XV. On the scientific labours of Jeremias Benjamin Richter. Addressed to the Imperial Academy of Sciences of St. Petersburg, at the public sitting of Dec. 29, 1840

M. Hess

XV. *On the Scientific Labours of* Jeremias Benjamin Richter. *Addressed to the Imperial Academy of Sciences of St. Petersburg, at the public sitting of Dec.* 29, 1840, *by* M. HESS, *Member of the Academy*.*

GENTLEMEN,

THERE is perhaps no one here present who does not reckon amongst the fairest enjoyments of thought those moments which from time to time he is able to devote to the remembrance of men of genius who have bequeathed to us important truths. And when I proceed to show that the veil which obscures the memory of one of these has yet to be torn away,—that the labours of twenty years employed in rendering a truth evident to the eyes of the most incredulous, are not yet appreciated,—you will then, I cannot doubt, grant me a moment of the attention which your kindness would not have refused to a cause less disinterested.

In the exact sciences, as in all other cases, nature does not allow us to proceed *per saltum*; it is necessary that every thing should be unfolded gradually. It is the most simple phænomenon which first takes its place in the domain of intelligence; the most complicated—the most difficult, is that which comes the last. Thus when at the beginning of the eighteenth century, astronomy, thanks to the numerous labours summed up by the mind of one great man, thanks to the simplicity of his principle, assumed the rank of a science almost perfect, about that time did chemistry, with difficulty, attempt to assume a scientific form. You will perhaps suppose that this is to be attributed to the men who were engaged in it; but you will soon abandon this idea when I tell you that Newton, who discovered the law of gravitation, also applied himself to chemistry, that he decomposed the subtile matter of light, whilst

* From the *Recueil des Actes de la Séance Publique*, Dec. 29, 1840.

Phil. Mag. S. 3. Vol. 21. No. 136. *Aug.* 1842.　　　G

not only the air, but water and even earth still resisted the efforts of three generations.

However, George Stahl, a resident at Berlin, established his theory of phlogiston which so long preserved its dominion in the mind. Air was at last decomposed, and water also. Lavoisier next analysed the phænomenon of combustion; and from this period the new ideas became diffused; the impulse was given, earth itself was analysed, and the number of combinations was increased in a wonderful manner, without the existence as yet of any known law to reduce this labyrinth to order. Many persons still remember the manner in which analyses were recorded; everything was reduced to hundredths, and thence resulted a confusion the shackles of which must have been felt, in order to appreciate the system of notation now used, at its just worth. It was Jeremias Benjamin Richter, assessor at the office of mines at Berlin, who first gave order to this chaos. You therefore would expect that the highest esteem would invest his works, that his name was revered. No; Richter was not appreciated, he was almost forgotten whilst alive. He died at Berlin the 4th of May, 1807. The same year a celebrated author tells us, that being employed in drawing up a treatise on chemistry, amongst other works but *little read* he ran through those of Richter. He was struck with the mass of light which he found there; but by a fatal chance he attributed to Wenzel, whose works he must have read at the same time, the most beautiful result obtained by Richter, that which was to serve for a foundation to the whole edifice. In order to explain how it was that Richter had been forgotten, the author to whom we allude says that his results were not exact, which must have weakened the impression the perusal of his works must have made, and so much the more as Richter almost always took the *carbonate of alumina* as the point of departure, a combination which we know does not exist. Let us not be surprised, then, that the most celebrated French authors repeat, on the authority of a great name, the same errors concerning works which they have not read; we see, for example, the author of the *Leçons sur la Philosophie Chimique* explain things in the same way, and reduce the merit of Richter almost to nothing. " Can you believe," says he, " that in establishing his doctrines he nearly always takes the carbonate of alumina as the point of departure?" In short, Richter is there reproached with having *too much obscured the questions upon which Wenzel had begun to throw light* *.

[* Our own countryman Dr. Wollaston, it would appear, justly appreciated the labours of Richter : see the paper explaining his *"Synoptic Scale of Chemical Equivalents"* in the Philosophical Transactions for 1814, p. 3, 4. —Edit.]

If in general, gentlemen, it is a duty to render justice to merit, in the present case it is at the same time a right; for J. B. Richter, almost unknown by the rest of Europe, was elected a correspondent of this Academy on the 14th of May 1800. Let us examine his title to our esteem. It is the best homage we can render to his memory.

Richter published in 1792 and 1793, a work in three volumes under the title of *Anfangsgründe der Stöchiometrie, oder Messkunst chemischer Elemente,* in which he sets forth his ideas in the form of a systematic treatise. But this form, you know, is little suited for spreading new ideas. How can a reader be expected to gather five hundred known ideas in order to discover one that is original ! Has not each professor his treatise, and would it not be a punishment to have to study nearly the greater part of it ? This mode of publication does not promise success to any but authors who have already acquired great celebrity, and with whose works we are obliged to become acquainted. So Richter, beginning by a work in three volumes, was not read. Seeing that the great truth which he had in view was not appreciated, that he was exposed to unjust criticisms, whilst his work was not read, he resolved to publish his researches separately, which he did under this title, *Ueber die neueren Gegenstände der Chemie,* in eleven small volumes of from 100 to 250 pages each. They appeared from 1793 until 1802. " I should (says Richter in 1799) certainly not have followed up these two first volumes (Stücke) by seven others, if too severe a criticism of the antiphlogistic school did not endeavour to put under the bann of sound reason all those who think differently from it, and if to this was not added the annoying circumstance that my *Stöchiometrie,* although endowed with a sound constitution, is nevertheless consigned to the shelf of the shop-keeper."

In the introduction to the first part, Richter tells us he hopes that the part of chemistry which treats of affinities and quantities will soon become a part of applied mathematics. Here then is the preconceived idea, the point whence Richter set out;—weigh even the form of his expressions, and you divine nearly all his life. " Some experiments which I have just made, having the same aim in view (says Richter, vol. i. § 121), make me think that if we could employ suitable expedients, we should find that the neutrality of pure elements, setting out from one amongst them which is taken as unit, increases in a positive progression." We see the idea was truly philosophical; it was necessary to develop it and to become assured whether such a relation existed or not. It was a source of serious errors to him, and drew upon him too severe judgements. He devoted a part of his works to fathom

G 2

this question, and remained persuaded that the equivalent of all bases belongs to an arithmetical progression, whilst the numbers, which express the equivalents of the acids, form geometrical progressions, the ratio of which is different according to the different groups of acids.

Now it is well established that facts do not support this notion of Richter's: we shall therefore pass over this part of his works, and I shall return to them but once, in order to show how it was that his experiments were sometimes so far from the truth as not to undeceive him. But if we go back to the time when he lived, we shall feel that the question raised was vast, and that if his undertaking was not crowned with success, he at least deserves that these words should be applied to him:

" Quem si non tenuit, magnis tamen excidit ausis."

Amongst the numerous subjects which Richter treats of in the first volume, I shall only quote the method which he points out for extracting platinum from the ore of that metal; for it is still used. He recommends precipitating the solution of that metal by sulphate of potash, to wash and dry the precipitate and to decompose it by the carbonate of potash, so as to divest it afterwards of all the salts by washing it with water. The metal then remains brilliant as silver. The explanation of the processes follows, which gives him an opportunity of making some very important remarks. When we shall have found, says he, numerical expressions for affinity, then these seeming anomalies will disappear. Upon this occasion he explains the difference between simple affinity and double affinity, and observes that it is nowhere proved that we can really isolate a simple body, for, he says, each time that we disengage an alkali or a metallic acid, if it be only carbonic acid, heat must then be substituted for the acid; lime is an example of this. So it is with the acid from which we take a base, it is combined with, or even neutralized by heat. In the case of a simple affinity, we suppose but two elements, whilst this shows you that there are at least three, for every time that neutrality takes place, heat is substituted for the third element. This is even the case when a metal is dissolved by an acid and neutralizes it, for then it is the acid that furnishes the heat, which becomes united with the other elements. Richter therefore knew that bodies were pervaded by heat; he urges the necessity of taking these phænomena into consideration, but he does not yet take a perfectly just view of them; he believes that heat is just added to the elements, when we know, on the contrary, that it has just been disengaged.

The third volume (1793) is wholly devoted to a critical examination of Lavoisier's antiphlogistic system.   Up to that time Richter had only known it by very insufficient extracts. But in 1792 appeared a German translation of Lavoisier's treatise on chemistry, by Girtanner.   Richter obtained and read it, and was convinced of the truths of the new system. Yet indulgent towards others and a stranger to the spirit of party, he excuses those who refuse to admit it.   " For," he says, " in the ancient system, metals and sulphurs were considered as compound bodies, earths and acids as simple bodies; in the new system it is just the contrary: now imagine a man whom you would persuade that all he sees he sees reversed, and then condemn him for his incredulity.   But, nevertheless, an error does not become a truth should it even count myriads of ancestors."

Do not suppose however that Richter, upon embracing the new system openly, abandons himself to it without criticism. No. No one to my knowledge has better perceived what there was good in the fundamental principle of the phlogistic system. We must not expect that a system which served, for nearly a century, as a starting-point for the numerous investigations of chemists, that a system which could rally round it all facts, should be entirely illusive.   " All the facts on which the partisans of the antiphlogistic system rest," says Richter, " are not only insufficient for the refutation of the reality of phlogiston; but on the contrary, they do but rectify our ideas with regard to it and render its existence more evident; for example, when we assert that phosphoric acid is composed of phosphorus and oxygen, this conclusion has no foundation, since in reality no other conclusion can be drawn from the experiment, except that this acid is composed of the radical of phosphorus and of oxygen.   Not any induction can be drawn respecting the nature of this radical itself, for it is only known as combined with oxygen or with phlogiston (*Brennstoff*) ; which, however, does not prevent us from indicating the relative quantity of the elements, since, for us, the weight of phlogiston, like that of heat, is an infinitely small quantity." Such was the capacity of Richter's mind, that in the midst of the lively contention of two parties who do not agree, he quietly examines the question, seizes the literally palpable truths of the new school, and yet does not abandon the more abstract, more hidden but not less real truths of the old system.   Perhaps Richter had a model, but then this model was Lavoisier, and no other.   But it is certain that at the present time, this manner of viewing the subject is banished from all works which treat of this science, and that it is after a lapse of forty years

that considerations of another order, supported by decisive experiments, seem to enable us to appreciate his ideas properly.

Before Richter, and in his time also, it was supposed that the affinity of a substance was in the direct ratio of the quantity necessary to saturate another body. Richter compares the quantities of tartaric and of acetic acid necessary to saturate the same quantity of lime. He finds that more tartaric acid is necessary, and concludes that its affinity is greater, and that consequently this acid should displace acetic acid. He makes trial of this, and really it is tartaric acid which seizes the lime and displaces the acetic acid. Few examples are found more suitable than this for characterizing the difficulties which are met with every day in chemistry, for here is a well-observed fact, a conclusion drawn; an hypothesis follows, then comes the experiment which confirms it. You believe your principle well established? By no means. Another fact comes and overturns it. Subsequently Richter again takes up the question, and this time he clearly proves, that *affinity is not exerted in the ratio of the masses which combine.*—Vol. x. p. 187–195.

It is in the fourth volume (viertes Stück, 1795) that Richter establishes truths which will always be reckoned amongst the most important acquisitions in the region of the exact sciences. He begins by researches on the capacity of saturation of hydrofluoric acid; for this he uses several bases, and does not neglect alumina. He tells us (p. 10) that he took 650 grains of very pure carbonate of alumina, which he saturated with hydrofluoric acid. Here then is what he is accused of, for carbonate of alumina does not exist! The parenthesis, then, where he says that 1000 parts of this alumina contained 542 of pure alumina, has not been read. Nor have his calculations been followed, for he everywhere takes into account alumina at the rate of 542 parts for 1000. All of you, gentlemen, who add experience to a general knowledge of chemistry, will know that it is very difficult, I may say almost impossible, to obtain pure alumina; if we precipitate it from its solutions by the carbonate of ammonia, it always retains a little of this salt, and water besides. It is only by calcination that we can obtain it really pure; but then it becomes difficult to dissolve in acids. This, doubtless, is the reason why Richter used non-calcined alumina, and determined by a separate experiment the real amount of that earth which it contained.

After having found the quantity of different bases by which 1000 parts of hydrofluoric acid were saturated, a verification is required. For this purpose he decomposes fluoride

of calcium by sulphuric acid, and infers the quantity of lime to be found in the hydrofluate from the quantity contained in the sulphate.   He thus finds by analysis, that 1000 parts of hydrofluoric acid require 1882 of lime for their saturation; by synthesis he finds 1865 parts.   After that, he finds that the same quantity of acid was saturated by 3797 parts of potash, and continues in these terms: " It has been shown (he speaks of his *Stöchiometrie*) that the quantities, whether of alkali, or of alkaline earth, which served to saturate the same quantity of one of the three *volatile** mineral acids, were in *constant relation* with each other."   Richter then examines whether the results which he has just obtained support this proof: he had before found that 1000 parts of muriatic acid require 1107 parts of lime for perfect saturation, and 2239 of potash.   For hydrofluoric acid he had obtained 1882 parts and 3797.   But 1107 : 2239 = 1882 : 3807, which differs very little from the result of the experiment.

A happy and important discovery is not all; the consequences of it must be felt; the promptitude of intelligence must go beyond the tardiness of experience, for it is only in this future that we can be armed against all the shackles of the present. Now this is the manner in which Richter announces and extends the consequences of his experiments (vol. iv. p. 67, year 1795).   When two determining (*determinants*) elements (i. e. two acids,) each taken at the rate of 1000 parts, are saturated by the substances a, b, c, d and α, β, γ, δ, so that each time a and α, b and β, &c. are always the same substance; in this case the (substances) quantities a, b, c are among themselves absolutely in the same relation as α, β, γ.

This theorem of Richter's is a true touchstone for all experiments which relate to neutrality; for if the results do not agree with this principle, they must be rejected without hesitation.   But, he adds, although according to the announcement of the principle we may use relations known and determined by experiment, in order to find others by calculation, it will always be useful to verify these last by the fact, for we gain by it, after having recognised certain relations, the means of verifying the numbers themselves from which we had set out, and thus to correct the little inaccuracies by which they might be affected.

Richter then points out the work to be done; but in order to feel all the importance there is in its being done with the greatest precision, it will suffice to tell you that he forms a *plexus of number*, which covers the entire domain of chemical researches whatever they may be, and that it is precisely from not having

* By these he understood the sulphuric, nitric and muriatic acids.

performed analysis with skill enough, that Richter remained all his life uncertain on several points.

Here is certainly one of the most striking proofs of the progress we owe to him. He makes analyses, and deduces a general principle from them, and from that time these same analyses are no longer sufficient for the increasing wants of the science. To set out from hence the task imposed by Richter becomes gigantic. New methods are necessary. We owe them to M. Berzelius; it is he who executed this work with a precision very rarely equalled, and which not only has not been surpassed, but never will be by these methods.

Richter, after having established this principle, continues to apply himself to the subject; he determines the capacity of saturation of acetic acid, by lime, by magnesia, by barytes, and finds that in order to saturate 1000 parts of this acid, supposed anhydrous, Ca 523, Mg 405·6, Ba 1465 are necessary, which gives for the composition of these salts,

	According to Richter.	According to Berzelius.
For 100 of Ca Ā ..	Ca 34·34 Ā 65·66	Ca 35·63 Ā 64·37
Mg Ā ..	Mg 28· 8 71· 2	Mg 28·66 71·34
Ba Ā ..	Ba 59· 4 40· 6	Ba 59· 8 40· 2

Let us observe that there is no question of alumina; it is, says he, because he is not able to find with precision the point of saturation for this base. You therefore see a real difficulty which stops him, this combination being one of those which he is more certain of determining by calculation than by experiment.

These researches lead Richter to the conclusion that acetic acid follows the same law as the acids before considered. He then shows that the same law is also applicable to the citric, oxalic, tartaric, formic, and several other acids. It is essential to observe, that in order thus to prove by experiment the generality of the principle which he had established, an entire series of analyses was necessary for each acid, and it will be easy to judge of the ardour and time he must have expended on these labours. But in these same works he applies his principle; as for example, he often meets with difficulties in finding the point of saturation for carbonic acid, he sets out then from a combination which he thinks well known. There again he avoids alumina as not adapted to his object, and he selects carbonate of lime. His choice could not then fall better. He finds that 1000 parts of carbonic acid are saturated by 1373 parts of lime, which gives for 100 parts Ca 57·86 and

$\dot{C}$ 42·14; according to Berzelius, $\ddot{C}a$ 56·29 $+ \ddot{C}$ 43·71. Not only does Richter not choose the carbonate of alumina, but he examines the question to discover why the carbonate of this base treated by an acid disengages less carbonic acid than another base. You see then the ambiguity that there is in this substance by no means escapes him, and he continually returns to it as an enigma. Richter, armed with so powerful a principle as that which he had discovered, could not limit the application of it to his own labours; he also applies it to those of others, and rectifies or confirms them; for he was, so to say, endowed with a sense more than his contemporaries.

Berthollet had found, as Lavoisier says in his treatise on chemistry, that 69 parts of sulphur absorb 31 parts of oxygen to become transformed into sulphuric acid. Richter repeats the experiment and comes to a very different result. He oxidates sulphur by nitric acid; then converts it into sulphate of lime and obtains 947 parts of this latter for 222 of sulphur, which makes 856 parts for 201 of sulphur, whilst we admit at present 857·1. He then greatly approaches the truth, but to deduce the composition of sulphuric acid, that of the sulphate must be known exactly. This not being sufficiently well known to him, he finds that 201 parts of sulphur absorb 227 instead of 300 of oxygen to be converted into sulphuric acid (vol. v. p. 124), which compared to Berthollet's result, is still a very beautiful approximation, since this latter had only found 90 parts instead of 300. Then he is reproached with Bergmann's researches on the sulphates of potash and barytes. They are not just, he says, for if we suppose the salts compound, as Bergmann points out, and if one of them is mixed with a neutral salt, by which it may be mutually decomposed, there will be an excess of acid or of base, which cannot happen; every one knows that the solutions remain neutral (vol. vii. p. 94 and 95): therefore his analyses are false.

Klaproth had discovered strontian*; he describes and analyses several of its salts, without attention to Richter's principles. The latter applies them and finds that the analyses of Klaproth agree with the principle, and consequently that they are exact.

It is this very important discovery which has been attributed to Wenzel. This question therefore demands an attentive examination; for, take this title from Richter, and you make him fall back into the category of ordinary philosophers.

---

* [Strontian was first discovered by Dr. Hope; though its discovery about the same time, or shortly after, by Klaproth, appears to have been an independent one.—EDIT.]

It is no longer a summit; it is no longer to him that the che-
mist owes the compass without which he could not navigate.
Well, not only does Richter in his *Stöchiometrie,* vol. iii.
p. 285, use this principle in order to verify the results of his
contemporaries, but even those of Wenzel are submitted to
this test. This, it may be objected, is not a proof, for he may
not have quoted the author from whom he has borrowed the
idea. But I have read and re-read Wenzel, and not a word,
not a trace of this idea is to be found in his work. It was
possible that an edition reprinted in 1800 might be inex-
act; I referred to that of 1782, and with the same result.
Here however is an unexceptionable proof that the principle
in question really belongs to Richter and not to Wenzel.
Open Wenzel's work, and you will find at the end a chap-
ter which is entitled " Applications of the laws of affinity
to particular cases" (*Anwendung der Lehre von der Ver-
wandtschaft der Körper auf besondere Fälle*). This is the
manner in which Wenzel expresses himself: "In chemistry, as
in every other natural science, the essential aim is to compare
recognised facts in their mutual relation, in order to deduce
other truths which are not perceived at first view. In the
experiments above quoted, we came to a knowledge of the
phænomena which took place, by the fact of the union of two
substances. We saw in what order, under what condition,
and in what proportions they are combined. The greater part
however of these experiments, considered singly, are not of
great importance, whilst we only limit science to that. But
they acquire importance as soon as we apply them properly,
for their merit essentially depends upon a happy application."
    Let us follow Wenzel in his applications, and let us choose
for this purpose § 7. There he proposes as a question to find
the simplest and most advantageous manner of obtaining cry-
stallized verdigris. Here is what he proposes:—the sulphate
of copper and the acetate of lead are both soluble in water;
if these two solutions are mixed, the sulphuric acid by virtue
of its affinity for the oxide of lead will seize upon this and
form an insoluble substance, which may be utilized in the
arts on account of its whiteness. The liquid will contain
some acetate of copper which we separate from the precipi-
tate. Depending upon his analyses, Wenzel calculates the
quantity of oxide of lead contained in a given quantity of
acetate of lead. He then calculates the quantity of sulphate
of copper necessary to precipitate all the oxide of lead. That
done, he examines the question, to learn whether the acetic
acid which the oxide of lead has just left is sufficient to dis-
solve all the oxide of copper which has just been left by the

sulphuric acid, and always starting from his analyses, he comes to the conclusion that the acetic acid set at liberty is not sufficient to dissolve all the oxide of copper, and that for the quantity of copper employed, which is 124 parts, there will be found of it $9\frac{1}{3}$ parts mixed with the sulphate of lead as an oxide. In this case Richter, starting from his principle, would necessarily say, these analyses are false ! as he did in many cases. What does Wenzel? he, on the contrary, concludes that after having separated the solution from the precipitate, this last must be treated with a little sulphuric acid to remove the oxide of copper. Here then is a very evident proof that Wenzel did not even suspect a similar relation to that which was discovered by Richter. Richter not merely discovers this principle, but he comprehends it in its totality; he follows it in all its consequences, and nothing can show us more fully the depths of his convictions with respect to this, than some words which are to be found in the preface to the 10th volume. " The theorems of stœchiometry," says he, " contain a *necessity*; they may be constructed and have the value of *à priori* principles."

These principles conduct him to new generalities. He finds that when a metal is precipitated from its solution by another metal, the quantities of oxygen necessary to preserve equal quantities of the two metals in solution, are to each other in the inverse ratio of the masses of the two metals. Further on, he concludes, since when several metals are precipitated from solution by one another, the solution always remains neutral, it is sufficient to know the difference of weight between one of these metals and its oxide, to deduce from it the quantity of oxygen which all the others contain in the state of oxide. For this it is sufficient to take a constant quantity of the same acid, for then all the metals that may be dissolved in this acid will contain the same *quantity of oxygen*, which will then only have to be deducted from the weight of the oxide, in order to obtain that of the metal.

Richter takes sulphuric acid for a starting-point, and prepares a table of the composition of the metallic sulphates ; in this table the quantity of oxygen of the metal being necessarily constant, he designates it by the letter U. This is what we now designate by the letter O. Richter was then very near establishing a system of equivalents, just like that which is at present used ; for that object it was sufficient to refer all the numbers to this constant quantity U. But this simple idea had not struck him, for in another column he gives the composition of the muriates, taking 1000 parts of muriatic acid as a starting-point ; in another column, indeed, he gives the composition of

the nitrates, taking for starting-point 1000 parts of nitric acid. His numbers therefore varied continually, which must have kept many relations concealed from his sight.

Nevertheless these tables constructed by Richter have another peculiarity which merits our attention. The names of metals are not found in them in writing, but the signs then used are substituted for them, as ♂ manganese, ♂ iron, ♂ zinc, ☽ silver. But here signs fail him, for several metals had just been newly discovered; these Richter expresses by the two initial letters of the name, for example, χϱ for chrome, Ti for titanium, Te for tellurium. Here then is the first idea of the notation so happily completed by M. Berzelius.

We see Richter continually occupied with the phænomenon of neutrality. What then is the neutrality of a solution? This is a thorny question, and one to which, even at the present time, many authors answer only in an obscure and evasive manner. Neutrality, says Richter, is absolute or relative: it is absolute when the solution does not exert any reaction on test papers; it is relative when the neutral salt nevertheless exerts an acid or alkaline action. But in this case, he says, however decided may be the reaction exerted by a metallic solution (for example the nitrate of silver), you recognize, nevertheless, that it is neutral, because the least addition of an alkali causes a precipitate which will not dissolve again without adding an acid.

Although Richter had recognised the fact that different metals required the same quantity of oxygen in order to be dissolved in the same quantity of acid; notwithstanding, he says, when metals become charged with oxygen without the intervention of an acid, that by no means prevents them from taking very different quantities. Richter, as we see, was not ignorant that there were different degrees of oxidation, and he employed himself in determining several of them. As, however, the works of Richter which relate to the oxides of metals are far from being very exact, let us examine an example in order to discover to what the inaccuracies met with in his determinations are to be attributed.

He knew, for example, that arsenic formed two combinations with oxygen, arsenious acid and arsenic acid. He determines by a direct experiment the quantity of oxygen which arsenious acid takes to become converted into arsenic acid, and finds that 100 parts of acid absorb 17·2 of oxygen, which is not far distant from the real number, 16·17. He afterwards seeks to determine the quantity of oxygen which metallic arsenic absorbs to become converted into arsenious acid, and he finds for 100 parts of metal 15·1 parts of oxygen, de-

viating greatly from the true number, which is 31·9. Having
a false idea of the composition of arsenious acid, he neces-
sarily deduces a false composition for arsenic acid.   Now
this is the way he obtains a number so far from the truth:
he converts a given weight of regulus of arsenic into arsenic
acid, and then into arseniate of lead.   But instead of drawing
a conclusion from the weight of this latter, he first tries to
determine the quantity of arsenic acid which the precipitate
should contain, and for that purpose sets out from the arseniate
of magnesia, which must necessarily compromise all the re-
sults; for in order to determine the composition of that salt, he
saturates a solution of arsenic acid by the carbonate of magne-
sia, a salt whose composition is not always constant. Then he
determines the quantity of arsenic acid from a table of density
previously constructed. In this then consists Richter's greatest
fault, I will even say the only one which he has committed, but
from which several others originate: he did not yet quite ap-
preciate the difference which exists between a direct and an in-
direct method.   This is the true source of all his errors.   To
make amends for this, each time that he makes a direct expe-
riment, he approaches very nearly to the truth; for example,
if he wished to know of how much oxygen and cobalt the oxide
of this metal is formed, he determines this quantity in a direct
manner, and finds for 100 parts of cobalt 26·5 of oxygen, which
does not widely deviate from 27, which is the real number.
But Richter distrusts himself. He tells us (vol. ix. 1798, pre-
amble) that he cannot easily manipulate; that he was never
able to finish an analysis without losing something at the end
of all the operations; and that he never dared to undertake
an investigation if there was any question of stœchiometrical
determinations, with so small a quantity as 100 grains, but that
he needed 500.   This is perhaps the reason why Richter at-
tached great value to the tables of density, whether for acids
or for salts. A considerable part of his time too was employed
in making them. At the end of each acid he gives a table in-
dicating the acid contents in a solution at different degrees of
density. He does the same for the salts which are most used.
Richter was also much employed at different times in con-
structing areometers and alcoholometers; we still use many
instruments which bear his name.
    It is not only when Richter treats of general questions that
he merits all our attention; he often captivates it by questions
which are quite special.   A few examples will suffice in order
to appreciate him.   We have seen that he confirms the re-
searches of Klaproth on the composition of the salts of stron-
tian, but, he says, my conclusions are not just unless the salt

which I have used was pure. He had prepared this salt by
dissolving the natural carbonate; the object in question then
was to know if it did not contain lime or barytes. He finds
that a solution of strontian is not troubled by adding a solu-
tion of ferrocyanate of potash, whilst the least portion of
lime or of barytes may be discovered by this means. In its
turn lime is distinguished from barytes by the solubility of its
sulphate. This work has been quite forgotten, and in our
time a chemist in high esteem at Berlin again takes up the
question, and supported by more recent works gives absolutely
the same solution of the problem which his countryman Rich-
ter had given so long before (Pogg., *Ann.* vol. xliv. p. 445*).

Richter finds that it is difficult to prepare very concentrated
nitric acid because of the great quantity decomposed by heat.
Now this inconvenience is remedied by using a quantity of
sulphuric acid double that necessary for decomposing the
nitre. Richter proposes another means which merits our at-
tention; he adds to the nitre one-third of its weight of per-
oxide of manganese, and the quantity of sulphuric acid neces-
sary for decomposing the two substances. He finds that the
disengagement of oxygen which accompanies the distillation
of nitric acid prevents the formation of nitrous acid.

It was already known in Richter's time that salts while
passing from the state of solution to that of crystals, gave out
heat. The same phænomenon takes place when water becomes
ice; it was therefore thought fit to indicate the analogy of the
two phænomena by saying ice of crystallization, instead of
water of crystallization, the term which had been used till then.
Richter puts the question, whether water which is found com-
bined in crystals exists in them in the state of ice or not.
This is the manner in which he succeeds in solving this in-
teresting question. He dissolved 1440 parts of crystallized

sulphate of soda ($\mathrm{Na\ \overset{..}{S}} + 10\ \mathrm{\overset{.}{H}}$); the temperature of which
was $15°\cdot55$ C. in 3405 parts of water, the temperature of which
was $76°\cdot67$ C. The solution obtained indicated a tempera-
ture of $48°\cdot96$. Supposing that the capacity of the elements
for heat remains the same, Richter finds that

$$\frac{1440 \cdot 15{\cdot}55 + 3405 \cdot 76{\cdot}67}{4845} = 58{\cdot}4$$

should be the temperature of the liquid. There is therefore
a lowering of temperature of $9°\cdot44$. He admits that the spe-
cific heat of the liquid was $0\cdot75$, and that consequently the

[* A translation of the paper (by H. Rose) here referred to will be found
in Phil. Mag. S. 3. vol. xiv. p. 78.—EDIT.]

depression of temperature observed is equivalent to that which would have been produced by the fusion of 457·4 parts of ice at 0°. But as he finds that the 1440 parts of salt employed contain, not 457 parts of solidified water, but 803 parts, he concludes thence that this water had not lost as much heat as the water should necessarily have lost in order to freeze, and that consequently it is not correct to say ice of crystallization.

Notwithstanding the depth of his views, Richter was not the less exposed to critical attacks which were often unjust. His replies were always not only moderate, but in general as calm as if he had discussed an uncontested subject. When M * * * makes me such a reproach, says he, I bear it without thinking myself injured; I merely believe that irony does not suit the end which criticism ought to have in view and which should be to convince. Besides, every one cannot follow an author step by step in order to judge with knowledge of the subject, for it is not sufficient, for this purpose, to turn over the leaves of a work. Several times in his prefaces Richter complains of not being read with attention. Thus to give an idea of the manner in which his views were treated, I will mention another critic (M. Fries) who thought, for example, that it was impossible to explain why the elements followed a fixed law in their relations of neutrality. To that Richter replies, that nature would be very poor if she were limited only to what was intelligible for him and for his criticism.

Another critic asked him with more reason to give a summary of his doctrine which might be comprehended by every one. Richter's fault was that he did not express himself clearly; if circumstances had caused him to undergo the severe discipline of the French language, if Richter, like Lavoisier, had drawn his logic from the school of Condillac, the truths which he published would have spread with more facility, and he would have produced the same results with less labour.

In the sciences, gentlemen, labour is divided into two very distinct categories; some from their novelty and the generality of their results open a new field to investigation, and spread great truths which astonish the generation which sees them originate. These works, gentlemen, make an epoch in the history of the development of intelligence, and man is hardly ever ungrateful for this benefit. Others, sometimes as difficult as the preceding ones, are but a tribute of our love for science,—a right to the esteem of our contemporaries. They pursue and extend paths already opened. They cause us to be esteemed while we live; a certain deference surrounds us: but let us not deceive ourselves; it is but the homage which politeness imposes by the fact of our presence, for after

us, a generation which passes over our grave is sufficient to cause these titles not to be remembered; the facts are quoted, the authors are forgotten.

The works of Richter, as we have seen, belong to these two distinct classes, and if it is true that a few words should suffice to sum up the entire life of a celebrated man, that of Richter is altogether summed up in these words (taken from the Wisdom of Solomon, xi. 22) which he placed as an epigraph at the head of all of his works which treat of chemical proportions:

"God made all things, in measure, and number, and weight."

XVI. *On the Extension* of* Budan's *Criterion for the Imaginary Roots, and a new Method of effecting the Separation of the nearly equal Roots of a numerical Equation.* By JAMES R. CHRISTIE, *Esq.*†

BUDAN has shown that his criterion of the presence of imaginary roots only fails when, in the pair of roots $\alpha \pm \beta \sqrt{-1}$, α is a positive proper fraction and β is less than $\cdot 5$, on account of the effect of his reciprocal transformation being that of converting these roots to the new form

$$\frac{\alpha + \beta \sqrt{-1}}{\alpha^2 + \beta^2} \text{ or } \alpha_1 \pm \beta_1 \sqrt{-1},$$

wherein α_1 must, in the failing case, be less than unity.

In the reduced reciprocal equation these roots become

$$\alpha_1 - 1 \pm \beta_1 \sqrt{-1};$$

and they may, as before, be shown to be imaginary unless β_1 be less than $\cdot 5$.

If we suppose α to be *not greater* than β, then $\frac{1}{2\beta}$ will be the least value of the fraction β_1; but β is less than $\cdot 5$, consequently this value of β_1 must exceed unity. It appears therefore that, in the case of α *not greater* than β, the condition upon which the failure of the criterion depends, ceases to exist in the roots as they appear in the first reduced reciprocal equation. The same will hold true if α does not exceed $\beta \sqrt{3}$, since the least value this condition allows for β_1 is $\cdot 5$.

Let us now see in what manner α and β enter into the second reciprocal equation.

* It is proper to mention that, in 1840, I pointed out the practical application of this method, in an example which was casually brought under my notice, to my friend and colleague Mr. Davies, who considered the then crude remark as of sufficient importance to be inserted, with the example, in his last edition of Hutton's " Course of Mathematics."— J. R. C.

† Communicated by the Author.