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To cite this article: John Blackwall Esq. F.L.S. (1831) XXIII. An examination of M. Virey's observations on Aëronautic spiders, published in the Bulletin des Sciences Naturelles, Philosophical Magazine Series 2, 10:57, 180-187, DOI: [10.1080/14786443108675515](https://doi.org/10.1080/14786443108675515)

To link to this article: <http://dx.doi.org/10.1080/14786443108675515>

Published online: 14 Jul 2009.

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the velocity of the particles such as would carry them in void space from one extremity of the planetary regions to another in a second of time, the ratio of their magnitude and mean distance will approach that of the stars which form a nebula; whilst the mean space described in the medium by a particle before the direction of its motion is altered by impingeing against others, may be indefinitely smaller than any conceivable magnitude. However extreme the rapidity of this action may be considered, it is yet finite, and cannot therefore be reckoned irrational or contrary to the institutions of nature. It is the true primitive standard to which all velocities [upon the hypothesis] may be compared, as from it every other motion in the universe is derived.

Professor Œrsted, as far back as 1813, in his work on the Identity of Electricity and Chemical Affinity, deduced, from an extended series of experiments and general view of science, "that all effects are produced by a fundamental power operating in different forms of action." It is remarkable that the principle we have been explaining is in perfect unison with the sentiments of this eminent philosopher. A medium is supposed to envelope the universe, every particle of which is a minute reservoir of power, which is conserved in the rectilinear and rotatory motion with which it is endowed. These motions, by the introduction of the gross particles of different substances, are alternately transformed into each other; and thus the primordial power of Œrsted's doctrine by the varied structure of the particles of matter "operates in different forms of action," and is everywhere developed in the diversified series of natural changes.

St. John's Hill, Edinburgh, April 14, 1831.

XXIII. *An Examination of M. Virey's Observations on Aëronautic Spiders, published in the Bulletin des Sciences Naturelles.* By JOHN BLACKWALL, Esq. F.L.S.*

THE *Bulletin des Sciences Naturelles* for July 1829, p. 131—134, contains a notice, from the pen of M. Virey, of my memoir on Aëronautic Spiders, printed in the Transactions of the Linnæan Society, vol. xv. part ii.; and it is particularly deserving of attention, that the author, by an extraordinary misapprehension, originating apparently in an imperfect acquaintance with the English language, not only distorts the facts I have promulgated and perverts the arguments founded upon them, but even attributes to me opinions the

* Read before the Linnæan Society, May 4, 1830; and communicated by the Author.

very reverse of those advanced in my paper. Of the great injustice done to me in that article I have every reason to complain, were I disposed to give way to personal feelings; my sole object, however, in animadverting upon it is the promotion of science, being impressed with the belief that the principal errors into which M. Virey has fallen, if suffered to remain unnoticed, must tend to retard its progress. That I should be thus forced into collision with a distinguished fellow-labourer in the field of natural history, I sincerely regret. As it would be tedious to enter into a minute consideration of the various misconceptions on the part of M. Virey, adverted to above, I shall particularize such only as directly militate against the theory I have endeavoured to establish relative to the ascent of aëronautic spiders in the atmosphere.

In narrating the fact observed by me in October 1826,—namely, that the tissues usually denominated gossamer-webs are formed at the surface of the earth, and are afterwards raised into the atmosphere by ascending currents occasioned by the rarefaction of the air contiguous to the ground when heated by the sun in serene weather,—M. Virey states that I witnessed the ascent of webs more than a hundred feet long, (“des traînées de plus de cent pieds de long de ces toiles,”) and that I investigated the process employed by the *spiders* in fabricating “ces subtils *calicots*,” as he facetiously terms them, in reference to the manufacturing district where my researches were made, “pour s’élever dans l’atmosphère et franchir au loin les espaces.” Now, so far from ascribing the construction of these webs to spiders, in the instance referred to, I attribute their formation to the adhesion of the slender filaments of which they are composed on being brought into contact by the mechanical action of gentle airs. That the filaments are produced by spiders I am perfectly well aware, but I have no where asserted that these animals convert them into the webs termed gossamer; neither have I affirmed that they employ such webs to effect their aërial excursions, as M. Virey intimates: on the contrary, the accomplishment of their purpose is shown by me to depend upon ascending currents of rarefied air impingeing against the fine lines emitted from their spinners, which generally remain distinct throughout their entire length. The occurrence of spiders upon gossamer-webs I represent to be entirely accidental; and that the former are instrumental in promoting the ascent of the latter I positively deny: yet M. Virey professes to say on my authority, that the animals aspire “à les faire envoler, en les fixant légèrement à l’extrémité d’un corps en pointe.” My account of the height to which I have seen these webs raised
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in the air appears to have been mistaken by Mr. Virey for a statement of their length. This is the more surprising, as I have plainly indicated that they rarely exceed a *few feet* in longitudinal extent.

I forbear to offer any comments on the inaccuracy and confusion so conspicuous in M. Virey's survey of my opinions concerning the insufficiency of winds, evaporation, and electricity to occasion extensive and simultaneous ascents of gossamer-webs and spiders, and shall dismiss his review of my memoir with a few strictures upon the following passage, which clearly proves that not even my most careful expositions of the inferences I have deduced from exact experiments have escaped perversion. "Ces animaux" (spiders) "peuvent s'élever avec leurs tissus, et tantôt retomber selon le degré de gravité qui domine et les fait alors précipiter sur la terre. D'ailleurs, certaines particules d'air raréfiée ne peuvent-elles pas se trouver comme renfermées dans le tissu gazeux de ces araignées et prendre à la manière des ballons un mouvement ascendant? L'auteur s'attache à développer l'idée de cette possibilité; il montre que des araignées peuvent expulser des fils à une certaine distance et les attacher par la matière gomme-visqueuse dont ils sont formés, à un lieu plus ou moins éloigné." In experimenting with a view to demonstrate the fact, that spiders do not raise themselves into the atmosphere by the exercise of any physical power with which they are endowed, I repeatedly separated from the spinners of individuals, when they were ascending, the fine lines which contributed to give them buoyancy. The result was conclusive. The animals were quickly precipitated to the ground, and the detached lines rose with increased velocity in consequence of being acted upon by a diminished gravitating force. How completely the true nature and object of this investigation have been misunderstood by M. Virey appears from the first sentence of the extract just given from his paper, upon which it would be superfluous to offer any comment. After inquiring, in the next place, whether the upward direction taken by gossamer-webs may not be ascribed to the rarefied air inclosed in them, M. Virey adds, that I have endeavoured to develop this idea; a piece of information which perplexes me not a little, as I am wholly unconscious of having made any such attempt. But the most remarkable circumstance remains to be considered. It is assumed by M. Virey, in opposition to the fact which I have uniformly and strenuously maintained,—a fact, let it be remembered, confirmed by direct experiments carefully conducted and accurately detailed,—that I have rendered it manifest that some spiders are capable of propelling their
lines

lines to a distance. On what grounds this assumption, so contrary to the whole tenour of my experience, is founded, I have yet to learn.

I shall now proceed to examine the observations on the ascent of spiders communicated by M. Virey to the French Institute in June 1829, and published in the *Bulletin des Sciences Naturelles* for October in the same year, p. 130-134; availing myself of the opportunity thus afforded, to introduce several interesting particulars which have recently come to my knowledge. Having already rendered it sufficiently apparent that M. Virey does not comprehend my views relating to the ascent of gossamer-webs and spiders, any notice of the allusion made to them in the present article is unnecessary; I pass on therefore to the author's remarks on the power which he supposes spiders to possess of darting out their lines to a distance. "Je m'étois assuré déjà," he writes, "que jusqu'à la distance de deux pieds environ, une araignée savait lancer prestement un fil vers un point quelconque, l'y attacher, et s'enfuir soudain sur cette corde. Il faut que dans le nombre de leurs filières elles aient des tubes éjaculateurs, puisqu'elles lancent ces fils indépendamment d'autres sur lesquels elles s'avancent, et qu'elles émettent en même temps." This opinion, which has been entertained by so many eminent naturalists, may be refuted without difficulty. If spiders be placed on a twig fixed upright in a glazed earthen vessel with perpendicular sides, containing a sufficient quantity of water completely to encompass its base, they will be found totally incapable of escaping from their place of confinement in a still atmosphere; but when exposed to a current of air, or when blown upon with the breath, the case is otherwise, as many species may then be perceived to emit from their spinners a little of their viscous secretion, which being carried out in a line by the agitated air, becomes attached to some object in the vicinity, and affords them the means of regaining their liberty.

These facts were first established by me in 1826, in the manner here described; and in the summers of 1828 and 1829 I repeated the experiment with about thirty distinct species of spiders under every variety of circumstances which appeared likely to influence the result. My former conclusions, however, were most unequivocally confirmed, and I am confident in asserting that these animals have not the power of darting their lines even through the space of half an inch.

It is certain that spiders can open and close the orifices of their spinners at pleasure, and can allow their liquid gum to
escape

escape so as to form one line or more of greater or less tenuity and strength, as may suit their convenience. When suspended by a vertical thread, for example, they frequently suffer another thread to be carried out horizontally, if blown upon in that direction; but that they have tubes so organized as to enable them forcibly to project their lines to a distance, as M. Virey conjectures, is quite inadmissible.

The curious fact, that all spiders possessing an apparatus for spinning are not endowed with the instinct to let out their lines when placed on a twig insulated by water and exposed to a current of air, I have proved by numerous experiments: and as this is the case with some of the more common species, as *Aranea domestica* and *Chubiona atrox*, I take this opportunity of calling the attention of investigators to the circumstance, which if unnoticed might occasion them some disappointment*.

M. Virey, who has experimented with various species of spiders, and especially with the young of *Epeira diadema*, informs us that "on doit faire ces observations dans une chambre close, où l'air très calme ne puisse recevoir aucune agitation." Being unable to detect the presence of any lines which he considered could contribute to the ascent of these animals, he concludes that the phænomenon must take place without their concurrence. "Réfléchissant," he remarks, "aux moyens par lesquels ces insectes gravissent dans l'air, une seule chose m'a paru la plus vraisemblable, c'est qu'à l'aide des huit pattes que l'animal peut faire vibrer avec agilité, il nage dans l'air. On conçoit que ces membres rapprochés, ramant quatre à quatre simultanément de chaque côté, frappent l'air comme des ailes, et peuvent fort bien enlever cet insecte d'ailleurs si léger. Ce procédé paraît le seul possible dans ce cas. D'ailleurs l'extrême rapidité, ou l'agilité incroyable de ces pattes en trépidation, comme la vibration des ailes chez les oiseaux ou les insectes diptères qui planent dans l'air, font qu'on ne peut pas toujours bien distinguer leur mouvement." And again, "Il est donc plus probable que ces petites araignées *volent avec leurs pattes*, que de supposer des effets électriques, ou l'agitation de l'air, ce que nous avons démontré faux par l'observation directe." In this bold but fanciful conjecture M. Virey has been anticipated by Dr. Lister, who, in treating upon his

* It would appear, however, from a notice respecting the habits of *Aranea domestica* published in the Zoological Journal, vol. i. p. 283, that, under certain circumstances, the instinct in question is manifested by that species of Spider.—EDIT.

"araneus subfuscus, minutissimis oculis è violâ purpurascens, tardipes, et gressu et figurâ cancro marino non adeò dissimilis," observes, "Certè egregius funambulus est, et mirificè florum ejaculatione delectatur: neque solum in aëre, uti superiores, vehitur; sed ipse etiam ascensum velificationémque molitur, pedibus sc. arctiùs ad se invicem applicitis sese quodammodo librat, cursum promovet regitque nihilo seciùs quàm si illi essent à naturâ concessæ alæ vel remorum ordines*."

The extreme liability of air to be put in motion, and the exceeding levity of the lines of very small spiders, are facts which M. Virey, in the prosecution of his researches, has not attended to with sufficient minuteness. This is evinced by his supposing that in a close room the air could not be agitated to such a degree as to affect the results of his experiments. Have then, I would inquire, the temperature of the body, the motion of the limbs, and the act of respiration been altogether overlooked as causes of disturbance? It would appear that they have; no allusion whatever being made to them, in this respect, by M. Virey, who even recommends that the hand be passed before young spiders, when afloat, in order to determine whether they are supported by a line or not; a proceeding calculated to mislead the inquirer, if executed with haste, as it generally must be, by disturbing the air and occasioning a deflection of the line, which consequently might escape his observation.

These difficulties are entirely avoided by my mode of experimenting. The vessels containing the twigs on which the spiders are placed being generally locked up in a book-case, or put under bell-glasses, the tranquillity of the atmosphere immediately surrounding the prisoners is ensured without detriment to their physical powers. If then they are endowed with the capability of flying, what is to prevent them from exercising it when thus stimulated to exertion? Every facility is afforded them which they can enjoy when at liberty, except that of air in motion; yet they are never found to escape from the twigs by flight, notwithstanding their best endeavours to quit them are persisted in pertinaciously. I have tried this experiment with several hundred spiders of nearly thirty distinct species, including the *Epeira Diadema* in various stages of growth, and uniformly with the same success. In a calm atmosphere they are quite incapable of regaining their liberty, but if placed in a current of air, many species let out their lines with the utmost facility, along which, when attached to any object, they pass in security.

* De Araneis, p. 85.

To obviate such objections as might be urged against the use of glass or glazed earthen vessels by the advocates of the electrical hypothesis of the ascent of spiders, I have in many instances employed polished vessels of silver, tin, iron, &c. without producing the least difference in the results. I am not ignorant that these results have been represented as absolutely subversive of my views. Spiders, it is argued, are *compelled* to dart out their threads by the electrical excitation occasioned by currents of air, the phenomenon being considered inexplicable on any other principle whatever; but it is not found expedient to assign a reason for this sudden conversion of the power ascribed to these animals of propelling their lines to a distance, into a merely involuntary action entirely dependent upon air in motion. The fallacy of the foregoing supposition may be proved by placing spiders of the same species on copper rods insulated by water, and subjecting them to the influence of a stream of air so slight as scarcely to be perceptible. Under such circumstances, any electricity induced in the spiders by the feeble current must be carried away as speedily as it is excited, by so excellent a conductor as copper; nevertheless, the animals can emit or retain their liquid gum at pleasure, as lines may frequently be seen streaming from the papillæ of some individuals; while others, on the same rod, do not let out any, and may be instantly diverted from their purpose, should they make the attempt, by the most trifling causes of disturbance. It is manifest, therefore, that spiders do not fly, in the strict sense of the word; and that they are not raised into the atmosphere by the agency of electricity is equally evident; in short, not a doubt can be entertained by those whose minds are open to conviction, that their ascents are effected by means of upward currents of air impinging against the lines which proceed from their papillæ.

The two species of aëronautic spiders, whose proceedings are detailed in the Linnean Society's Transactions, vol. xv. part ii., I have ascertained to be *Thomisus cristatus* and *Lycosa saccata*, both of which are distinctly mentioned as aëronautic spiders by Dr. Lister*. The first I have described as having two pair of eyes situated in the anterior part of the head and arranged thus, $\cdot \cdot \cdot$; the second as having three pair in front whose arrangement is thus represented by dots $\therefore \therefore$. The species noticed in my paper as remarkable for the skill it displayed in spinning its way up the sides of a phial in which it was confined, and for having existed seventy-five days without food or moisture, was *T. cristatus*; *L. saccata* being neither so

* De Araneis, p. 79, 80, 85.

expert in climbing, nor so tenacious of life under similar circumstances. I may remark, that in experiments instituted to decide how long spiders can live without food, the influence of season should not be disregarded. In winter, unless a high temperature be maintained by artificial means, the vital functions are performed with much less energy than they are in summer: of course the natural demand for sustenance, if it do not cease altogether, is greatly diminished in the former period, and the animals suffer little comparatively from abstinence.

Another spider of a diminutive size, frequently observed to take aerial excursions, is the *Drassus ater* of Latreille, which appears to be identical with the *Aranea obtectrix* of Bechstein.

Aëronautic spiders, properly so called, or those species which *instinctively* employ their lines to sail in the atmosphere, will probably be found almost exclusively among such as are active during the day and decidedly erratic. Numerous facts tend to corroborate this idea, the correctness or inaccuracy of which can only be determined by more extended observations.

XXIV. On Mengite, a new Species of Mineral; on the Characters of Aeschenite; on Sarcolite, as distinct from Analcime and Gmelinite; with other Mineralogical Notices. By H. J. BROOKE, Esq. F.R.S. L.S. & G.S.

Ilmenite.

A MINERAL under this name is said to have been described in 1821 in Kastner's *Archiv.*, &c. No. 1. by Prof. Kupffer of Kasan. It was discovered by Mr. Mengé, near Lake Ilmen in Siberia, accompanied occasionally by a titanious iron-ore in modified rhomboids, of which a description and figure, but without measurements, were given by Mr. Levy in the *Phil. Mag. and Annals*, N.S. vol. i. p. 26.

Probably from having seen only the iron-ore, Prof. Rose of Berlin has stated that this was the ilmenite of Kupffer. The ilmenite is, however, a distinct substance, having for its primary form a *right rhombic prism* of $136^{\circ} 30'$, the terminal edge being to the lateral edge very nearly as 17 to 11. The colour is a more intense black than the rhomboids of titanious iron, and the surfaces of some of the crystals are perfect and brilliant. I have not observed any cleavage; the fracture is uneven to conchoidal with a vitreous lustre. Spec. grav. 5.43. Scratches glass slightly. The matrix is cleavelandite.

The crystals I have examined are generally small, length-