June 25, 1838.

SIR WM. R. HAMILTON, A.M., President, in the Chair.

Dr. Apjohn made some remarks upon the subject of the Specific Heats of Gases.

The author stated that he had received, some months since, a memoir on the subject of the specific heats of gases, by M. Suerman, an eminent Dutch philosopher; upon examining which, he was not a little surprised to find that the method of the author was identically the same with that which he had himself employed, in a paper on the same subject, which has appeared in the last volume of the Transactions of the Academy. The following passage of M. Suerman's preface, however, completely removed his apprehensions of having been anticipated—"Tandem opus aggressus, et occupatus in idonea paranda supellectili diarium accepi Anglicum, quo in collegio, quod Dublini habetur, Chemiæ Professoris Apjohn continebatur disquisitio, ex eodem illa principio fluidorum elasticorum calorem specificum derivans. Primum,—quid sileam?—animo despondebam quum novitatis colorem quæ mihi præcipue arridebat, de meo evanescere viderem proposito."

This passage, he stated, he had been long anxious to bring before the Academy, lest, when Suerman's Thesis came to be noticed in the British journals, any Member of the Academy should suppose that he had borrowed his method from the learned Dutchman, and had done so without acknowledgment.

Dr. Apjohn then proceeded to remark upon some points, in reference to which he considered himself as having been misapprehended by M. Suerman. Thus he is represented in the following passage, as adopting an erroneous method of estimating the caloric of elasticity of aqueous vapour. "In computando vero formulam quam a claro Gay-

Lussac propositam vidimus, errorem effugere studuimus quem miror, etiam a viris celeberrimis commissum esse. Ponitur enim et in hac formula, et psychrometrica quam Dr. Apjohn proposuit, *l*, sive caloricum vaporis latens, terminus constans. At vero et theoria assumit, et experientia, quatenus adhuc innotuit, docet, non esse caloris quantitatem quam durante evaporatione vapor latens reddit, quacumque temperatura constantem, sed quantitatem calorici, quæ determinato vaporis pondere continetur, quod in maximo densitatis habetur."

Dr. Apjohn could not admit the applicability of this extract to himself, and in his vindication referred to the following passage, which occurs in his first paper on the Dew-point:—" It must of course be admitted, that the caloric of elasticity of vapour varies with the temperature, and that it is represented by 1129 only at the temperature of 50°, a point chosen by me as being nearly the mean temperature of Dublin. In strictness, the number employed should be $967 + 212 - t'$, but it would be easy to show that the uniform use of 1129 cannot give rise to any material error." The latter part of this passage, he observed, was intended only to apply to the meteorological use of his formula, and not at all to it when employed in the investigation of the question of gaseous specific heat. And had M. Suerman repeated the calculation of his experiments he would have found that he, Dr. A., had, while engaged with the specific heats of the gases, invariably employed the rigorous method of estimating the caloric of elasticity of vapour.

Dr. A. then drew attention to a tabular view of his results compared to those of Suerman, from which it appeared that there was a very close correspondence between them, a correspondence noticed and admitted by Suerman in the following passage: " siquidem ad diversissimum attendamus apparatum quo usus est, fatendum satis bene sibi convenire experimenta D^ris.^ Apjohn, atque nostra. Utraque vero multum distant ab experimentis doctorum Deloroche atque Berard, ad quæ nostra propius accedunt ratione fluidorum elasticorum elementariorum, experimenta D. Apjohn ratione aerum compositorum." The opinion, however, here expressed cannot, Dr. A. conceived, be considered as well sustained by the numbers adduced by Suerman. Thus, to give but a single example, the specific heat of hydrogen compared to that of an equal volume of air, as deduced by Suerman, was 1.3979, and as deduced, *according to Suerman*, by Dr. Apjohn. was 1,8948. Here is a material difference, quite beyond the probable errors of experiment, in explanation of which Dr. Apjohn stated, that 1,8948, and the other numbers attributed to him by Suerman, were, in point of fact, not those which flowed from his experiments; and at the same time admitted, that when they were so considered by Suerman, he, Dr. A. himself, was in some degree to blame. He had published his first results as the specific heats of equal weights, when they were in reality the specific heats of equal volumes; but in arriving at them it was necessary to be subjected to a particular correction, which, however, materially different in its influence upon them, according as it is applied to the case of equal volumes or of equal weights. Of these facts Suerman was not aware, and was thus prevented from perceiving that the very close correspondence which he recognized between the two series of results amounted to an almost perfect identity.

This identity, however, Dr. A. stated to be true of them when viewed relatively but not absolutely. The direct determinations by Suerman of the specific heat of air and of the different gases, were, Dr. A. alleged, in every instance greater than those at which he had arrived. This he attributed to three causes: 1st. To Suerman's estimating the caloric of elasticity of vapour higher than Dr. Apjohn had done. 2nd. To his employing the formula of Gay-Lussac, which differed from that which Dr. Apjohn had employed in containing as a divisor, not p, but $p-f'$. But, 3rd, principally to Suerman having in no one instance obtained in his experiments depressions of such magnitude as those which he himself had observed. Dr. Apjohn did not explain the cause of this latter circumstance, as it would have required him to describe the very elaborate but rather complicated apparatus of M. Suerman, and to enter upon other details of a critical nature, which he conceived unsuited to a general meeting.

In conclusion, Dr. Apjohn stated, that M. Suerman had, in one direction, prosecuted the research in question further than he himself had done, having experimentally investigated the specific heat of air at a series of pressures less than that of the atmosphere. M. Poisson had given, in his *Traitè de Mecanique,* a formula for solving such problems, derived from analytical considerations, which however was found by Suerman to lead to numbers quite different from those to which his experiments had conducted.

To the preceding abstract of his observations, Dr. Apjohn is desirous of appending the following formulæ:—

$$a = \frac{f'e}{48d} \times \frac{30}{p} \qquad (1)$$

$$a = \frac{f'e}{48d} \times \frac{30}{p-f} \qquad (2)$$

In each of these a is the specific heat of the gas compared to that of an equal volume of atmospheric air, f' the tension of aqueous vapour of maximum elasticity, at the temperature shown by a wet thermometer placed in a current of such gas, and d the depression, or difference between the indications of the wet and a dry thermometer. Formula (1) is that which Dr. Apjohn communicated to the Academy in November, 1834, and which he employed in his researches on specific heats. Formula (2) is that which has been used by Suerman, and the investigation of which he attributes to Gay-Lussac. The former formula had been previously arrived at by Ivory, (a circumstance to which Dr. Apjohn has alluded in his first paper on the Dew-point, without, however, having been at the time aware that Ivory's result and his own were perfectly identical,) and the same is probably true of Dr. August of Berlin, as may be collected from the following passage of Dr. Suerman's Thesis, p. 69:—

"Formulam psychrometricam ex theoria mixtionum aeris ac vaporis, anno 1834 deduxerat D. Apjohn, iisdem innisus principiis quibus Gay-Lussac de aere sicco, August et Ivory de humido, problema solverant, non tamen hos auctores secutus."

The Rev. Cæsar Otway read a paper "on the Ruined Abbeys in the Province of Connaught."

His object was to shew, in the first instance, the difference between the ancient Irish monastic establishments, and those subsequent to the Anglo-Norman conquest. He then called the attention of the Academy to the rapid demolition of these interesting religious structures by the people, who make them places of common and much prized sepulture, and (desiring to mark the places where their friends are buried) recklessly tear down the quoins, corbells, capitals of pillars, and all the elaborate ornaments they can lay their hands on, in order to answer the purpose of head stones.

Mr. Otway suggested the possibility of appointing persons who would act as conservators of these ancient edifices, and expressed his hope that the clergy might be made instrumental in putting a stop to the dilapidations he complained of. Mr. Otway alluded to the well known Fresco paintings on the walls of the choir of the Abbey of Knockmoy; and having stated that a rapid decay is in progress, whereby there is a likelihood of these interesting representations being speedily obliterated, he suggested that some artist (one who combined the execution of a good draughtsman with the taste and enterprize of an antiquarian) should be employed to copy them. Mr. Otway then alluded to the property which one of the vaults at Knockmoy possesses of preserving from corruption the bodies therein deposited, and took occasion to animadvert on the careless and unbecoming way in which these depositories of the dead were left open to public intrusion. Having spoken of Rossreilly Abbey, near Headford, and exhibited a moss-covered skull taken from these ruins, he next adverted to the Abbey of Cong; there also he shewed that the prejudices and superstitions of the people are accelerating the demolition of the building, and, as an instance, he stated how, not long ago, the tomb alleged to be that of Roderick O'Connor, was overwhelmed by a person who, in consequence of a dream, undermined the Abbey-wall to come at hidden treasure. Mr. Otway concluded his paper with an account of Clare Island and Abbey, the residence and place of interment of the famous Grace O'Mealy.

Sir William Betham read a paper on two remarkable pieces of antiquity preserved at Cong, in the County of Mayo.

The first, he stated, is a cross, whose perpendicular shaft is six inches high, the arms one foot six inches, and the whole five-eighths of an inch thick. Upon the edge is the following inscription, intimating that this reliquary once enclosed a portion of the true cross:

HAC. CRVCE. CRVX. TEGITVR. QVA. PASSVS. CONDITOR. ORBIS.

There are several other inscriptions in the Irish characters and language, of which Sir William also gave readings and translations; but these he has since withdrawn.

The second reliquary described in this paper, was known popularly, as the author stated, by the name of the *Breastplate;* but in his opinion it was undoubtedly a case for a manuscript copy of the Gospels. Sir William exhibited a wax model of the front of this reliquary, with a drawing of the cross, which he presented to the Museum of the Academy.

Mr. Petrie, by permission of the President, made some remarks on Sir William Betham's paper, in which he pointed out the original uses of these ancient reliquaries, and detailed at considerable length their history, gathered from the inscriptions found upon them, (of which he gave translations,) and from the mention made of them in the Irish Annals and other records.

It was resolved, that Mr. Petrie be requested to prepare a paper for the Transactions of the Academy on the history of these reliquaries, in order that the valuable information he had collected respecting them might be preserved, and made more generally known.

Dr. Coulter exhibited a specimen of the *Sphinx porcellus*, taken at Killiney, and stated that this rare insect had probably never before been found so far north in Ireland.

The reading of papers being concluded, the session was closed with the following

ADDRESS BY THE PRESIDENT.

The time has now arrived for terminating the present session; and it will, no doubt, be gratifying to you, as it is to me, that our closing act should be the public presentation of a Medal to one of our most distinguished Members; that Medal being the first which has been awarded by your Council in the exercise of the new and fuller power confided by you lately to them, and in execution of the plan which was announced to you at the time when you gave them that enlarged discretion, with respect to the bestowal of honorary rewards.

That plan, as you may remember, differs little from the scheme suggested by me in the inaugural address which I had the honor to deliver on the occasion of first taking the chair of this Academy: the only difference, indeed, so far as science is concerned, being the subsequent adoption of a suggestion of Professor Lloyd, repecting a change of distribution of those subjects which were included by me under the two great heads of Physics and Physiology, but by him under those of Experimental and Observational Science, or Physics and Natural History. The time for acting upon this modification has not, however, as yet arrived; and before the suffrages of your Council were collected, at its last meeting, on the question of the absolute and relative merits of the various communications which have lately been made to our Transactions, it was resolved to postpone, till after the recess, the consideration of all scientific or other awards, except only that which should be made for the most important paper in pure or mixed mathematics, communicated during the three years which ended in March 1837, and already actually printed. The papers coming within this definition were few; the authors of them were only two, Professor Mac Cullagh and myself. The decision, which in theory is a decision of the President and Council, and which did in fact receive my cordial and previously expressed concurence, was in favour of Mr. Mac Cullagh's paper "On the Laws of Crystalline Reflexion and Refraction," contained in the just published part of the eighteenth volume of the Transactions of this Academy.

It may happen that upon future occasions of this sort, if it shall again become my duty to present from this Chair those Medals which may hereafter be awarded, for papers of other triennial cycles, and upon other subjects, I may not think it necessary or expedient to occupy your time by any but the briefest statement of the grounds on which those future awards may have been made. But on the present occasion, which is (to me at least, and in relation to our new plan) the first occasion of its kind; while the subject is one of a class to which my own inquiries have been much directed, and upon which, therefore, I may speak with a less risk of impropriety than upon many others; and while we, as an Academy, by extra hours and extra nights of attendance, during that busy session which is now about to close, have earned for ourselves a little leisure, on this last night of meeting, without interfering (as we hope) with the rights, or even with the convenience of authors; I think myself allowed to enter more at large into the merits of the award, and to lay before you some of the thoughts which the perusal of the present prize essay has suggested to my own mind.

When ordinary light is reflected at the common boundary of two transparent and uncrystallized media, as when we see (for example) the reflexion of the sun in water, the reflected light differs from the incident in both direction and intensity, according to laws which were known to Euclid in so far as they regard *direction*, but of which the discovery, in so far as *intensity* is concerned, was reserved for the sagacity of Fresnel. In general, the laws which regulate the changes of the direction of light have been found easier of discovery than those which regulate its changes of intensity; the laws of the reflexions and refractions of the lines along which light is propagated, than the laws of the accompanying determinations or alterations of its planes of polarisation; or, to express the same distinction in the language of the theory of undulations, it has been found easier to assign the form of the waves which spread from any origin of disturbance through any given portion of the elastic luminiferous ether, than to assign the directions and relative magnitudes of the vibrations which constitute those waves, and the laws which regulate the changes of such vibrations, in the passage from one medium to another.

The laws which regulate such *changes of vibration*, produced by reflexion and refraction, at the boundaries of crystallized media, have been the special object of Mr. Mac Cullagh's investigations, in the paper now before us. But in investigating them, he has been obliged to consider also the laws which regulate the vibrations of the ether, in the *interior* of a crystallized body, and not at its *surface* only; the laws of the *propagation* as well as those of the *reflexion* and *refraction* of light. His researches are therefore connected intimately with a wide range of optical phenomena; and the hypotheses on which his formulæ are founded, and which seem to have their own correctness proved by the experiments of many kinds with which they have been successfully compared, though liable, of course, like every physical induction, to be modified in some degree by future observation, appear to be entitled to assume henceforth a very high rank among the principles of physical optics.

The method which Mr. Mac Cullagh has adopted may be said to be in general the method of *mathematical induction*, as distinguished from *dynamical deduction*. He has not sought to deduce, from any pre-supposed attractions or repulsions, and arrangements of the molecules of the ether, any conclusions respecting the vibrations in the interior or at the boundaries of a medium, as necessary consequences of those dynamical principles or assumptions. But he has sought to gather from phenomena a system of mathematical laws by which those phenomena might be expressed and grouped together, be conceived in connexion with each other, and receive an inductive unity. He has sought to arrive at laws which might bear somewhat the same relation to the optical observations already made, as the laws of Kepler did to the astronomical observations of his predecessor Tycho Brahe, without seeking yet to deduce these laws, as Newton did the laws of Kepler, from any higher and dynamic principle. And though, no doubt, it is to such deduction that science must continually tend; and though, in optics, some progress has been actually made, by Cauchy and by others, to a dynamical theory of light, as a system of vibrations regulated by forces of attraction and repulsion; yet it may well be judged a matter of congratulation when minds are found endowed with talents so high as those which Mr. Mac Cullagh possesses, and willing to apply them to the preparatory but important task of discovering, from the phenomena themselves, the mathematical laws which connect and represent those phenomena, and are in a manner intermediate between facts and principles, between appearances and causes.

It was thus, that, in a former paper, Mr. Mac Cullagh proposed, as mathematical expressions for the phenomena of Quartz, a system of differential equations, which are indeed simple in themselves, and seem to agree well with observation, but have not yet been shewn to be consistent with dynamic views. And in that later memoir for which the present prize is awarded, he has, in like manner, adopted some hypotheses, and rejected others, without apparently regarding whether and how far it may seem possible at present to reconcile such adoption or such rejection with received opinions respecting the mechanism of light; exhibiting thus, a kind of intellectual courage, in admiring which I am fortified by the opinion of Sir John Herschel, who lately, in a conversation and a letter, expressed himself thus to me: "The perusal of Mr. Mac Cullagh's paper on the Laws of Reflexion and Polarisation in Crystals, has, although cursory, produced a very strong impression on my mind that the theory of light is on the eve of some considerable improvement, and that by abandoning for a while the *à priori* or deductive path, and searching among phenomena for laws simple in their geometrical enunciation, and of more or less wide applicability, *without (for a while) much troubling ourselves how far those laws may be in apparent accordance with any preconceived notions, or even with what we are used to consider as general principles in dynamics*, it may be possible to unite scattered fragments of knowledge into such groups and masses as shall afford glimpses of their fitness to combine into a regular edifice."

The hypotheses which are the bases of Mr. Mac Cullagh's theory of Crystalline Reflexion and Refraction are the following. He supposes that the form of the wave surface in a doubly-refracting crystal is that which was assigned by Fresnel, and that the vibrations are tangential to this surface, but that they are perpendicular to the ray, and consequently *parallel* to the plane of polarisation; whereas Fresnel supposed them to coincide with the projection of the ray upon the wave, and consequently to be perpendicular to the plane of polarisation. Professor Mac Cullagh supposes also, with Fresnel, that the *vis viva* is preserved, or in other words, that the reflected and refracted lights are together equal to the incident; but in applying this principle to investigate the refracted vibrations, he supposes, in opposition to Fresnel, that the density of the ether is *not changed* in passing from one body to another. And he supposes, finally, that *the vibrations in two contiguous transparent media are equivalent;* or, in other words, that the resultant of the incident and reflected vibrations is the same, both in length and direction, as the resultant of the refracted vibrations; whereas Fresnel had supposed only that the vibrations parallel to the separating surface, but not that the vibrations perpendicular to the same surface were equivalent.

And here I may be permitted to state, what indeed cannot fail to be remembered by many here, that when the British Association for the Advancement of Science met in this city, about three years ago, (in August, 1835), a communication was made by Mr. Mac Cullagh to the Mathematical and Physical section, "on the Laws of Reflexion and Refraction at the Surface of Crystals," which embodied nearly all the principles or hypotheses that I have now recited, and of which an abstract was printed in the London and Edinburgh Philosophical Magazine for October, 1835, having indeed been published even earlier (in September, 1835) by Mr. Hardy here. The only supposition, which was not either formally stated or clearly indicated in this abstract, was that of the preservation of the *vis viva;* instead of which principle of Fresnel, Mr. Mac Cullagh was, at one time, inclined to employ a relation between pressures, proposed by M. Cauchy. Since, therefore, the leading principles of the new theory of Reflexion and Refraction were all made known by Mr. Mac Cullagh so early as the August of 1835, were printed in Dublin in the September of that year, and in London in the October following, it will not, perhaps, be attributed solely to national partiality if we claim for him the priority of discovery on this curious and important question, notwithstanding that a very valuable and elaborate memoir on the same subject, embodying the same results, was communicated, in December, 1835, to the Academy of Sciences at Berlin, by M. Neumann, and was published in 1837, before the publication (though after the reading) of that essay of Mr. Mac Cullagh, to which the present prize is awarded.

It is, however, an interesting circumstance, and one which is adapted to increase our confidence in these new laws of light, that they should have been independently and almost simultaneously discovered in these and in foreign countries; and it will not, I trust, be supposed that I desire to depreciate M. Neumann's admirable essay, if having recalled some facts and dates which bear upon the question of priority, I proceed to point out a few of the features of Mr. Mac Cullagh's briefer paper, which have appeared to me to deserve a peculiar and special attention. I mean the geometrical elegance of the principal enunciations, and the philosophical character of the interspersed remarks.

As a specimen of the former, I shall select the theorem of the *polar plane*. When light in air is incident on a doubly-refracting crystal, it may be polarised in such a plane, that one of the two refracted rays shall disappear; and then the one refracted vibration which corresponds to the one remaining refracted ray, must (by the hypotheses or laws already mentioned) be the resultant of the one incident and one reflected vibration; and consequently these three vibrations must be contained in one common plane, which plane it is therefore an object of interest to assign a simple rule for constructing. In fact, the refracted vibration is known, in direction, from the laws of propagation of light in the crystal, and the hypotheses already mentioned; if, then, we know how to draw through its direction the plane just now referred to, we should only have to examine in what lines this plane intersected the incident and reflected waves, in order to obtain the direction of the incident and reflected vibrations, and afterwards (by the rules of statical composition) the relative magnitudes of all the three vibrations, or the relative intensities of the incident, reflected, and refracted lights. Now Mr. Mac Cullagh shows, that the desired construction can be deduced from the properties of the doubly refracting medium or wave, as follows: Let OT, OP represent in length and in direction the velocity of the refracted ray, and the slowness of the refracted wave; so that, by what has been before supposed, the refracted vibration OV is perpendicular to the plane TOP; then, if a plane be drawn *through the vibration* OV, *parallel to the line* TP, this plane, which Mr. Mac Cullagh calls the *polar plane* of the ray OT, will be the plane desired; that is, it will contain the incident and the reflected vibrations, if these be uniradial, or, in other words, if they have such directions, or correspond to such polarisations, as to cause one of the two refracted rays in the crystal to disappear.

Many elegant geometrical corollaries are drawn, in the Essay, from this theorem of the polar plane; but I shall only mention one, (which includes, as a particular case, the remarkable law for determining the angle of polarisation of light reflected at the surface of an ordinary medium, discovered by Sir David Brewster,) namely, that when the light reflected from the surface of a doubly refracting crystal is completely polarised, or, in other words, when the reflected vibration has a determined direction, independent of the direction of the incident vibration, then *the reflected ray is perpendicular to the intersection of the polar planes of the two different refracted rays.*

In this and other applications of the theorem of the polar plane to the case where the incident light is polarised so as to undergo a double refraction, the obvious manner of proceeding is to decompose its *one biradial* vibration into *two uniradial* vibrations, and to treat these separately, by applying to each the construction above described. Yet Mr. Mac Cullagh remarks, that it requires proof that the reflected and refracted intensities, thus determined, will have their sum exactly equal to the intensity of the incident light; or, in other words, that the law of the *vis viva* will hold good for the resultant vibrations, though we know, by the construction, that it holds good for each system of uniradial components taken separately. In fact, if the two separate incident vibrations, which correspond to the two separate refracted vibrations, be inclined at an acute angle to each other, they will generate by their superposition (according to the law of interference) a compound incident light, of which the intensity exceeds, by a determined amount, the sum of the two separate or component intensities; and it requires proof that the two separate reflected vibrations will in like manner be inclined to each other at that precise acuteness of angle which will allow the intensity of the compound reflected light to exceed, by precisely the same determined amount, the sum of the two separate intensities, corresponding to the two separate reflected vibrations: (or that the same sort of equality of differences between incident and reflected resultants and sums will take place, when the angles are obtuse and not acute;) the two refracted vibrations being not in general (in either case) superposed upon each other. Professor Mac Cullagh has arrived at an equation of condition, as necessary for the foregoing agreement, which expresses a property of the laws of propagation deduced from the laws of reflexion and refraction, however singular it may appear that the latter laws should give any information respecting the former; and he states that he has found this equation to express rigorously a property of Fresnel's wave. His demonstration of this latter property having not yet been published, I have been induced to investigate one for myself; and have thus been conducted to a construction of the condition in question, so simple that it may perhaps be mentioned here. Let R and W denote the planes VOT and VOP in the figure before referred to, which may also be called the planes of ray-polarisation and of wave-polarisation, for the ray OT, or for the corresponding wave; and let P', T', R', W' be analogous to P, T, R, W, but referred to any other ray or wave; then the following is the relation to be satisfied:

$$\mathrm{OT} \, . \, \mathrm{OP}' . \cos \mathrm{RW}' = \mathrm{OT}' . \, \mathrm{OP} \, . \cos \mathrm{R}'\mathrm{W};$$

RW' and R'W denoting here diedral angles. Under this form, it is easily proved that Fresnel's wave surface possesses rigorously the property in question. Mr. Mac Cullagh's equation has been otherwise obtained by M. Neumann, namely, as a condition for the possibility of depressing the equation of the *vis viva* to the first from the second degree.

On this and many other points of the investigation, Mr. Mac Cullagh (as I have already said) has thrown out many interesting and philosophical remarks; for instance, that the perfect adaptation which thus appears to exist between the laws of the propagation and those of the reflexion and refraction of light, is a strong indication that these two sets of laws are derived from some one common source, in other and more intimate laws not yet discovered; and that it is allowed to hope that the next step in physical optics will lead us to those higher and more elementary principles by which the laws of reflexion and propagation are linked together as parts of the same system. His remarks on the probable connexion between the theories of metallic and crystalline reflexion, and on the hopefulness of ascending to a true theory of light by the method of mathematical induction from phenomena, (exemplified, as has been seen, in his own papers,) rather than by attempting prematurely to make deductions from dynamical principles, are also well worthy of attention, though my own habits of thought lead me to feel an even stronger interest in dynamic and deductive researches.

But I have suffered myself to speak at greater length than has been usually occupied by others before, or is likely to be occupied by me hereafter on other similar occasions, and certainly at greater length than was required to justify the award of your Council. The reasons which I pleaded at the commencement of this address may, perhaps, serve partly as my excuse for having occupied your time so long; and some additional indulgence may have been thought due by those who remember that many years ago, both here and elsewhere, in public and in private, I expressed strongly my admiration of the talents of him to whom I have now the gratifying office of presenting this first public mark of honour from his scientific brethren and cotemporaries.

[*The President then, delivering the Medal to Professor Mac Cullagh, addressed him as follows:*—]

Professor Mac Cullagh,

I present to you this medal, awarded to you by the President and Council of the Royal Irish Academy. Accept it as a mark of the interest and intellectual sympathy with which we regard your researches; of the pleasure with which we have received the communications wherewith you have already favoured us; and of our hope to be favoured with other communications hereafter. And when your genius shall have filled a wider sphere of fame than that which (though already recognized, and not here only) it has yet come to occupy, let *this* attest, that minds were found which could appreciate and admire you early in this your native country.

DONATIONS.

Comptes Rendus Hebdomadaires des Séances de l'Academie des Sciences. Par MM. les Secretaires Perpetuels. Premier Semestre. No. 22, 1838. Tables Alphabetiques, Juillet, Decembre, 1837. Presented by the Academy.

Journal of the Franklin Institute of the State of Pensylvania, and Mechanics' Register. Edited by Thomas P. Jones,

M. D. New Series, Vol. XX. Presented by the Franklin Institute.

Proceedings of the Numismatic Society, 1836-37. Presented by the Society.

A Philosophical and Statistical History of the Inventions and Customs of Ancient and Modern Nations, in the Manufacture and Use of Inebriating Liquors. By Samuel Morewood, Esq. Presented by the Author.

Two Spear Heads. Presented by Sir William Betham.

A Model in Wax of the ancient Reliquary described in Sir William Betham's Paper as " the Breastplate of Cong," with a Drawing of the Crozier. Presented by the same.