Dingle peninsula, from Foylaturrive on the *south* to Brandon bay on the *north*, we find that the strata consist of a base of dark blackish-gray clayslate," &c. Now by reversing the points, Mr. Weaver has made the dark gray clayslate to occur on the north side of the peninsula, namely, at Brandon bay, while it really occurs on the south: the misquotation was doubtless unintentional, but as Mr. Weaver's argument was founded on this misconception, it is unnecessary to reply to it.

In concluding my observations, I cannot avoid expressing my regret that Mr. Weaver was not present either at the meeting of the British Association at Newcastle, or at the Geological Society of London, when I communicated my views relative to the geological structure of the south of Ireland; for, *viva voce* discussion tends more to clear up geological differences than lengthened written descriptions; and where both parties are in search of truth there is little difficulty in attaining it.

Dublin, July 8, 1840.

XXVI. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* By M. DUMAS.

[Continued from vol. xvi. p. 505, and concluded.]

Organic Radicals.

FOR some years organic chemistry has so frequently used what we call *organic radicals*, that it will appear singular to see, if not their existence, at least the reality of the absolute function which they have been made to play, here put in doubt.

We know that by the term organic radicals we mean to designate certain compound bodies which might fulfil their functions in the manner of simple bodies, and which might enter, as they do, and following the same laws, into combination with the various bodies of nature.

If by organic radicals, bodies analogous to cyanogen, to amidogen, to the oxalic or benzoic radical be intended, there is no doubt that there, in fact, compound bodies perform the function of simple bodies, like those analogous to them in mineral chemistry, the oxide of carbon, sulphuric acid, the binoxide of azote, and nitrous vapour.

But if by the term organic radicals we must, as M. Berzelius wishes, designate certain invariable compounds which would fulfil the function of the metals, the theory of types, while admitting their concurrence, cannot allow their permanency.

Thus to fix our ideas, in the theory of types the essence of bitter almonds is a type in which we can substitute for an equivalent of hydrogen an equivalent of chlorine, of bromine, of iodine, of oxygen, or of amidogen, without the type being altered,

But whilst admitting that an element might be substituted for the system $\text{C}^{28} \text{H}^{10} \text{O}^2$, the theory of types does not consider it as an invariable group. It believes that hydrogen may be taken from that group, that chlorine may take its place, or that it may be made to undergo every other modification without its fundamental nature being altered by it.

In a word, by a reciprocity easy to foresee, and which to receive all its development would require a detail of formulæ which I cannot enter upon here, we arrive at the conclusion, that in the same manner that it is possible in an organic compound to substitute sulphuric acid, which fulfils the same function for hydrogen, so we may in certain organic matters substitute a simple body for a group of molecules representing a compound body.

To say that nitrous vapour takes the place of hydrogen in nitrobenzine, is the same as if we said that in æther potassium may take the place of æthyle.

But we must not conclude from this that æthyle is a permanent, immutable (*immuable*), unchangeable compound, for experience proves the contrary. Only by losing some hydrogen and gaining chlorine everything leads us to suppose that it preserves its character, as does the æther, of which it makes a part.

But I admit that in a given type there are certain compound groups for which simple bodies may be substituted, and which in so far would deserve the name of radicals. They fulfil the same function as ammonium, which takes the place of the potassium in alum, for example.

Thus I cannot consider these groups as immutable bodies, for experience has pronounced the contrary, and every theory which would absolutely rest on this basis would go too far.

Amongst the researches which contribute the most to mo-

dify the opinion on the function of the organic radicals, we should cite in the first place the important observations of M. Laurent on the essence of bitter almonds, and those not less remarkable of M. Piria on the hydruret of salicyl*. To resume ; nothing hinders me from retaining the name of organic radicals for certain molecular groups capable of being substituted for elementary bodies which may reciprocally be substituted for them, but these groups may in their turn be modified by substitution, like the other bodies which do not perform this function.

I had a memoir of M. Gehhardt put into my hands, but too late for me to make use of it in the present notice, in which these questions are examined in a manner which appeared to me very worthy the attention of chemists.

Nomenclature.—Amongst the questions which are presented to us as being the immediate consequence of the point of view which we have just set forth, there is one which deserves particular attention ; it has relation to the principle itself of our chemical nomenclature, and to the modifications which the progress of the science has led us to make it undergo.

At the memorable period when the French Academicians, under the influence of the immortal discoveries of Lavoisier, conceived and unfolded the project of a reform in the old chemical nomenclature, they grounded themselves upon the view which Lavoisier himself had just established, that is, upon the existence of those undecomposed substances which were recognized as the material elements of all bodies.

Seeing that by the aid of these elements all the bodies of nature could be produced, that in associating them two and two binary bodies were formed, that in combining these one with another salts were produced, and that in combining these salts in their turn double salts were obtained, the nomenclature had to follow the philosophical principle in all its developments. It required that the names of the elements should be set forth in those of the binary compounds, that they should reappear in the names of simple salts, in those of double salts, &c.

What strikes us in the chemistry of Lavoisier, and in the nomenclature which was the consequence and the expression of it, is the antagonism of the elements which combine to form the binary compounds ; it is the antagonism of the acids and of the bases which combine to form salts ; it is the antagonism of the salts which combine to form double salts, &c.

* [See Lond. and Edinb. Phil. Mag., vol. xvi. p. 210, 211.]

The chemistry of Lavoisier and its nomenclature seemed then to have foreseen and prepared the electro-chemical theory, which has had nothing else to do than to call one of these antagonist bodies the positive element, and the other the negative element.

But let us not lose sight of the great discovery of Lavoisier; it is the discovery of the elements. This is the fundamental principle by which he revived chemistry and natural philosophy. Not a truth of this order is discovered without leaving its impress on all our thoughts; and for the same reason that Lavoisier had established that all the bodies of nature might be formed by means of some elements, he would be led to define the compound bodies by the elements which compose them, and there, in fact, is the principle that our nomenclature has appropriated.

Now not only is the nomenclature of Lavoisier no longer sufficient for us, but it expresses a system of ideas quite contrary to that which we seek to cause to prevail.

It is no longer sufficient for us, because in organic chemistry thousands of combinations are produced with three or four elements, and consequently those could not lend themselves to name all the compounds which result from them.

It is positively contrary to the system of ideas explained above, in this, that it derives the notion of the bodies from the nature of its elements, whilst the latter have only what may be called a secondary interest in the classification.

Each type must have a name, and this name should be found in the numerous modifications which it may undergo, so that it should never disappear so long as the type itself is not destroyed.

It is on this principle that I have already formed the following names: *acetic acid* and *chloracetic acid*, *æther* and *chlor-æther*, *olefiant gas* and *chlorolefiant gas*; names, the object of which is to set forth, as may be seen, the permanency of the types, notwithstanding the intervention of chlorine in the compounds.

The theory of types views these bodies in some degree as *casts from the same mould, with different materials*. It would have the nomenclature always recal their fundamental molecular arrangement, and that it should be put in the first line, whilst the nomenclature of Lavoisier applies itself to the material, brings out the nature of it, and places this notion first.

The theory of types tells you, here is alum of chromium;

the nomenclature of Lavoisier sees in it sulphate of potassa and of chromium under the form of alum.

Alum is a type; all the alums are cast in the same mould; their form is what the theory of types would set forth especially; that which essentially defines each of them. It acts as an artist, who in seeing the statues consisting of different materials cast from the same mould, will say to you, "Here is the Venus of Milo in brass, in lead, in plaster." The artistic type strikes him before he dreams of the material, and he will never think of saying that he is about to show you brass, plaster, or bronze in the form of the Venus of Milo.

An entire reform of the organic nomenclature and of some parts of the mineral nomenclature, appears to me, then, both urgent and possible.

Electro-chemical theory.—We just now saw how the principle of dualism, introduced by the chemistry of Lavoisier in the definition of every chemical combination, was favourable to the conception of what is called the electro-chemical theory. We have also understood how the theory of molecular types swerves from this order of ideas, for it does not suppose two antagonist elements present in the bodies, acting as would two masses endowed with different electricities, and held in combinations by the mutual action of these two electricities.

Does a chemical combination constitute a simple edifice or a double monument? this is the question. In the theory of types, the formulæ combine, and are written without attending to the reduction (*dédoubler*) of each body into two others. In the electro-chemical theory they combine, and are written in such a manner as always to paint to the mind these two principal divisions of the edifice which they represent.

This is the manner in which the theory of types has been driven to separate itself from the electro-chemical theory, or rather that in which this latter has been led to combat the other from its first appearance. The question, however, is given in the clearest way in the following letter from M. de la Rive. The skilful Genevese philosopher, whose name will always be united with the history of electro-chemistry, wrote to me on the 25th of October last (1839) :—

"I have read your researches on *substitutions* with very great interest. They interested me the more as I have been occupied for more than a year upon a rather large work on the electro-chemical theories. I dare not, I must confess, go as far as you; and without believing in the theory of Berzelius such as he has presented it, yet I cannot help thinking that there is something well-founded in the table of the relative chemical

powers of bodies. Now, that hydrogen can perform the function of chlorine exactly, is what I can hardly admit.

"Allow me to ask you if chemists are not rather easy (*facile*) when they group their symbols in every way. There is in this facility of permutation something which does not completely satisfy us physicists, and which appears to lend itself rather too complacently to all combinations. Is there not something arbitrary in the manner in which chemists make these choices? To attack the electro-chemical theory you group your formulæ in a certain manner; immediately to defend this theory M. Berzelius groups them in another manner; where is the law of nature?"*

I shall be pardoned for quoting this letter; it depicts the opinions of philosophers upon questions still new to many minds, and in all cases very obscure to those persons who have not followed them step by step in their development.

Those who have taken a part in the experimental researches of which we are speaking, know well that the electro-chemical theory guided my first studies, that I professed and admitted it for a long time on the faith of its inventors. They also know,

* [It is proper to add here, that M. de la Rive, in the *Bibliothèque Universelle* for February 1840, p. 193, after reciting the passage extracted from his letter by M. Dumas; as above, makes the following remarks on the subject of it and the comments of M. Dumas:—

"In writing these lines to M. Dumas, I sought, as he himself remarked, to satisfy myself concerning a question which becomes every day more obscure. On the one hand, we cannot help recognizing that in organic chemistry, especially the electro-chemical theory, or rather the chemistry which connects the development of electricity with the play of the affinities with which it is always accompanied, has on its side powerful arguments, even when we do not admit on this point all the views of M. Berzelius. On the other hand, there are certainly some phænomena, especially in organic chemistry, in which the function of the same elements in the formation of compounds seems to change its nature in a manner so complete and so extraordinary, that we cannot admit of their possessing a previous predisposition to conduct themselves in such and such a chemical manner, or what comes to the same thing, an absolute electro-chemical power. Would not the result of this seem to be that the electro-negative or electro-positive properties do not previously exist in bodies? That they do not exist until the bodies are presented to each other, and that from that time instead of being *absolute* they are *relative*, that is to say, depend for the same body on the relations which exist between its own nature and that of other bodies in the presence of which it is found? This point of view can only be thoroughly examined by means of direct experiments. I shall return to it when I have finished bringing together a number of facts sufficiently considerable for its justification, if, as I presume, I find it to be founded. I shall be glad to try thus to reconcile, at least in part, the function which M. Berzelius attributes to electricity in the chemical phænomena with the very remarkable laws at which M. Dumas seems to have arrived."]

that it is the force of circumstances, that it is a clear and convincing experience, the production of chloracetic acid, which has led me to admit that hydrogen and chlorine perform the same function in certain compounds. I constructed my formula according to pure chemical experience, my mind being free and disengaged from every view of general theory.

But to admit that chlorine may take the place of hydrogen and perform the same function, was to separate oneself from the chemists who would explain all the phenomena of combinations by means of what is called the electro-chemical theory. I understood it thus, and I found it necessary to explain myself in a direct manner. Besides, how could we believe that this consequence would have escaped the penetration of M. Berzelius, when we see all the value he attaches to giving an immediate explanation, according to the electro-chemical theory, of each of the facts which daily enrich the theory of substitutions, and when we are able to appreciate the high talent he displays in the combination of the formulæ which his theory requires?

It was not necessary to say to M. Berzelius, that in the views of electro-chemistry the *nature* of elementary particles should determine the fundamental properties of bodies, whilst in the theory of substitutions it is from the *situation* of these particles that the properties are especially derived.

We have, however, on this head decisive facts in the domain of mineral chemistry itself. Thus oxygen, sulphur, selenium, tellurium, chromium, iron, manganese, magnesium, and hydrogen constitute a series of bodies capable of taking each other's places, without the form or essential properties of the compounds being changed by it. Thus M. Berzelius attributes to the nature of the elements the function (*rôle*) which I attribute to their position : this is the ground of our respective opinions : let us now come to the point where they separate in practice.

Amongst the consequences of the electro-chemical theory, one of the most immediate consists in the necessity of viewing all chemical compounds as binary bodies. We must always find in each of them the positive particle and the negative particle, or the whole of the particles to which these two functions are attributed. Never was view more capable of shackling the progress of organic chemistry. All the difficulties which we have felt for some years in the inquiry concerning the fundamental formulæ of bodies, the discussions, the misconceptions, the errors, spring from prepossessions which this view had given rise to in our minds.

Some examples will make these two points of view easy of comprehension.

Carbon can combine with oxygen, and thus form carbonic oxide and carbonic acid. In its turn carbonic oxide combines with chlorine, and produces the acid gas discovered by Dr. John Davy. The electro-chemical theory should see in this last an acid chloride of oxide of carbon. The theory of types views it, on the contrary, as carbonic acid, in which for half the oxygen chlorine is substituted. Thus the bodies CO^2 , COCh , CS^2 are modifications of the same type*.

Oxygenated water is a type, and one of the neatest and best-defined (*nets*) that chemistry possesses. Supply the place of the hydrogen by a metal, and you will have the binoxides of calcium, barium, strontium, and in general the simple (*singuliers*) oxides. For these, substitute in its turn for half the oxygen, chlorine, as is the case in chloro-carbonic acid, and you will produce the decolorating chlorides. Thus oxygenated water, the simple oxides and the decolorating chlorides, belong to the same type, to which must also be added the compounds which binoxide of azote forms with the alkaline oxides, so, for example, that we may have the following series:

These compounds of oxides and of chlorine have received all kinds of definitions in the electro-chemical system. Chlorides of oxides, of chlorites, of hypochlorites, have been made of them, as people were guided by the pretended necessity of always putting together in the formula of a compound two antagonist bodies, the positive and the negative.

This is precisely the character of the differences at the pre-

* Here is what I said of phosgene gas in 1828:—"It is easy to see that chloro-carbonic acid corresponds to carbonic acid itself. In fact, in all its combinations one volume of chlorine takes the place of one half-volume of oxygen; it is then as if the oxide of carbon had been changed into acid, by substituting for the half-volume of oxygen which it was necessary to add, a volume of chlorine."—See my *Traité de Chimie*, vol. i. p. 513.

sent time between the electro-chemical school and the school of molecular types.

If acetic acid is deprived of all its hydrogen, and chlorine substituted for that hydrogen, we say that acetic acid and chloracetic acid possess the same molecular arrangement, and that they should possess the same general actions so long as their molecule is not destroyed. Urged by the convenient principles, the electro-chemical system, M. Berzelius, on the contrary, makes of chloracetic acid a separate body, in which he arranges the elements into two groups, which he supposes to be combined with each other. In his opinion the chloracetic acid becomes a compound of oxalic acid and chloride of carbon, a formula which is in no way justified, for chloracetic acid treated with potassa should give chloride of potassium and oxalate of potassa, whilst, according to my experience, it really gives carbonic acid and chloroform.

It is just the same thing as when it was said that the bin-oxide of calcium and the chloride of lime belonged to the same type, that which the interesting and decisive experiments of M. Millon have so well proved; whilst M. Berzelius, relying on ingenious researches, was induced to assert that lime when uniting with chlorine gave rise to a chlorite.

If M. Malaguti takes two equivalents of chlorine from æther, the theory of types foresees and explains that in their place there must have entered into the new product two equivalents of chlorine. It sees æther in the new product, as to the molecular constitution and the fundamental properties.

But M. Berzelius, on the contrary, as might have been supposed, disposes the elements of this new body, and those of the products of which it makes a part, in such a way as to make them constitute binary compounds, which according to these formulæ would possess actions quite opposed to those which have been recognized by M. Malaguti.

In all cases in which the theory of substitutions and the theory of types see *single molecules* losing some of their elements and substituting others for them, without the edifice being modified in its form or its exterior actions, the electro-chemical theory reduces (*dédouble*) these same molecules, solely we must say to find those *two antagonist groups* which it afterwards supposes combined, in virtue of their reciprocal electrical action.

Thus in my opinion the electro-chemical theory has been drawn out of the circle which experience traces for us, when it would have explained the new facts of organic chemistry. But is it to be asserted that the electrical properties of bodies are

without influence on chemical phenomena? Unquestionably not : only it must be agreed, that it is at the moment when the combinations are made, at the moment when they are destroyed, that the function (*rôle*) of electricity may be observed.

But when the elementary molecules have taken their equilibrium, we know not any longer how to define the influence that their electric properties may exercise, and no one has put forth views on this subject which agree with experience.

I have then been induced to declare that the facts which I have just discovered were irreconcilable with the electro-chemical theory of M. Berzelius, who considers hydrogen as always positive and chlorine always negative, whilst we see them supply each other's place, and perform the same function.

But I am far from denying, on that account, that the chemical and electrical forces may be the same, and there is no reason to take up the defence of the general function of electricity in chemical phenomena, when it is simply a particular electro-chemical theory which is under discussion. What I wished to say, what I said, is, that when we have endeavoured to represent the electric state of the combined molecules, pure hypotheses have been attained without any result for the science.

When, on the contrary, as has been done so happily by our colleague M. Becquerel, an endeavour has been made to take advantage of this electricity which shows itself at the moment of chemical combinations or decompositions, results the most important and the most fruitful have been obtained.

It is in this class of facts that the beautiful discoveries of Davy may be classed, those which M. Becquerel pursues with so much perseverance and success ; in fact, the experimental law with which Mr. Faraday himself more recently enriched chemical philosophy.

All the discoveries of these great physicists have reference to the phenomena of the chemical action, and are quite independent of the views which they may have expressed on the function of electricity in compound bodies.

In the course of this memoir I have several times made use of the actions (*réactions*) of bodies, as being the only method quite proper for unfolding their real nature. There is notwithstanding an objection in the experiments themselves, to which I have often referred the reader, thus :—

A chemist who, without knowing the origin of it, had had to study the body $C^8 H^6 Ch^4 O$, seeing that under the influence of potassa this body is changed into chloride of potassium and acetic acid, would certainly have seen in it either a chlo-

ride of the acetic radical, or acetic acid, in which for a portion of the oxygen chlorine had been substituted; yet this body is nothing but the chlorinated æther of M. Malaguti.

Just so, when I was occupied with the study of chloroform $C^4 H^3 Ch H^6$, the manner in which it acts with the potassa, the formation of the chloride of potassium, and of the formic acid which result from it, led me to consider it as being formic acid, $C^4 H^2 O^3$, in which equivalent quantities of chlorine supplied the place of the oxygen; yet M. Regnault has lately shown that chloroform is nothing but some hydrochloric æther of methylene, in which chlorine has supplied the place of a portion of the hydrogen, the body $C^4 H^6 Ch^2$ being changed by this substitution into $C^4 H^2 Ch^6$.

The result of these examples, which might be multiplied, would be, that the actions of bodies are not a faithful guide, for they lead us to refer to acetic acid, a body derived from æther, and to formic acid, a body which represents hydrochloric æther of methylene. But in looking nearer, we see, in fact,

Æther	$C^8 H^{10} O$
Chlorinated æther	$C^8 H^6 O$
	Ch^4
Acetic acid	$C^8 H^6 O$
	O^2 ,

belong really to the same molecular grouping, and that in saying that chlorinated æther is derived from æther, and that it produces acetic acid, nothing really contradictory has been affirmed.

On the other hand,

Methylic æther	$C^4 H^6 O$
Formic acid	$C^4 H^3 O$
	O^2
Chloro-methylic æther	$C^4 H^6 Ch^2$
Chloroform	$C^4 H^2 Ch^3$
	Ch^4 ,

constitute bodies of the same molecular grouping, so that chloroform may be viewed as anhydrous formic acid, or as bichlorinated chloro-methylic æther, without these two ways of regarding it at all contradicting each other.

The result of this is, that chemical actions, without possessing the absolute character which has often been given to them, deserve a confidence which may have been momentarily shaken, but which a profound examination again establishes in its true place in our minds.

In fact, we have admitted that substitutions may unveil the molecular grouping of bodies by furnishing a set of equa-

tions of condition which the general formula ought to satisfy. Now it is evident that the metamorphosing actions are often nothing but means for operating substitutions, by taking advantage of affinities more complicated than those which are made use of in the ordinary substitutions.

It is therefore more than ever requisite to apply to the study of the actions of bodies, and not to trouble ourselves about the distance which often separates the point from which we started from that which we reach; for it may well happen, that these two points, so unlike in their properties, are really united to each other by the theory of substitutions, and belong to the same molecular grouping.

The law of substitutions expresses, then, a simple experimental relation; it is limited to the expression of a relation often observed between the hydrogen lost and the chlorine absorbed, by a hydrogenated body submitted to the action of chlorine. This law establishes only, that if the substance loses 1, 2, 3 equivalents of hydrogen, it will gain 1, 2, 3 equivalents of chlorine; but it does not explain this fact.

The theory of types goes further; it explains what the law of substitutions is content to determine. It considers organic bodies as being formed of particles, which may be displaced and have their places supplied by others without the body being destroyed, so to speak. In the cases above-quoted, the molecule of acetic acid, that of æther, may lose hydrogen and take chlorine, without ceasing to constitute an acid or basic molecule, formed of the same number of equivalents and endowed with the same number of fundamental properties.

It is then because, that on pain of being destroyed, the molecule of acetic acid must take an equivalent of chlorine to stand for the equivalent of hydrogen which it loses, that this substitution, this *remplacement* is effected. Thus it is that the theory of types explains the law of substitutions.

The substitution of one element for another, equivalent for equivalent, is the effect; the preservation of the type is the cause. The organic molecule, the organic type, constitute an edifice, in which a course (*assise*) of hydrogen can have its place supplied by a course of chlorine, of bromine, or of oxygen, without the exterior relations of that edifice being thereby modified. But it is necessary, when the course of hydrogen is taken away, to put something in its place; if not, the edifice crumbles or is transformed.

The law of substitutions was hardly put forth before it became the subject of severe criticisms in Germany, to which I thought it useless to reply. If this law was just, it was for

experience to teach us; if it was false, it was experience which would pronounce its falsity. In all cases it was necessary to leave time to determine its place in science.

The theory of types was scarcely published when the same criticisms were reproduced, at least by M. Berzelius; and notwithstanding all my devotion to the interests of the science, I would again have left to time and experience the care of pronouncing on these debates.

But when I reflected, it seemed quite evident to me, that as a consequence of the researches of organic chemistry, general chemistry had reached one of those periods of crisis, when every one owes to science the testimony of his convictions.

We cannot conceal from ourselves that two systems of ideas are before us:—one, which is supported by all the authority of the past, the rights acquired by quiet possession now for nearly a century, the tacit assent of a great number of chemists, and which reckons amongst its defenders and at their head, a philosopher illustrious amongst the most illustrious, M. Berzelius; the other, which consists in asserting that the bodies formed of the same number of chemical equivalents placed in the same manner, belong to the same molecular type, and often to the same chemical type.

This latter attributes to the number and arrangement of the particles an influence of the first order, which in the ideas of the received chemistry belongs especially to the nature of those particles. The law of substitutions would be the experimental demonstration of this new system, and would have led some of its partizans to adopt it. I do not claim its invention, for it does but reproduce and give precision to, under a more general form, opinions which are to be found in the writings of great chemists, and particularly MM. Robiquet, Mitscherlich, Liebig, Laurent, Persoz, Couerbe, &c. It is precisely this coincidence between the numerous facts, to the discovery of which the law of substitutions has led, and the opinions already known relative to the influence of certain pre-existing molecular arrangements, that has given me the confidence necessary for their adoption in my turn when I proposed the admission of organic types.

Here we have, then, before us two systems: one which attributes the principal agency to the nature of the elements, the other which reserves it for the number and arrangement of the equivalents.

Pushed to an extreme, each of them in my judgement would be found to lead to an absurdity. Regulated by experience and kept by it within prudent limits, each of them must take a large share in the explanation of chemical phæ-

nomena; and to explain by a last word the meaning which I attach to their respective functions, I shall say that in chemistry the nature of the molecules, their weight, their form and their situation, must each exercise a real influence on the properties of bodies.

It is the influence of the nature of molecules that Lavoisier has so well defined, it is that of their weight which Berzelius has characterized by his immortal labours. It might be said that the discoveries of Mitscherlich relate to the influence of their form, and the future will prove whether the present labours of the French chemists are destined to give us the key to the function which belongs to their position.

We subjoin to the preceding memoir by M. Dumas, a translation of an extract of a letter from M. Baudrimont, published in the *Comptes Rendus*, for March 16.

"M. Dumas says, that the law of substitutions, and the theory of chemical types, are unconcerned in the reclamations of M. Baudrimont, who does not admit them. This requires an explanation from me.

"I cannot admit M. Dumas's law of substitutions; first, because it has not the character of a physical law; secondly, because it is but the strict expression of an order of facts, much more extended than M. Dumas supposes; *but I admit chemical substitutions; for substitution is only one of the modes by which bodies may enter into combination.*

"I should without doubt do more than M. Dumas in saying to the Academy: *Chemical compounds are produced, either by direct combination or by displacement, or by substitution, or lastly, by several of these modes united. Substitution may be non-equivalent, equivalent, isotypic, isorhythmic, or isomorphic*; let me be pardoned this neologism. But this formula, which is true, has not the character of a law; it is but the general expression of facts which are within the knowledge of all men, ever so little versed in chemistry; for chemical substitutions have been known ever since we arrived at the knowledge that one metal can precipitate another, taking its place in a saline solution; ever since hydrogen was first obtained by displacing it by iron or zinc in the pretended sulphuric and chlorhydric acids diluted with water; ever since we knew that chlorine displaces bromine and iodine; ever since we knew isomorphism by substitution; and ever since M. Bendant made more than a thousand applications of them to the calculation of the composition of minerals.... I admit, then, chemical substitution; but I repudiate the pretended law of M. Dumas, for the reasons I have just set forth.

“As to what relates to chemical types, which M. Dumas says I do not admit, I dare hope that the Academy, and all enlightened men will not participate in this opinion; for it will without doubt be allowed that he who first classed chemical types, must necessarily have admitted them, even before M. Dumas had acquired any notion of them, as his memoir appears to show, since he makes this notion only go back to his experiments on chloracetic acid, the discovery of which is posterior to my thesis.”

M. Dumas replies, that his memoir is conceived in such terms, that it should have spared the Academy all the reclamations of which it has been the subject. In a historical note which he intends soon communicating to the Academy, he will show in what the views which are represented as identical differ, and to whom belongs the discovery of each of the principal points of the theory.

XXVII. *On the Ferrosesquicyanuret of Potassium*. By
ALFRED SMEE, Esq., Surgeon.*

THE action of chlorine upon the ferrocyanate of potassium is a subject of much interest to the chemist, and has not been examined to any extent in this country. It therefore has been my endeavour to investigate this action carefully, and to see under what circumstances the change from the ferrocyanate into the ferrosesquicyanuret takes place; and the methods which are here detailed to obtain this latter salt uncontaminated with impurities, will be found free from the difficulties and uncertainties attending on the present mode of preparing it.

When a current of chlorine is passed through a solution of ferrocyanate of potassa, or an aqueous solution of that gas is added to it in certain quantities, the persalts of iron are not precipitated. This solution has no smell of chlorine, and is changed from a yellow colour to a dark red, and deposits on evaporation red crystals. A similar change takes place when bromine is added to the ferrocyanate, and in both cases the weight of the entire red mass is equal to that of the yellow ferrocyanate, plus the weight of the chlorine or bromine used, but minus the quantity of water which the yellow crystals are known to contain. This indicates, first, that the red crystals are anhydrous; and secondly, that the chlorine or bromine is actually absorbed by the salt. The former fact is con-

* Read before the Royal Society, June 18, 1840; and now communicated by the Author.