the probability that the story of the hallucination is untrue. I do not consider his suspicions well grounded, the account of the shock, and its sequel, having every appearance of truth. If the hallucination took place, its date, owing to the consequences, would be specially well marked, and the odds against the coincidence would be enormous.

My estimate of Case 231 differs considerably from that of Mr. Peirce, who, I think the reader will agree, overshoots his mark in making it count for *less than nothing*. But, owing to the uncertainty as to twelve hours' limit, I have conceded its omission from this particular list.

Case 236. I have nothing to add beyond noting that the assumption of the antecedent probability that the hallucination would refer to the decedent as *nineteen-twentieths* is perhaps Mr. Peirce's masterpiece in that line. Even if we neglect the facts of hallucination in general (as, for instance, their tendency to take the form of "after images," and to represent objects which the percipient is in the daily habit of seeing), the above exemption would at least imply — what there is not a syllable in the account to suggest — an utterly abnormal

absorption of the percipient's mind by the thought of one particular relative.

Case 237. Mr. Peirce thinks it "important" that the mother of the percipient "thought she might have been dreaming." The mother does not say so in her own evidence, and all that her daughter says is, that she "was greatly amused at my scare, suggesting I had read too much or been dreaming." If Mr. Peirce ever has a waking hallucination while he is reading, and at once mentions it to some one in the room, I would wager a good deal that the same comment will be made; and if he is good enough to send me an account of the occurrence, I engage not to think the objection "important," even though the objector, like the mother in the case before us, should "continue to disbelieve in ghosts." If I were the "advocate" that Mr. Peirce considers me, I should certainly rely on his treatment of this case to do more for me with the jury than the best of my arguments.

Case 238. "I assume it as antecedently certain," we read, "that the hallucination would refer to her husband, whom she seems to have loved. This is the assumption the most favorable to telepathy, since he was a well man." Mr. Peirce omits to tell us how he has learned that she did not love any other "well" man, woman, or child; and by what statistics he has ascertained that a person must be loved, in order to become the object of a hallucination.

Case 240. The signification which Mr. Peirce quite unwarrantably squeezes out of a pluperfect is contrary to the fact. There had been no "reconciliation" between the percipient and the dying man; nor was she aware, at the time, of her mother's visiting him. I must continue to characterize Mr. Peirce's assumption of a practical certainty that the hallucination would relate to the decedent, as monstrous.

Case 249. In connection with this case, I would refer the reader to the remarks on mistakes of identity, and their relation to the cumulative argument in "Phantasms of the Living," Vol. II., pp. 62-63. The percipient, it will be observed, had as little doubt as to who the person was whom he had seen as if the whole figure had been in view; in that sense the recognition was perfect, which is all that the argument requires. The "monstrous assumption" in this case (an antecedent probability of one-half that the hallucination would represent the decedent, on the ground of his being a neighbor, not known to be seriously ill) is a veritable Mammoth.

Case 298. With our knowledge of the witness's character, we find it impossible to doubt that the news of the man's illness and death reached her in the way described.

Case 300. As to the case itself I have nothing to add. I wholly



dissent, however, from Mr. Peirce's view that "the reality of ghosts is put beyond doubt at once," if sailors' yarns are believed; for the ghostly incidents in such yarns could almost always be explained on the hypothesis of purely subjective hallucination or illusion.

Case 350. I do not think that I endeavored to "make much" of Mr. Peirce's mistakes in relation to the facts of this case. Nor do I even complain of his hypothesis, except so far as the statement of it implies the erroneous view that a case has no legitimate place in a cumulative argument in favor of one explanation, merely because another explanation is conceivable. Personally, I think the hypothesis that the witnesses had a hallucination, decidedly more probable than Mr. Peirce's suggestion of the pendent skull; and I cannot help thinking that *had there been no coincident death*, and no telepathic theory to confute, he would have agreed with me. Yet it must be clear that, in estimating the relative probability of the two explanations (hallucination and skull), we have nothing whatever to do with the coincidence. We ought to forget it. And even if we remember it, it will, of course, tell for, and not against, the hypothesis of hallucination, *since* it brings in the chance (which Mr. Peirce would admit to exist, however infinitesimal he would consider it) that there was a hallucination of telepathic origin, in addition to the chance that there was a hallucination of purely subjective origin.

Case 695. As for the "meagreness" of the story, a clear statement of all the essential facts, given without a word suggestive of adornment or exaggeration, is not evidence which a disparaging epithet will much injure. The words "told at second-hand," though true, are misleading. I have explained ("Phantasms of the Living," Vol. I., pp. 148-9) that "the evidence of a person who has been informed of the experience of the percipient, while the latter was still unaware of the corresponding event," is quite on a par with the percipient's own evidence; indeed, in some ways it is even preferable. And it is, of course, at its best when, as in this case, the information has led the witness at once to make a written note of the date, which leaves absolutely no doubt as to the coincidence. Mr. Peirce's hypothesis of anxiety, which Mrs. Teale "concealed in order not to alarm her husband," is quite gratuitous. Her husband says that she was not anxious, and not given to brooding, and the last news of her son had been reassuring. The "monstrous assumption" — of an antecedent certainty that the hallucination would relate to the son — reappears in due form.

Case 697. Mr. Peirce having surmised that the percipient had heard of the death during the day, I stated that the surmise was incorrect, as most readers of the account would perceive. He urbanely replies

that my testimony "goes for nothing." I will not, however, do him the injustice of supposing that he really doubts my statement to have been made on authority, — that of our informant, Mr. B.

The assumption of a high antecedent probability (one-fourth) that the hallucination could relate to the decedent — a clerk in the office of the percipient's husband, whom she had only occasionally seen, and as to whom even her husband was "in no anxiety" — is in this case ludicrous as well as monstrous; for Mr. Peirce bases it on the fact that her husband, in telling her of the young man's death, used the hackneyed phrase "sad news," which, says Mr. Peirce, "shows that her pity had been excited"! He should really be a little more consistent in his view of the emotions which beget hallucinations. A little time ago it was *conjugal love*. A woman loved her husband, and this made it certain that any hallucination of hers could represent no one but him. But now the degree of *pity* which is implied in the fact that somebody who tells one of the death of an acquaintance calls the news "sad," is found to have immense power in the same direction. And hence a dilemma: for Mr. Peirce must assume either that the loving percipient had not this degree of pity for any human being, or that the pitying percipient did not love her husband.

Case 702. "In his original account the percipient has the year wrong." This is Mr. Peirce's version of the fact that, writing in May, 1886, without referring to documents, the witness describes an event which had really occurred three years and eleven months before as having occurred "some three years since." I have explained that the percipient's mistake as to the *date* of the coincidence has no importance, since it has no relation to his evidence as to the *fact* of the coincidence. When he handed me the longer account (giving the date of the death, which proved to have coincided with his vision on June 15) he said that he was trusting to memory for the date, but that he believed he could hunt up the letter which contained it. He did so the same evening, with the result which it pleases Mr. Peirce to describe as "cooking" the story. The date, June 15, actually occurs in the portion of the letter quoted, where it is given as the day of the funeral, the death being simply stated to have occurred "on St. Barnabas' day" (June 11). Thus the mistake was not only unimportant, but extremely natural.

As to the case in Vol. I., p. 130 [misprinted 230 in Mr. Peirce's rejoinder], note, though precluded from giving it in detail, I regard it as of great value. The difference between it and fully reported cases is merely that, in respect of it, the reader is more dependent on the judgment of those who present the evidence. I have said that the narrative was of the ordinary type and unsensational in character;



and that the witness was not biased by a credulous love of marvels appears from her remark that, though "confident of having seen the vision [of an old school-friend who died on that day at a distance], her common-sense makes her "wish to put it down to imagination."

Mr. Peirce's concluding remarks, where he repeats his heroic hypothesis as to the "millions" whom our appeal for evidence has tapped, call for no special reply. What I have to say on the important point of the value that may be attached to "unscientific observations" is said at length in "Phantasms of the Living," Vol. I., Chap. IV.

#### POSTSCRIPT TO MR. GURNEY'S REPLY TO PROFESSOR PEIRCE.

BY FREDERIC W. H. MYERS.

MR. GURNEY did not live to give his final revision to the above paper; and in the course of correcting the proofs an inaccuracy in his earlier "Remarks on Prof. Peirce's paper" has been observed by us, which, so far as it goes, tells in his own favor. I shall, therefore, correct it here, as my only addendum to this his latest word of controversy. I see, indeed, several arguments by which his chain of reasoning — strong as that seems to me already — might be reinforced. But I cannot say with certainty how far he would have pressed any of these arguments himself. And, on the other hand, I am absolutely sure that he would never knowingly have allowed a single sentence to stand which overstated his own case in the smallest particular.

In Proceedings, p. 161, first paragraph, Mr. Gurney states that, in his census of hallucinations, questions as to the person's bodily or mental state at the time of the experience were kept separate from the question as to the fact of the experience. This is entirely true of the *mental*, but only partially of the *bodily*, state. For the question on the census-paper was, "Have you ever, when in good health and completely awake, had a vivid impression of seeing, or being touched by, a human being, or of hearing a voice or sound which suggested a human presence, when no one was there?" Inquiries as to date, recognition, anxiety, as well as further inquiries as to health, were made subsequently. Thus, Mr. Gurney was the sole judge as to what degree of *anxiety* should exclude a case from the census; but the percipients themselves were, in the first instance, the judges as to what degree of *ill-health* should exclude a case from the census; and, consequently, Professor Peirce's objection to the inclusion in the group of evidential cases of certain cases where he thinks there was *anxiety* falls to the ground; his objection to the inclusion of

cases where there was *ill-health* has logical validity. For, so far as the *anxiety* went, the same canon was applied by Mr. Gurney to both the groups which were compared together, the evidential group and the group of miscellaneous hallucinations; and the degree of anxiety which excluded a case from the one group excluded it also from the other. But, so far as the *ill-health* went, the respondents in the miscellaneous group might conceivably have answered "No" to the first question in the census-paper, if they had seen a hallucination when slightly unwell, and might then have judged themselves by a standard of health stricter than that used by Mr. Gurney in testing cases to be admitted into the evidential group. Cases 174 and 702 should, therefore, in strictness be dropped, — not, of course, from the evidence in general, but from this particular comparison between the two groups. And, in fact, Mr. Gurney admits this on pp. 174, 175. It is plain, therefore, that his erroneous statement on p. 161 was a mere slip of expression, due, no doubt, to the fact that, in actual practice, the appraisal of *ill-health* (as well as of anxiety), in the miscellaneous group, was mainly left by the respondents to Mr. Gurney himself. If the respondents had seen a hallucination at all they usually answered "Yes," whether they had been somewhat out of health at the time of seeing it or not. This we know partly from the testimony of those who collected the answers, and partly by the evidence on the face of the answers themselves. The error above pointed out, therefore, has probably had but very slight effect on the calculation; and, in any case, it is amply met by dropping cases 174 and 702 from the group used for comparison.

I may add that Mr. Gurney by no means considered that the information which he had obtained as to hallucinations, by his census and other methods, was enough. He always intended to take a further census before long. It is to be hoped that his example, in thus substituting the laborious but fruitful methods of statistics for the vague generalities current on this subject before him, may be followed in England and elsewhere; and, in any future census, it would probably be better to leave the percipient's state of *health*, as well as of *anxiety*, for subsequent inquiries, and to make the question first asked as short and simple as possible.