air is between 32 and 40 degrees of Fahrenheit's heat scale (for that is the condition in which the danger is found to be most certainly incurred), should be most carefully maintained by the judicious use of sustaining food, and by the avoidance of every kind of injurious derangement or excess. When once internal congestion has been set up, and the cold has been "caught," the thing to be done is immediately to bring back vigorous circulation and exhalation in the skin. The Turkish bath is one of the most convenient and certain of all contrivances for ensuring this object: in its absence the vapour bath, or hot air bath may be employed. The action of the bath is to be reinforced by the administration of stimulants, first and foremost amongst which stands concentrated food. Indeed, the Professor's pet stimulant seems to be "Whitehead's Solid Essence of Beef," a New South Wales preparation, in which the nutritious principle of an ox is condensed into about nine pounds of easily transportable material, in which thirty pounds of beef are concentrated into one pound of little cakes, each about the size of an ordinary silver five-shilling piece, and weighing half an ounce. One cake is calculated to prepare two large breakfast cups of good beef-tea. This preparation differs from Liebig's Extract of Meat chiefly in containing the gelatinous as well as the fibrous constituents of the flesh. The Gresham Professor scattered the little round cakes, out of neat half-pound cases, liberally to his audience, recommending them to begin at once to fortify themselves against the inclement atmospheric influences. He gave one very interesting instance of the value and power of this preparation by alluding to a case that had fallen within his experience on the very day of the lecture. A patient had been brought into the Brompton Hospital in a sinking state, resulting from inability to take food. He was at the time all but pulseless and cold, and evidently on the brink of the grave. He was placed in bed, and a cupful of the beef-tea prepared from the "solid Essence" administered. The preparation was retained in the stomach, and in ten minutes from the time of its administration, there was steady warmth all over the skin, and restored circulation.

There is one expedient both for preventing and curing "colds," which was not alluded to upon this occasion, but which is nevertheless as powerful as any of the measures which were described, and it may sometimes be drawn upon in circumstances when those plans cannot be adopted, in consequence of the sufferer being compelled by the exigencies of life to continue to meet exposure to chilling influences. This is abstinence from drink, and liquid food of any kind, until the internal congestion is removed. The remedial action through the skin does its work by drawing away the superabundance of the circulating fluid from the overcharged part. But this desirable result is even more certainly ensured if the general bulk of the circulating fluid, or blood, is diminished by withholding supplies of the more liquid, or watery, ingredient; which may be done where the digestive power is unimpaired, without in any way diminishing the richer, or more immediately nourishing portion. The instant the general bulk of the circulating blood is diminished, the excess contained in the congested and overcharged membranes is withdrawn and the cold is relieved. Somewhat severe thirst sets in; but curiously enough, simultaneously with the occurrence of this thirst, the congested internal membranes grow moist, and exhale gently and naturally in consequence of the relief of the overcharged vessels. All that is then necessary is to keep the supply of drink down to the point which enables some measure of thirst to be maintained; and during its maintenance there is not the slightest chance of the recurrence of the cold. Dr. Thompson dwelt emphatically in his lecture, upon the fact that, whereas certain ailments, such as the eruptive fevers, bring with them an almost complete immunity from the recurrence of the affection, it is just otherwise with ordinary colds. The more frequently they occur, the more frequently they may be looked for. They bring with them increased susceptibility of the internal membranes to congestive derangements. Under such circumstances diminution of drink, sustained at the point of persistent moderate thirst, is the most powerful and certain preventive of congestive disorder, and the most sure remover of undue internal susceptibility, that can be adopted.

The second lecture was mainly devoted to a description of stimulants, and an experimental explanation of the way in which the amount of the saccharine, acid, and spirituous ingredients of wines may be ascertained. There is one very noteworthy peculiarity in all these lectures, which renders them peculiarly fit for the class of audience at which the Gresham College is aimed: Dr. Symes Thompson is a master of the art of giving

a clear notion of the whereabouts of a fact, or principle, to popular apprehension. It is to be extremely regretted that in a vast metropolis like ours, and, indeed, in all our large towns, such courses as these Gresham Lectures are not more common. It is absolutely impossible to over-estimate the good they can do, not only, as in this case, in showing what "to eat, drink, and avoid," but generally in inducing thought and work. Further lectures of the Gresham course are announced for the months of April, June, and September.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Where are the Nebulæ?

MR. PROCTOR's interesting paper in your last number reminded me of an essay on "The Nebular Hypothesis," originally published in 1858, and re-published, along with others, in a volume in 1863 ("Essays: Scientific, Political, and Speculative," Second Series), in which I had occasion to discuss the question he raises. In that essay I ventured to call in question the inference drawn from the revelations of Lord Rosse's telescope, that nebulae are remote sidereal systems—an inference at that time generally accepted in the scientific world. On referring back to this essay, I find that, besides sundry of the reasons enumerated by Mr. Proctor for rejecting this inference, I have pointed out one which he has omitted.

Here are some of the passages:—

"The spaces which precede or which follow simple nebulae," says Arago, 'and, *à fortiori*, groups of nebulae, contain generally few stars. Herschel found this rule to be invariable. Thus, every time that, during a short interval, no star approached, in virtue of the diurnal motion, to place itself in the field of his motionless telescope, he was accustomed to say to the secretary who assisted him, "Prepare to write; nebulae are about to arrive." How does this fact consist with the hypothesis that nebulae are remote galaxies? If there were but one nebula, it would be a curious coincidence were this one nebula so placed in the distant regions of space as to agree in direction with a starless spot in our own sidereal system? If there were but two nebulae, and both were so placed, the coincidence would be excessively strange. What, then, shall we say on finding that they are habitually so placed? (the last five words replace some that are possibly a little too strong). . . . When to the fact that the general mass of nebulae are antithetical in position to the general mass of stars, we add the fact that local regions of nebulae are regions where stars are scarce, and the further fact that single nebulae are habitually found in comparatively starless spots, does not the proof of a physical connection become overwhelming?"

The reasonings of Humboldt and others proceeded upon the tacit assumption that differences of apparent magnitude among the stars result mainly from differences of distance. The necessary corollaries from this assumption I compared with the hypothesis that the nebulae are remote sidereal systems in the following passage:—

"In round numbers, the distance of Sirius from the earth is a million times the distance of the earth from the sun; and according to the hypothesis, the distance of a nebula is something like a million times the distance of Sirius. Now, our own 'starry island, or nebula,' as Humboldt calls it, 'forms a lens-shaped, flattened, and everywhere-detached stratum, whose major axis is estimated at seven or eight hundred, and its minor axis at a hundred and fifty times the distance of Sirius from the earth.' And since it is concluded that our solar system is near the centre of this aggregation, it follows that our distance from the remotest parts of it is about four hundred distances of Sirius. But the stars forming these remotest parts are not individually visible, even through telescopes of the highest power. How, then, can such telescopes make individually visible the stars of a nebula which is a million times the distance of Sirius? The implication is, that a star rendered invisible by distance becomes visible if taken two thousand five hundred times further off!"

This startling incongruity being deducible if the argument proceeds on the assumption that differences of apparent magnitude among the stars result mainly from differences of distance, I

have gone on to consider what must be inferred if this assumption is not true; observing that "awkwardly enough, its truth and its untruth are alike fatal to the conclusions of those who argue after the manner of Humboldt. Note the alternatives":—

"On the one hand, what follows from the untruth of the assumption? If apparent largeness of stars is not due to comparative nearness, and their successively smaller sizes to their greater and greater degrees of remoteness, what becomes of the inferences respecting the dimensions of our sidereal system and the distances of the nebulae? If, as has lately been shown, the almost invisible star, 61 Cygni, has a greater parallax than a Cygni, though, according to an estimate based on Sir W. Herschel's assumption, it should be about twelve times more distant—if, as it turns out, there exist telescopic stars which are nearer to us than Sirius, of what worth is the conclusion that the nebulae are very remote, because their component luminous masses are made visible only by high telescopic powers? . . . On the other hand, what follows if the truth of the assumption be granted? The arguments used to justify this assumption in the case of the stars, equally justify it in the case of the nebulae. It cannot be contended that, on the average, the *apparent* sizes of the stars indicate their distances, without its being admitted that, on the average, the *apparent* sizes of the nebulae indicate their distances—that, generally speaking, the larger are the nearer, and the smaller are the more distant. Mark, now, the necessary inference respecting their resolvability. The largest or nearest nebulae will be most easily resolved into stars; the successively smaller will be successively more difficult of resolution; and the irresolvable ones will be the smaller ones. This, however, is exactly the reverse of the fact. The largest nebulae are either wholly irresolvable, or but partially resolvable under the highest telescopic powers; while a great proportion of quite small nebulae are easily resolvable by far less powerful telescopes."

At the time when these passages were written, spectrum-analysis had not yielded the conclusive proof which we now possess, that many nebulae consist of matter in a diffused form. But quite apart from the evidence yielded by spectrum-analysis, it seems to me that the incongruities and contradictions which may be evolved from the hypothesis that nebulae are remote sidereal systems, amply suffice to show that hypothesis to be untenable.

37, Queen's Gardens, Jan. 31

HERBERT SPENCER

Kant's View of Space

IN answer to my invitation, Mr. Lewes now "freely admits that Kant nowhere speaks of Space and Time as 'Forms of Thought,'" but still contends that 'Kant would not have disclaimed such language, as misrepresenting his meaning.' As well might he argue that although Euclid never uses the word *epipedon* (our English word *plane* or *plain*), to signify a curved surface (*επιφανεια*), he would not have remonstrated against the use of the term *cylindrical epipedon* or conical *epipedon*, to denote the surface of a cylinder or cone, in a professed exposition or criticism of his Elements of Geometry, because in common life we speak of rough or undulating plains, or because a plane admits of being bent into the shape of a cylindrical or conical surface. I think the ladies who are getting up their Planes and Solids at St. George's Hall would be of a different opinion from Mr. Lewes in this matter, and with good reason on their side.

Mr. Lewes, reiterating a statement contained in his previous letter, goes out of his way to affirm that he "uniformly speaks of Space and Time as forms of Intuition in his pages of exposition" of Kant's doctrine in his "History of Philosophy." Were the fact so, it would not in any material degree excuse the inaccuracy of subsequently styling them "forms of Thought;" and, moreover, the real point at issue is not Mr. Lewes's general accuracy or inaccuracy, but whether a mode of speech which he, along with others, employs, is right in itself and ought to be persisted in.

However, as Mr. Lewes has thought fit to put in a sort of plea in mitigation of former wrong-doing, I have taken the trouble of looking through his *exposition* and *criticism* of Kant in his History (ed. 1867), and in no single instance have I come upon the phrase *forms of intuition* applied to Space and Time, either in the one or the other; although he states he has *uniformly spoken of them* as such in the former. I have marked the word *intuitions* as occurring once, and *forms of sensibility* several times, but *forms of intuition* never. If *form of sensibility* is as good to use as *form of intuition*, *form of understanding* ought to be as good

as *form of thought*; but Mr. Lewes owns that the former is indefensible, whilst he avers that the latter is correct. If Mr. Lewes has ever called Space and Time *forms of intuition* in the History, it will be easy for him to set me right by quoting the passage where the phrase occurs, although that circumstance would not in any degree better his own position, and still less excuse the assertion of his *uniform* use of the term.

If Mr. Lewes cannot quote correctly from his own writings, it will surprise nobody that he misquotes the language of an opponent. He repeats, "Intuition without thought is mere sensuous impression," and adds, "Mr. Sylvester demurs to this," "My words are" (NATURE, Jan. 13, 1870): "To such a misuse of the word *energy* it would be little to the point to urge that *force without energy* is mere *potential tendency*. It is just as little to the point in the matter at issue for Mr. Lewes to inform the readers of NATURE that *intuition without thought* is mere *sensuous impression*." So that, according to Mr. Lewes, to say that a proposition is *little to the point* is *demurring to its truth*.

I should not hesitate to say if some amiable youth wished to entertain his partner in a quadrille with agreeable conversation, that it would be *little to the point*, according to the German proverb, to regale her with such information as how

"Long are the days of summer-tide,
And tall the towers of Strasburg's fane,"

but should be surprised to have it imputed to me on that account that I demurred to the proposition of the length of the days in summer, or the height of Strasburg's towers.

In another passage, Mr. Lewes gives me credit for "saying correctly that Intuition and Thought are not convertible terms"—a platitude I never dreamed of giving utterance to; but that I am "incorrect in assuming that they differ as potential and actual"—words which, or the like of which, in any sort or sense, never flowed from my pen. Surely this is not fair controversy, to misquote the words and allegations of an opponent. It seems to me too much like fighting with poisoned weapons. I decline to continue the contest on such terms; and, passing over Mr. Lewes's very odd statement about *species* and *genus* with reference to Intuition and Thought, shall conclude with expressing my surprise at his and Mr. G. C. Robertson's confident assumption that Kant uses in the title of his book *pure reason* in a far wider sense than in the body of his work, simply because to arrive at the *Pure Reason* he has to *go through* the Critick of the Sensibility and of the Understanding. If in a history of the Reign of Queen Victoria the author should find it expedient to go back to the times of the Norman and Saxon conquests, would it be right to infer therefrom that he used in his title-page the name Victoria in a generalised sense, to include not only her most Gracious Majesty, but also the Tanner's daughter and Princess Rowena?

Perhaps by this time many of the Naturalistic readers of the journal who regard the human intelligence as forming no part of the scheme of Nature, wish Space at the bottom of the sea; but the more the subject is canvassed, and the greater the number of English authorities brought forward to back up Mr. Lewes in wresting the words of Kant from their proper scientific signification, the higher meed of praise seems to me to accrue to Dr. Ingleby for stemming the tide of depravation, and banishing, as I feel confident this discussion will have the effect of doing, from the realm of English would-be philosophy, such a loose and incautious way of talking as that of giving to Space and Time the designation which the Master has appropriated to the categories of his system, and to them alone. J. J. SYLVESTER

P.S.—I should be doing injustice to the very sincere sentiments of respect I entertain for Mr. Lewes's varied and brilliant attainments (which constitute him a kind of link between the material and spiritual sides of Nature), and of gratitude for the pleasure the perusal of his "History of Philosophy" has afforded me, were I to part company with him without disclaiming all acrimony of feeling, if perchance any too strident tones should have seemed to mingle with my enforced reply. In naming him in the original offending footnote (the fountain of these tears), my purpose was simply to emphasise the necessity of protesting against what seemed to me an unsound form of words, *apropos* of Kant, which went on receiving countenance from such and so eminent writers as himself and the others named; and I should be false to my own instincts did I not at heart admire the courageous spirit with which, almost unaided and alone (like a good knight of old), he has done his best to defend his position and maintain his ground against all opponents.

J. J. S.