By the side of this first assemblage of facts, of which the meaning seems to me not doubtful, I find another of at least equal importance—that of meteoric rocks evidently eruptive.

The meteoric iron recently discovered in the cordillera of Deesa, in Chili, having been submitted by me to a careful analysis, both chemical and mineralogical, appeared to me clearly to be formed from the mixture of two meteoric rocks, known, each of them, by masses of which they are entirely constituted. The one, stony and black, fell at Sétif, Algeria (June 9, 1867); the other, metallic, constitutes the mass of iron found in 1828 at Caille, in the south of France. Besides this, the metallic portion of the iron of Deesa, in which the black angular fragments are encrusted, has manifestly preserved the character assumed by the iron of Caille when it is subjected to fusion, so that the mode of formation of the Chilian mass cannot be considered doubtful. We must believe that on a globe, large enough to have been the seat of considerable pressure, masses of iron from Caille, still melted, were injected into superposed layers of Sétif rock so as to give birth to dykes, identical, except in their mineralogical nature, with those which the crust of the earth everywhere presents to our view.

These two orders of facts, which seem to me indisputable, being admitted, there remains to explain how fragments of polygenic conglomerates, or of dykes, can wander through space, and here only it is that the hypothetic part of my work begins.

From what precedes the meteorites in question are, by definition, planetary fragments. It remains to learn how the rupture of the planet whence they come can have taken place. On this it is evidently impossible to argue with any certainty.

Nevertheless, it appears to me that several considerations may greatly facilitate a choice among the different explanations which present themselves to the mind.

In the first place the *unity of composition* of the solar system, mentioned by Mr. Maskelyne, is evident.

Secondly, it is manifest that in the same system there exists a perfect *unity of geological phenomena*.

Lastly, but this, perhaps, has less weight, it appears to me that we should have recourse to accidental causes to explain natural phenomena only when every other means is forbidden.

This said, I observe that without making any other hypothesis than that of Laplace, we arrive at the conclusion that the stars tend of themselves to become broken. The earth is cracked in all directions; these fissures, designated as *faults*, are known to everyone. Little by little, as they form, they become reunited by the injection of an internal melted cement. But if the supply of this cement failed, the molecular operation which has opened the faults would still continue its action to enlarge them; we observe this in the moon, which, far more advanced in refrigeration, manifests by its fissures a phenomenon hitherto unknown in our earth. Evidently if we suppose to have been formed at the same time as the moon, a much smaller globe, that globe will have arrived actually at a state of cold far more advanced than that of the moon; and the fissures, excessively multiplied, and increased in depth and in width, may have finished by reducing the globe into separate fragments.

We have no positive proofs that such events have really happened, but is it not a very simple hypothesis to admit that meteorites, which bear so evidently the impress of a detritic character, may have had such an origin?

It is very probable that once parted from one another, the fragments are scattered along the orbit, and it is evident that they will tend progressively to approach the central star, so as to finish by falling on its surface under the form of meteorites.

Now, whether these fragments have been sorted or whether they have not, whether this sorting, if it exists, be or be not in accordance with that which the facts of observation have seemed to point out to me; I consider the question as entirely secondary as regards the general theory, and I request permission, in order to keep within the limits of the present discussion, to lay it absolutely aside for the present. I will simply repeat, in concluding this note, already somewhat long, that positive facts alone have served as the basis of my theory, and that the different circumstances on which my opponent has so learnedly insisted, possess for me but a secondary importance.

At the same time, I sincerely congratulate myself in the fact that my work has had the good fortune to fix the attention of a scientific observer so well placed as Mr. Maskelyne for submitting the mineralogical and lithological part of it to a severe verification.

DR. STANISLAS MEUNIER, Aide Naturaliste au Muséum

23, rue de Vaugirard, à Paris

Monographs of M. Michel Chasles

PAR une lettre inséréee dans le No. 36 de NATURE, page 190, M. C. Ingleby fait appel aux lecteurs de votre Revue pour obtenir quelques renseignements au sujet de "l'Aperçu historique" de M. Chasles, imprimé à Bruxelles en 1837. Le travail, qui porte pour titre exact: "Aperçu historique sur l'origine et le développement des méthodes en géométrie, particulièrement de celles qui se rapportent a la géometrie moderne," a été publié par l'Académie royale des sciences de Belgique dans le tome xi. de ses "Mémoires couronnés et des savants étrangers" (in 4to.), et il est très-difficile aujourd'hui de s'en procurer des exemplaires. Toutefois, M. Ingleby pourra s'adresser, pour consulter ce mémoire, à la Société royale de Londres, qui doit certainement le posséder dans sa Bibliothéque. Voici d'ailleurs la liste des établissements scientifiques de Londres qui ont reçu cet ouvrage a l'époque de sa publication: Société royale, Société astronomique, Société royale de littérature, et Société linnéenne.

J'espère que ces details pourront être utiles à votre honorable correspondant.

Bruxelles, le 8 Juillet A. LANCASTER,
Attaché au Secrétariat de l'Académie royale des Sciences de Belgique

IN reply to Dr. Ingleby's note I may state that many papers by M. Chasles on various subjects in the history of Mathematics, are to be found in the volumes of the *Comptes Rendus* for 1837, onwards. His "Aperçu Historique" &c., originally appeared as a special volume of the Transactions of the Brussels Academy, but was sold as an independent work. It appeared in quarto, and was published in 1837. Like his "Traité de Géométrie Supérieure," it is very rare, and fetches an enormous price. Mr. Quaritch is, perhaps, the most likely bookseller in London to be able to procure it. The German translation by Sohncke is comparatively cheap, and may be readily obtained through Messrs. Williams and Norgate.

Torquay, July 9 G. E. DAY

The Specific Heat of Mixtures of Alcohol and Water

IN the report of the papers read at the Academy of Sciences, Paris, June 13, which appears in NATURE for June 30, it is stated that MM. Jamin and Amaury presented a note on the above subject, in which they point out, apparently as if it were something new, that the specific heat of some of these mixtures rises even above that of water.

Now, more than two years ago, March 26, 1868, we communicated a paper to the Royal Society giving the specific heat of various mixtures of alcohol and water, and drawing special attention to the remarkable fact that the specific heat of these mixtures is not only above the calculated mean specific heat, but that in all those of less strength than 36 per cent. of alcohol, it is higher than the specific heat of water itself. A knowledge of this fact should therefore be old by this time.

An abstract of our paper is printed in Proc. R. S., vol. xvi., p. 337. Subsequently we examined this and various other properties of similar mixtures more in detail, and communicated our results to the Royal Society in a second paper, an abstract of which is printed in Proc. R.S., vol. xvii., p. 333, and the paper in full in Phil. Trans. for 1869, Part II., p. 591.

The insertion of the above in the next number of your valuable journal will greatly oblige A. DUPRE & F. T. M. PAGE

Westminster Hospital, July 2

Geographical Prizes

HAVING been chiefly instrumental in causing prize medals to be offered by the Geographical Society for competition among the chief public schools, I do not like Mr. Wilson's letter in your last number to pass without comment.

Geography may be, to use his words, a subordinate branch of education, but I maintain that it is so only in the sense that it underlies a large part of liberal knowledge. It underlies the study of history. For example, I do not see how a boy could thoroughly understand Bible history without having acquired a very vivid conception of the geography of Palestine, and the same is true for all other histories, ancient and modern. It follows, as a matter of fact, that geography is incidentally taught to a considerable extent in schools, and I am sorry to say it is sometimes very ill-taught, as we learn from the reports of our examiners, but through some omission, not easily to be explained, if it be not the effect of a mere accident, geographical proficiency has never hitherto been adequately encouraged. Consequently, the Geographical Society has thought it right to step in to supply the needful encouragement. There is another good reason for the interference of the Society, in the fact that facilities of travel have rendered our interests much more cosmopolitan than formerly, while the public schools of the old-established type, have made no corresponding change in their curriculum. Mere youths now-a-days have exhausted the grand tour of two generations back, and a year or two of early manhood is often spent in America, Australia, and India, while books of travel load our library tables. It seems monstrous that a so-called liberal education should not qualify men to journey themselves, or to read the journeys of others, in an intelligent manner.

Mr. Wilson remarks, and his remark deserves respect, that the masters of Rugby were almost unanimous in rejecting the invitation of the Geographical Society, but I can fairly retort that other scholars no less practised in education and no less competent to decide, pronounced our system of prizes to be a valuable and much-needed institution.

It would be easy to write at great length in support of what we have done, and I might perhaps be expected to say something on the respective objects of the political and physical geography prizes, but I do not wish to provoke a discussion in your pages, because I am on the point of going abroad and should be unable to take further part in it. FRANCIS GALTON

"Kinetic" and "Transmutation"

1. WHEN, in 1864, I wrote for the *Reader* the history of the Baconian Philosophy of Heat, I found in use, in connection with the subject, the term "dynamical theory of heat," in English, which was employed as an equivalent for the expression "mechanische Wärmetheorie," current in German. The word "dynamical," already so vague from frequent abuse, corresponded but little, when used in its proper meaning, to the real intent of the theory in question; and the same remark applies, with at least equal force, to the word "mechanisch," even wider in its scope and as often misused. I was thus led to adopt the word "Kinetic," to supersede the above; and that in preference to the current word, "cinematic," which, in conjunction with "theory," would imply a tautology.

I am glad to see that Sir W. Thomson and Professor Tait, in their treatises on Natural Philosophy and on Heat, as well as in some remarkable papers on Atoms which have appeared in NATURE, frequently make use of the same word, "Kinetic," in connection with the theory of heat and of gases, as also in conjunction with "energy." Instead of the expression, "actual energy," originally introduced, I believe, by Mr. Rankine, Sir W. Thomson and Mr. Tait employ the term "Kinetic energy;" and from various motives, linguistic as well as strictly scientific, I venture to think that the original wording of Mr. Rankine in the case of "potential energy," should be likewise superseded, viz., by "dynamic energy."

2. In the *Philosophical Magazine*, I have been rated, indirectly, by Professor Challis, (for no mention is made of my name in connection with the subject), for having applied the word "transmutation" to rays, without recalling the fact of his having done so before me. I considered the expression "transmutation of rays" as the abbreviated and thoroughly English rendering of the words, "change of the refrangibility of rays, or light," used by Professor Stokes; and as such, requiring no authority but the precedent furnished by the existence of the analogous expression of "transmutation of matter." If, however, an authority had to be cited, it would have been Euler, in whose "Nova theoria lucis et colorum" (Opusc. var. argum.) the following passage occurs:—"Cum igitur a corporibus rubris radii tantum rubri, et a violaceis violacei ad nos pertingant, etiamsi radii albi in ea incidissent, manifestum est istam transmutationem a sola reflectione proficisci non posse."

As I have returned to this subject, I may be permitted to express my astonishment that Professor Challis, who thought it due to him that his name should be mentioned for being the author of the expression "transmutation of rays," should have on his part omitted, in speaking of the transmutation of Herschelic rays into Newtonic, a reference to my own share in the *res gestæ*. When I see the same thing being done in so widely circulated a treatise as that of Mr. Brooke on Natural Philosophy, and in one intended for even more popular reading, reproducing the teaching of the Polytechnic, I might think of entering a protest, if experience had not convinced me of its uselessness. C. K. AKIN

Parturition of the Kangaroo

I BEG leave to call your attention to certain comments in your issue of the 23rd of June on the proceedings of the last meeting of the Royal Geological and Zoological Societies of Ireland. It is usual when parenthetical observations are made in any journal without the customary affix "Ed." to ascribe them to the printer's devil. Now, your devil, in commenting on an *imperfect* report of your Dublin correspondent, would lead your readers erroneously to infer that I had adopted the ideas which he has been pleased to call "absolute nonsense," and takes me to task for saying "that the actual passage of the fœtal kangaroo from the uterus to the pouch was not yet proved;" he himself stating that my remarks were "in contradiction to the facts observed by the late Earl of Derby's father or by the present Professor Owen." Now, a critic calling in question the words of others should be careful of his own. No facts on the subject were observed by the late Earl of Derby's father, and Professor Owen, after elaborate arrangements for the observation, states that "as parturition took place in the night, the mode of transmission to the pouch was not observed." (Phil. Trans. for 1834, p. 344.) There have been four observers in this matter especially worthy of being noticed:—(1) the keeper at the Zoological Gardens, Knowsley, who, according to Lord Derby's statement, saw the young kangaroo born, and that it was placed in the pouch by the paws of the mother (Proceedings of Zoological Society for 1833, p. 132); (2) Professor Owen, as referred to above; (3) Mr. E. G. Hill, who, at thirty yards' distance, saw the kangaroo with her mouth take up what he thought was a stone, open the pouch with her paws, and place it in the marsupium, and that he shot the animal and found a newly-born fœtus in the pouch (Proceedings of Zoological Society for 1867, p. 476); (4) M. Jules Verreaux, who is mentioned by M. E. Alix as having seen the kangaroo remove the fœtus from the vulva with her mouth, and place it in the pouch (Annals of Natural History for 1866, p. 316). These all differ as to the actual facts observed, and would seem sufficient to justify me in the statement I had made. That Professor Owen does not consider the question settled, may be inferred from his concluding observations on the subject, "whether the circumstance of the parturition is constant, viz, the dropping on the ground, or whether the fœtus may occasionally be received by the mouth from the vulva, I am disposed to regard as a matter for further observation; but the main fact of the conveyance of the fœtus to the pouch by means of the mouth may now be held as the more probable (at least the more usual, if not the constant) way in the genus Macropus" (Proceedings of Zoological Society for 1866, page 382). I refrain from any comments, but I thought it right to remonstrate against statements which I felt were injurious to me, to the Society to which I have the honour to belong, and to the advancement of science. JOHN BARKER, M.D.

Dublin, July 1

The Extinction of Stars

IF you will kindly permit an amateur to rush in where astronomers fear to tread, I shall be glad to offer a few remarks on the above subject.

The progress of science enables us to trace, with a probability almost amounting to certainty, the career of a star from its birth; from the most diffused condition of its parent nebula; through the stage of primary agglomeration when it shines as our sun; through the process of cooling into a dim and cloudy spheroid, such as Jupiter or our earth; until cold rules supreme, and the once glowing orb rolls on, barren as our moon.

But when we have reached this stage, we have by no means done with the star. It must continue on its course, and, though in obscurity, it must retain its momentum and its attractive force. Our sun will thus one day travel in darkness, attended by a cohort of funereal planets, and perpetual night will reign over the solar system. This result appears to be but a question of time, and we are, therefore, led to the consideration that many systems must, in all probability, be already extinct, and wandering unnoticed. But as extinction is a gradual process, there will be multitudes of stars in various stages of dimness,