

X. *Analysis of Corundum, and of some of the Substances which accompany it; with Observations on the Affinities which the Earths have been supposed to have for each other, in the humid Way.* By Richard Chenevix, Esq. F. R. S. and M. R. I. A.

Read May 20, 1802.

§ I.

SOME kinds of corundum, such as the adamantine spar of China, and the sapphire, have already been analyzed by Mr. KLAPROTH. This would have rendered any further experiments unnecessary, were it not, that I have had at my disposal many kinds of corundum he did not possess, and also some substances accompanying it, which were unknown before the preceding communication of the Count de BOURNON.

As, from the result of my analyses, it appears that all the different kinds of corundum are nearly similar in their constituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I shall therefore confine myself to stating, once for all, such modes of analysis as were employed with stones of a similar nature; and then present a summary of the results: lastly, I shall conclude with an enquiry into a much contested point, which lately threatened a revolution in docimastic chemistry.

A principal character of corundum in general, as may be found in the Count de BOURNON's mineralogical description, is

U u 2

extreme hardness; and thence, the difficulty of reducing that substance into fine powder will be easily conceived. We are told by docimastic chemists, that the most advantageous method of pulverizing hard stones, is to make them red hot; and, in that state, to plunge them into cold water. But I found that this operation, when performed but once, was by no means sufficient for corundum. I therefore repeated it, till the stone appeared to be fissured in every direction. After this, the specimen to be pulverized was put into a steel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a steel pestle was very closely adjusted. A few blows upon the pestle caused the stone to crumble; and the fragments were then easily reduced into an impalpable powder, in an agate mortar, with a pestle of the same material. The abrasion from the mortar, usual in the pulverization of hard stones, was much diminished by the above precaution; rubies and sapphires being, in a short time, ground to a powder nearly as minute as the finest precipitate.

Mr. KLAPROTH, in his analysis before mentioned, had observed with how much difficulty the stones were acted upon by potash or soda. I found that the greatest heat a silver crucible could support, without melting, was not sufficient to produce a satisfactory fusion of one part of corundum, with six parts of either of those alkalis; nor did an exposure to that temperature during several hours, seem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered soluble in any acid; and what remained was the powder of the stone, wholly unchanged. The repeated filtrations and evaporations with which this treatment must be attended, not only render it tedious, but also produce

uncertainty in the results. Even when very finely powdered corundum was exposed, with six times its weight of potash, in a platina crucible, to a heat of 140° of WEDGWOOD, for two hours together, it was not acted upon in such a manner as to be fit for analysis. From all these experiments I concluded, that some more efficacious mode of rendering corundum soluble in acids was to be sought.

I boiled a great quantity of sulphuric acid upon very finely powdered corundum, in a platina crucible. But, although the acid, after a great length of time, had dissolved a little of the stone, I did not find this method more satisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were less effectual than the sulphuric. Phosphoric acid, held in fusion with corundum, did not dissolve any notable portion of that stone, or render it soluble in other acids.

I then had recourse to sub-borate of soda, (borax,) which I found to answer beyond my expectation. Two parts of that salt, calcined, and one of corundum, enter into fusion, at a temperature which I judged to be about 80° of WEDGWOOD;* and a glass, more or less coloured, is formed. This glass is soluble in muriatic acid; and, by this method, it is easy to obtain a complete solution of corundum. My general method of operating was as follows.

I took one hundred grains of corundum; and, having several times made it red hot, and plunged it into cold water, I put it into the steel mortar, and treated it as already mentioned. I then poured some very dilute muriatic acid upon it, to wash off whatever iron might have adhered, in consequence of its mechanical action upon the mortar. After it was dried and weighed,

* I have no doubt that a lower temperature would be sufficient.

I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general result. I then put the whole into a platina crucible, with 200 grains of calcined sub-borate of soda, and exposed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glass disappeared. If I wished to obtain the silica directly, I evaporated the whole to dryness; but, if otherwise, I precipitated by an alkaline carbonate, and washed the precipitate, in order to get rid of all the salts contained in the liquor. This latter mode I believe to be preferable. I then re-dissolved the precipitate in muriatic acid, and evaporated for silica. But, as corundum contains only a small portion of this earth, there was little or no appearance of jelly. When the silica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an excess of potash. By this operation, the alumina was precipitated, and then re-dissolved by the excess of potash, from which it was finally obtained by muriate of ammonia; the iron which had remained undissolved by the potash, having of course been previously separated from the alumina. This earth, and the silica, after being washed and dried, were ignited, and thus the weight of both was obtained.

I shall exemplify, in a single instance, this mode of treatment; and then present the results obtained from the different kinds of corundum. For this purpose, I shall select the blue perfect corundum, or sapphire, as the stone which has been the most ably analyzed by Mr. KLAPROTH. From a view of both analyses, the efficacy of the fusion with borax will be evident; and the results of the several experiments may be compared.

The chief difference between these proportions and those established by Mr. KLAPROTH, is in the silica. That chemist did not find any notable portion of it in the specimens he examined. This naturally induced me to make a very strict research, into every possible means by which any silica might have been introduced into the results; whether by the borax, the alkali, or any of the other re-agents I had used. But, finding very clearly, that none of these substances did contain any, I could no longer hesitate to believe, that the proportion I have here stated, was actually contained in the sapphire I analyzed. I am likewise convinced, that no more than the quantity I have mentioned was worn from the agate mortar and pestle; for my constant practice was, to weigh them, both before and after I had used them, in scales which, when charged with four pounds on each end, turn easily with the tenth part of a grain.

The general results, from all the different kinds of corundum, were as follows.

| | | | | | | | |
|--|---|---|---------|---|---|---|--------|
| <i>Blue perfect Corundum, or Sapphire.</i> | | | | <i>Red perfect Corundum, or Ruby.</i> | | | |
| Silica | - | - | 5,25 | Silica | - | - | 7 |
| Alumina | - | - | 92 | Alumina | - | - | 90 |
| Iron | - | - | 1 | Iron | - | - | 1,2 |
| Loss | - | - | 1,75 | Loss | - | - | 1,8 |
| | | | <hr/> | | | | <hr/> |
| | | | 100,00. | | | | 100,0. |
| <i>Imperfect Corundum from the Carnatic.</i> | | | | <i>Imperfect Corundum from Malabar.</i> | | | |
| Silica | - | - | 5 | Silica | - | - | 7 |
| Alumina | - | - | 91 | Alumina | - | - | 86,5 |
| Iron | - | - | 1,5 | Iron | - | - | 4 |
| Loss | - | - | 2,5 | Loss | - | - | 2,5 |
| | | | <hr/> | | | | <hr/> |
| | | | 100,0. | | | | 100,0. |

| <i>Imperfect Corundum from China.</i> | | | | <i>Imperfect Corundum from Ava.</i> | | | |
|---------------------------------------|---|---|---------|-------------------------------------|---|---|--------|
| Silica | - | - | 5,25 | Silica | - | - | 6,5 |
| Alumina | - | - | 86,50 | Alumina | - | - | 87,0 |
| Iron | - | - | 6,50 | Iron | - | - | 4,5 |
| Loss | - | - | 1,75 | Loss | - | - | 2,0 |
| | | | 100,00. | | | | 100,0. |

As I could not discover chrome, or any other colouring substance, except iron, in these stones, I can attribute their difference of colour only to the different state of oxidizement of the iron; but it is impossible to ascertain what that state may be, from so small a quantity.

The matrices of these stones, and the substances accompanying them, are more easily fused than the six kinds of corundum just mentioned. The usual and well known mode of treatment by potash, was sufficient to render these substances soluble in the acids. Since the many experiments of KLAPROTH, VAUQUELIN, and others, the mode of analyzing mineral bodies is become so familiar to chemists, that I shall mention particulars with respect to one only of the following substances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

1. A certain quantity of this matrix was reduced to powder, in the manner already described. 100 grains of it were treated with potash, in a silver crucible: they then afforded a limpid solution in muriatic acid. The liquor was evaporated; and, long before the mass was entirely dry, it had assumed the appearance of a jelly. When the saline matter in the evaporating-dish was dissolved in a slight excess of acid, a white powder remained at

Fibrolite.

| | | | | | |
|---------------------------|---|---|---|---|-------------|
| Silica | - | - | - | - | 38 |
| Alumina | - | - | - | - | 58,25 |
| A trace of iron, and loss | - | - | - | - | <u>3,75</u> |
| | | | | | 100,00. |

This is the only stone I have ever met with, that yielded nothing but silica and alumina; for the quantity of iron was so small as hardly to be taken into account. I have repeated this analysis three times; and have not found a difference of half a grain.

Thallite in Crystals, with a rough Surface.

| | | | | | |
|---------|---|---|---|---|----------|
| Silica | - | - | - | - | 45 |
| Alumina | - | - | - | - | 28 |
| Lime | - | - | - | - | 15 |
| Iron | - | - | - | - | 11 |
| Loss | - | - | - | - | <u>1</u> |
| | | | | | 100. |

Thallite in Prisms like the Tourmalin.

| | | | | | |
|---------|---|---|---|---|----------|
| Silica | - | - | - | - | 40 |
| Alumina | - | - | - | - | 25 |
| Lime | - | - | - | - | 21,5 |
| Iron | - | - | - | - | 11,5 |
| Loss | - | - | - | - | <u>2</u> |
| | | | | | 100,0. |

Thallite in Fragments, of a fine transparent Yellow Colour.

| | | | | | |
|---------|---|---|---|---|---------------|
| Silica | - | - | - | - | 42 |
| Alumina | - | - | - | - | 25,5 |
| Lime | - | - | - | - | 16 |
| Iron | - | - | - | - | 14 |
| Loss | - | - | - | - | 2,5 |
| | | | | | <u>100,0.</u> |

Fibrolite accompanying the Matrix of Corundum from China.

| | | | | | |
|---------|---|---|---|---|-------------|
| Silica | - | - | - | - | 38 |
| Alumina | - | - | - | - | 46 |
| Iron | - | - | - | - | 13 |
| Loss | - | - | - | - | 3 |
| | | | | | <u>100.</u> |

Felspar from the Sand of Ceylon.

| | | | | | |
|---------|---|---|---|---|---------------|
| Silica | - | - | - | - | 68,5 |
| Alumina | - | - | - | - | 20,5 |
| Lime | - | - | - | - | 7 |
| Iron | - | - | - | - | 1,5 |
| Loss | - | - | - | - | 2,5 |
| | | | | | <u>100,0.</u> |

As the greater part of the above substances were fusible without difficulty in potash, I preferred using a silver crucible to any other. It may be laid down as a general rule, with respect to delicate experiments, that in the treatment of metallic substances, we should not use metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The easily oxidizable metals cannot be employed; but silver and platina present advantages which no other metals seem to possess.

Theory would certainly give a general preference to platina, from its resistance both to heat and to acids; and practice will justify this preference, in all but a single instance. If a quantity of potash be kept for some time in fusion, in a platina crucible, it will be found that the crucible has lost several grains of its weight. The platina so dissolved may be looked for in the potash; and, if this be saturated with muriatic acid, and evaporated, we shall find the well-known triple salt, formed by the combination of muriatic acid with potash and oxide of platina. This action of potash upon platina, does not depend upon any mechanical cause, such as friction, the force that determines it being purely chemical. If a salt formed by potash, or a salt formed by ammonia, be mixed with a salt of platina, a precipitate ensues, which is a triple salt; and it is by this method, that the Spanish government detects the platina, in the ingots of gold sent from their American possessions. It is therefore evident, that an affinity does exist between potash and platina, in a certain state; and I imagine it to be this affinity, which causes the oxidizement of the platina, when potash is kept in fusion upon that metal. I must however observe, that my crucible was prepared by JANETTY, in Paris, according to a method he has published in the *Annales de Chimie*; and that he always employs arsenic, a little of which certainly remains united to the platina. What influence arsenic may have, remains to be determined. Soda does not form a triple salt with the oxide of platina; for I have frequently kept this alkali in fusion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact seems to corroborate my assertion, that the affinity of potash for oxide of platina, determines the oxidizement of the metal.

Whenever I suspected that platina had been dissolved, I could easily detect the smallest portion of it. A solution of platina, so dilute as to be nearly colourless, manifests, in a very short time, the colour of a much more concentrate solution, and becomes reddish, by the addition of a solution of tin in muriatic acid. This I have found to be, by many degrees, the most sensible test for platina; and it would answer the purposes of the Spanish government, much better than that they usually employ.

The alkalis have no immediate action upon silver; but I have observed, that crucibles of this metal, after they have been a long time in use, become somewhat more brittle than they were before.

Potash and soda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are so. But *fixed* is an absolute term, and cannot admit of degrees. If potash, such as we obtain from Mr. BERTHOLLET'S method of preparing it, be kept in fusion at a very strong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is usually produced by alkalis. Indeed, in preparing Mr. BERTHOLLET'S potash, the vapour of the alkali may be easily perceived. Soda is not quite so volatile; though far from being fixed. It appears also, that a little water increases the volatility of both potash and soda, as happens with boracic acid. This volatility of potash, has been advantageously applied of late to the art of bleaching.

§ II.

On the Affinities the Earths have been supposed to have for each other, in the humid way.

In the course of the foregoing analysis, I had occasion to make some further observations concerning a subject upon which I had been formerly engaged, namely, on the affinities the earths have been supposed to have for each other, when held in solution by acid or alkaline menstrea.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I published a paper upon the analysis of some magnesian stones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magnesia, of alumina for lime, and of alumina for silica. In the XXXIst volume, page 246, there is a memoir, by GUYTON DE MORVEAU, upon a similar subject;* and he there reports some experiments of his own, by which he was induced to think, that the earths do really possess a chemical attraction for one another. Since that time, the affinity of the earths has been received among chemists as an undoubted fact; and, at the end of Mr. KIRWAN'S *Essay on the Analysis of mineral Waters*, we find a list of earthy salts which produce a reaction upon one another, supposed to be caused by an affinity that tends to unite their bases, in the form of a precipitate, insoluble in the acids. Some other detached observations are to be found, in the *Journal de Physique*, and in the *Annales de Chimie*. The fact is certainly one of the most important in the docimastic art, and merits all the attention of the skilful in that branch.

In the XLth volume of the *Annales de Chimie*, page 52,

* He has taken no notice of any of the experiments contained in my paper.

DARRACQ has published a paper, intended as a refutation of the conclusions drawn by GUYTON. I had myself repeated the greater part of the experiments of the latter; and the results I obtained were exactly similar to those of DARRACQ. In fact, I had intended to continue the researches; but the very satisfactory paper of DARRACQ, appeared to me to render a further prosecution of them totally useless. However, a paragraph inserted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which GUYTON appears to be the author, shows that he has not derived from the Memoir of DARRACQ, that conviction which it certainly conveys. The paragraph in question is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. BABINGTON, dated December 17, 1800, and inserted in the IVth volume of NICHOLSON'S Journal, page 511. This letter contains an opinion which deserves to be canvassed, as it is not perfectly just; and the use GUYTON has made of it, has determined me to add my observations to those of DARRACQ.

I shall follow the order of GUYTON'S experiments, in the enumeration of those I made.

Exp. 1. From a mixture of lime-water and barytes-water, GUYTON obtained a precipitate. I obtained none.

Exp. 2. A solution of alumina in potash, mixed with a solution of silica in the same, gave a precipitate, after standing some time. This had been observed by DARRACQ, and by GUYTON, and agrees perfectly with the affinity which, before GUYTON published his paper, I had asserted to exist between these two earths.

Exp. 3, 4, 5. Lime-water, strontia-water, and barytes-water, produce a somewhat similar effect upon a solution of silica in potash.

Exp. 6. No precipitate took place from a mixture of barytes-water and strontia-water; nor from solutions of the carbonates of those earths, in water impregnated with carbonic acid.

Exp. 7. GUYTON obtained a precipitate, by mixing solutions of muriate of lime and muriate of alumina. I could not obtain any.

Exp. 8. Solutions of muriate of lime and muriate of magnesia, when mixed, did not afford a precipitate.

Exp. 9. Muriate of barytes did not, as GUYTON has asserted, form a precipitate with muriate of lime. He was right in saying, that muriate of strontia gave no precipitate with muriate of lime.

Exp. 10. Muriate of magnesia and of alumina, afforded me no precipitate. GUYTON says, that the liquors became milky.

Exp. 11. Muriate of magnesia, whether mixed with muriate of barytes or of strontia, afforded me no change; although GUYTON says he obtained an abundant precipitate, by mixing muriate of magnesia with muriate of barytes.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. GUYTON asserts, that there is a precipitate in this case.

Exp. 13. Muriate of barytes and of strontia, did not form a precipitate. GUYTON has remarked the same.

Exp. 14. From muriate of strontia and of alumina, I obtained no precipitate. With GUYTON the liquor became milky.

From all these experiments it appears very clearly, that GUYTON has pronounced too hastily, upon the affinity which he supposes barytes to entertain for lime, for magnesia, and for alumina; and that he is equally in the wrong, with regard to the affinity of strontia and alumina. With regard to *Exp.* 3, 4, and 5, although they appear to be true, yet it would require the

respective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid also, which must of course combine with the potash, during the treatment of the silica by that alkali, should be taken into account, in considering the cause of the precipitate.

The solutions which I used, of all the above salts, were in the most concentrate state; therefore, in the state most favourable for showing precipitation, if any had taken place.

It is not very difficult to account for the appearances that deceived Mr. GUYTON in his experiments, and for the cause that produced them. In one instance, he obtained a precipitate from muriate of lime and of alumina, because, in all probability, the alumina he dissolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a small portion of sulphuric acid.* In the next place, it is very likely that his solutions were sufficiently concentrate to give a precipitate of sulphate of lime. The same was the case with regard to his mixture of muriate of strontia with muriate of alumina. As to the general conclusion, that barytes has an affinity for lime, magnesia, and alumina, which strontia does not appear to possess, it is to be explained as follows. Lime often contains a little sulphate of lime. Mr. GUYTON'S magnesia, as well as his alumina, had probably been obtained from the sulphate; and we are indebted to Mr. BERTHOLLET, for the true nature of many similar precipitates.

* It is somewhat singular, that GUYTON should have observed this fact elsewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic salt, by alumina prepared from the sulphate of this earth, had been observed by SCHEELÉ, in his *Essay on the Affinities of Bodies*. But that great chemist referred the phenomenon to its right cause, viz. to some sulphuric acid remaining in all alumina thus prepared.

Barytes is a much more delicate test than strontia, for sulphuric acid; and, therefore, barytic solutions were affected by quantities of sulphuric acid, which strontia could not render sensible. This I have ascertained to be the case: for I have obtained copious precipitates, by barytes, in a liquor composed for the purpose, wherein strontia did not produce the smallest cloud, or show the presence of sulphuric acid.

A little care and attention are necessary, in preparing the earths, which are to be dissolved in the muriatic acid, for these experiments; and, if Mr. GUYTON had taken the requisite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from sulphuric acid; and, if this be obtained, there is not the smallest precipitate or cloud, in any of the cases I have mentioned. If any further proof be necessary, with regard to the cause of precipitates obtained in the manner stated by Mr. GUYTON, I may add, that I have repeated his experiments, and have always found the precipitates to be sulphate of barytes.

The general conclusion to be drawn from the observations of Mr. KIRWAN, already alluded to, is, that barytes has an affinity for lime, magnesia, and alumina, upon which earths strontia does not seem to have any influence. But these mistakes are to be accounted for in the same manner as those of Mr. GUYTON, viz. by sulphate of barytes being much less soluble than sulphate of strontia, and therefore showing the presence of a smaller portion of sulphuric acid, or, in other words, being a much more delicate test for that substance.

With regard to the letter already mentioned as being inserted in NICHOLSON'S Journal, and which drew some reflections from

Mr. GUYTON, it is necessary to examine as much of it as may be thought objectionable.

The author says, that he repeated the experiments of Mr. GUYTON, with an alkaline solution of silica and alumina, and that he obtained a precipitate; which precipitate, though containing silica, was totally soluble in the acids. "Here," he says, "the properties of the silex must be considerably altered. This must render all analysis with alkalis suspicious; and shows on what fallacious grounds the proud dominion of chemistry rests, which she has exercised so long, in such an arbitrary and overbearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretensions of chemistry; for the very circumstance of rendering silica soluble in the acids, is one of the discoveries that has most contributed to render certain, and to extend, our knowledge of analysis. No earthy substance is now thought fit to be submitted to further experiment, till a complete solution of it in an acid be first obtained; and, when that solution cannot be effected directly by the acid, it is always attempted by previous fusion with an alkali. This mode of rendering silica soluble in acids, is no new discovery; it has been long known; and the analysis of minerals has never been brought so near to truth, as since it has become an indispensable condition.

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline solution of silica and alumina. Alumina indeed appears to exercise an attraction, as I before stated, for silica, for magnesia, and for lime. All stones in which there is but little alumina, and a great quantity of silica, leave, after fusion with potash, a light and flocculent substance, which

cannot be dissolved by the acids: this substance, however, which is silica, has been in solution in the alkali. But, if a greater proportion of alumina be present, none of this flocculent precipitate appears; hence it is evident, that alumina must determine its solution. Its easy solubility, in the latter case, cannot depend upon the division of the particles of the silica in the stone; for, in the first place, after being fused with potash, the tenuity of the particles of every stone must be nearly the same; and, in the next place, I have not observed, that any earth, except alumina, can promote the chemical solution of the silica, though they must all occasion its mechanical division.

As to the affinity of alumina for magnesia, it is by much the most powerful of all those which any of the earths have for each other. I attempted to precipitate magnesia from muriatic acid, by ammonia, even in excess; but found that the whole muriate of magnesia had not been decomposed, and that a triple salt, or an ammoniacal muriate of magnesia,* had been formed. I then poured an excess of ammonia into a solution of muriate of magnesia, mixed with a large proportion of a solution of muriate of alumina. All the earth was precipitated; and nothing remained in solution, except muriate of ammonia. The liquor was then filtered, and the precipitate washed and dried. I dissolved it in muriatic acid, and boiled it with a great excess of potash. Some alumina was taken up, but by no means all the quantity that had been used. The precipitate which had resisted the action of potash, was again dissolved in muriatic acid, and precipitated by carbonate of potash. The carbonate of magnesia was held in solution by the excess of carbonic acid; and, by using potash and carbonic acid alternately, (the first to

* This salt is well known in chemistry.

dissolve alumina, the second to dissolve carbonate of magnesia,) I effected a separation of the earths. These experiments show, that there is an affinity between alumina and magnesia, and a certain point of saturation, where the action of potash upon alumina is wholly counteracted by the affinity of that earth for magnesia.

When a solution of potash is boiled upon a mixture of lime and alumina, the alumina is dissolved, together with a much greater portion of lime than can be attributed to the dissolving power of the water alone. But, if a solution of potash be boiled upon lime, without alumina, no more lime is taken up than would have been dissolved by an equal quantity of water not containing potash in solution; consequently, alumina seems really to promote the solution of lime in potash. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has since been noticed by Mr. VAUQUELIN.*

If the conclusions of Mr. GUYTON had been well founded, it would have been chemically impossible to arrive at truth in analysis. There were already real difficulties enough to be overcome; and Mr. BERTHOLLET has lately discovered some, which are not so easily answered as those I have just considered. The position of this chemist, however, has been too generally extended by him. If the power of masses were as great as he represents it to be, and if it increased *ad infinitum*, in proportion to the mass, it must follow, that, with any given substance, we could decompose any compound, provided the mass of the decomposing body were sufficiently great; but this is well known not to be the case.

* SCHEELLE was, in fact, the first who perceived this affinity. See his *Essay on Silica, Clay, and Alumina*.

From the experiments which I have related, it appears to be proved.

1st. That there exists an affinity between silica and alumina.

2dly. That there exists a very powerful affinity between alumina and magnesia.

3dly. That alumina shews an affinity for lime; but that the said affinity is not so strong as Mr. GUYTON had supposed, nor, if pure reagents be used, is it to be perceived under the circumstances stated by him.

4thly. That Mr. GUYTON was mistaken in every instance of affinity between the earths, excepting in the case of silica with alumina, which had been observed before his experiments; and that, in the other cases, he has attributed to a cause which does not exist, phenomena that must have resulted from the impurity of his reagents.

5thly. That neither the experiments of Mr. GUYTON, nor the opinion maintained in the letter from Freyberg, are sufficient to diminish, in any degree, the value of the assistance mineralogy derives from chemical investigation.