**Essays of Jean Rey, doctor of medicine, on an enquiry into the cause wherefore tin and lead increase in weight on calcination. (1630).**

# THE INCREASE

IN

# WEIGHT OF TIN AND LEAD

ON

# CALCINATION.

BY

JEAN REY

(1630.)

Alembic Club Reprints,
No. 11.

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# ESSAYS OF JEAN REY

DOCTOR OF MEDICINE

## ON AN ENQUIRY INTO THE CAUSE WHEREFORE TIN AND LEAD INCREASE IN WEIGHT ON CALCINATION

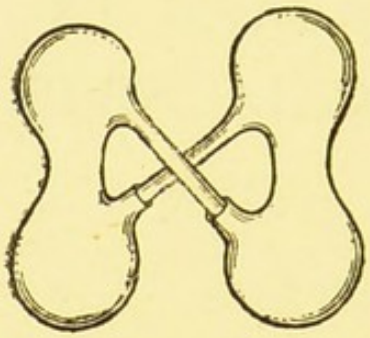
(1630)

Edinburgh:
PUBLISHED BY THE ALEMBIC CLUB

*Edinburgh Agent:*
WILLIAM F. CLAY, 18 TEVIOT PLACE

*London Agents:*
SIMPKIN, MARSHALL, HAMILTON, KENT & CO., LTD.

1904



# PREFACE.

THE title of the work which has here been translated is "*Essays de Jean Rey, docteur en medecine, sur la Recherche de la cause pour laquelle l'Estain et le Plomb augmentent de poids quand on les calcine.*" It was first published in pamphlet form at Bazas in 1630, and reprinted, along with notes and various letters bearing on the subject, at Paris in 1777. In these Essays Rey attributed the increase in weight of tin and other metals on calcination to the action of the air, thus to some extent anticipating the work of Lavoisier a century and a half later. He evidently well understood the value of properly chosen experiments, although he himself seems to have been little disposed to their performance, delighting rather in metaphysical speculations and curiously inadequate trains of reasoning, which yet lead often enough to just conclusions. Apart altogether from the historical interest of the work, the quaint and lively style of the author makes his Essays still worthy of perusal in the original.

J. W.

# ESSAYS OF JEAN REY,

DOCTOR OF MEDICINE,

## ON AN ENQUIRY INTO THE CAUSE WHEREFORE TIN AND LEAD INCREASE IN WEIGHT ON CALCINATION.

### PREFACE.

Some eminent personages having observed with astonishment that tin and lead increase in weight when they are calcined, have been seized with a praiseworthy desire to enquire into the cause of this phenomenon. It has proved a goodly subject, but the enquiry has been troublesome, and its fruits very small: in so far that after having directed their thoughts to all quarters, they have only been able to adduce reasons so feeble that no man of sound judgment dares trust them for support, or by their aid shelter his mind from all doubt. The sieur Brun, Master Apothecary in Bergerac, having lately paid attention to this augmentation, and believing, as I apprehend, that no one before him had been aware of it, has invited me in one of his letters to enter on this line of thought and furnish him with the cause. Now, since he is a person whose integrity of life, rare experience in his art, and other patent virtues, oblige every honest man to wish him well, I confess that these qualities have had such

power over my affections, that I cannot deny him this request. At his prayer, therefore, and friendly solicitation, I have devoted several hours to the question, and conceiving to have hit the mark I produce from them these Essays of mine. Not without foreseeing very clearly that I shall incur at first the accusation of temerity, since in them I run counter to sundry maxims approved for many centuries by the majority of philosophers. But what temerity can there be in exposing the truth to light of day when one has known it? Might I not more justly be reputed childishly fearful if I dared not divulge it, and sordidly envious if I held it concealed? Of the last two accusations I clear myself, hoping to see myself freed of the first by all intelligent persons, who, after having tasted of my reasons, will, if they find them to their liking, be grateful to me for having set them forth: and, if they like them not, will not refrain from praising my efforts to seek the truth in so arduous a question, and will be incited by my example to treat the matter more dextrously, to which I invite them. In any case I shall have testified to the public the desire I have of serving it by letting this paper slip from my hands, although it may be that some hurtful stigma will thereby be graven on my reputation.

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## ESSAY I.

### *That all things material under the canopy of Heaven are heavy.*

God, in creating the Universe, has made it neither wholly like himself nor wholly unlike: for, himself being One, he has made the world as it were not-One, by the diverse multiplicity of its innumerable parts; willing at the same time that they should reunite to a certain oneness by their exact contiguity. The superior world is

beyond our subject: the inferior and elementary world owes this contiguity to the heaviness divinely impressed in all its parts, assisted by the subtle fluidity of some of its simple bodies. It is through this quality, with which the matter of the four elements is more or less invested, that they are separated from each other and kept each in his own place, according to the requirements of the generation of mixtures and the adornment of the universe. For this matter, filling at every point the space enclosed beneath the vault of heaven, is continually urged by its own weight towards the centre. It is true that the earth as heaviest promptly occupies this place, and forces his brethren to retreat, so that water, being second in heaviness, is also second in place: the air then, being driven from the first and second place, is restricted to the third, leaving to fire, the least heavy of all, the uppermost region for his dwelling. Chemists furnish us with an agreeable representation of this when they take pulverised black enamel, liquor of tartar, *aqua vitæ* coloured bluish with litmus, and spirit of turpentine reddened with alkanet; and, casting the whole into a phial, agitate it until a confused mixture results. The vessel being then left in repose, the eye perceives with pleasure the process of disentanglement. The enamel reaches the bottom, representing to us the earth. The liquor of tartar is its neighbour, representing water. The *aqua vitæ*, resembling the air, occupies the third place. And the spirit of turpentine, to demonstrate the fire, assumes the highest position. All this occurs in virtue of heaviness, according as it is liberally or sparingly distributed in these bodies. In the same way the elements can acknowledge no other cause to arrange and dispose them in their places, there being no need to introduce the lightness which our predecessors have vainly excogitated to this end.

## ESSAY II.

### *There is nothing light in Nature.*

Almost all philosophers, ancient as well as modern, fearing an eternal confusion of the elements if they were all endowed with heaviness, have been led to adopt the belief that the two superior elements are equipped with a certain lightness whereby they raise themselves upwards to occupy each his place; and that the two lower elements are dragged downwards by their own heaviness. But having in the preceding Essay clearly shown that there is no need of lightness to effect this, heaviness alone being sufficient, I embrace the maxim which they themselves have very prudently put forward, "That we must never multiply the causes of things without necessity," and holding as assured that God and Nature do nothing in vain (as they also teach us), I believe that it would be otherwise if lightness were admitted, since it is superfluous. But that is not all. Fire is by nature so subtle that it scarcely merits the name of substance, and is consequently denuded of wellnigh all power of resistance, whence it follows that the air mounting upwards without hindrance would reach the heavens, expelling the fire from its place, and constraining it to seek a lower seat, to the detriment of their own doctrine. To this I add another difficulty, namely, the perpetual and fruitless strife which would exist betwixt the heavy and the light elements, the latter pulling upwards and the former downwards with all their strength. From this would arise at the place of their contact a state of tension incomparably greater than that of a cord pulled at both ends by two powerful hands, imposing upon it such a strain that rupture is seen to ensue. Such a state is very far removed from that bond of friendship by which Nature has sought to join neighbouring elements, em-

planting in their bosoms similar qualities, by means of which they communicate amongst themselves and ever amicably agree together. Whence it results that lightness is a vocable which signifies nothing absolute in nature, so that we must reject it, or, if we retain it, do so to denote solely a relation of one thing less heavy to another which is heavier.

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## ESSAY III.

### *There is no upward movement that is natural.*

What shadows would become if there were no substance is that which a natural movement upwards becomes when lightness is abolished. For in truth it would be a very monstrous thing to see natural effects which had no cause in Nature. We say that a thing moves naturally when it has the cause of its movement within itself. Now casting my eyes on every thing that moves, I find none that moves upwards of its own motion. It is true that water rises if we throw earth into the vessel that contains it; but all will admit that this is in virtue of no lightness residing in itself, but that the earth by falling down causes the water to rise. If water does not own lightness as the cause of this upward movement, why should air do so, taking the same road when water sinks in it? And why should fire when doing the same? I doubt not it will be said that, if the upward motion of the elements be not natural, it must be confessed violent, whence will follow the absurdity of seeing each of them keep its station in the universe by violence. To which I answer that the elements not having in themselves the cause of such movements, these latter may be said in that respect to be violent, but theirs is a gentle violence and in nowise destructive. Thus the motion of the spheres of the planets from East to West, having its

cause in a higher sphere, is called by us violent, although it does them no injury. Besides, those who speak thus will be their own critics, being constrained to admit not only the violent motion but even the violent rest of Water and of Air—the latter under the Fire, and the former above the Earth. Having thus banished lightness and its upward movement from the whole pale of nature, let us establish more firmly heaviness in the elements Air and Fire, which alone are in controversy.

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## ESSAY IV.

### *That Air and Fire are heavy, and move naturally downwards.*

If we had as free commerce with the element Fire as we have with Air we should not doubtless be so destitute of experiments to confirm our assertion that those which we produce with the latter, are conclusive for the former, in consequence of the proximity of their nature. Now since we are agreed that all which sinks downwards without constraint possesses heaviness, whence the movement proceeds, who shall deny this quality to the Air, seeing that as soon as we have dragged a stake out of the ground, it rushes into the hole to fill it? And that we cannot dig a well so deep that the air does not incontinently move thither, without external force, and without violence? I will go further: If there were a tube extending from the centre of the earth far upwards into the region of fire, open at both ends and filled with the four elements, each in its ordinary position, then if we withdrew the earth from beneath, the water would descend to occupy its place, leaving its own to the air, whose place would then be occupied by the fire. Then removing the water from the new position, air would fill it: and this being also emptied, fire would move thither, and

fill the whole tube, sinking down to the centre, simply because what formerly hindered it from so doing had been removed. Those who will say that this takes place in order that a vacuum may be avoided, will not say much : they will indicate the final cause, and we are concerned with the efficient cause, which cannot be a void. For it is quite certain that in the bounds of nature, a vacuum, which is nothing, can find no place. There is no power in nature which from nothing could have made the universe, and none which could reduce the universe to nothing: that requires the same virtue. Now the matter would be otherwise if there could be a vacuum. For if it could be here, it could also be there: and being here and there, why not elsewhere? and why not everywhere? Thus the universe could reach annihilation by its own forces; but to him alone who could make it, is due the glory of being able to compass its destruction. Now if a void cannot exist, how can it cause air and fire to sink downwards, contrary to their nature? Does not a real effect ever proceed from a really subsisting cause? Let us therefore say what is the truth, that it is heaviness which carries these elements downwards, that it may unite closely all their particles, and consequently close all avenues against a vacuum.

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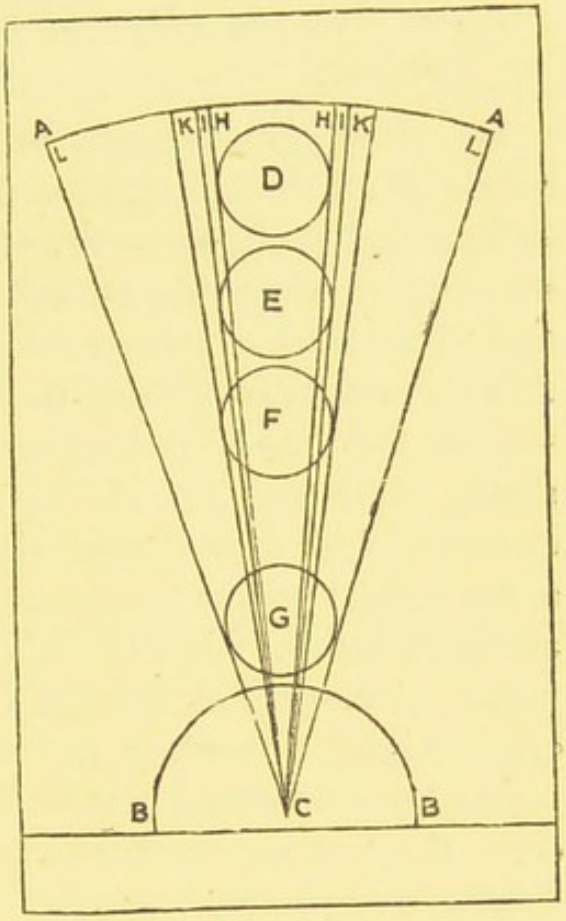
## ESSAY V.

*It is shown that Air and Fire possess heaviness by the swiftness of motion of heavy things being greater at the end than at the beginning.*

An error, however trivial, committed at the commencement of any discipline, increases as we progress, and most often involves us in very thorny difficulties. This we experience in the present subject: for Philosophers having gone astray almost at the very threshold of natural science,

attributing lightness and a natural upward movement to the two superior elements, see themselves afterwards greatly embarrassed to adduce the reason why the natural downward movement of heavy things is swifter at the end than at the beginning. The variety of opinions which we find amongst authors as to this question bears sufficient witness to their perplexity: aiming, as I do, at brevity, it is not my intention to reproduce them here. Whoever will, may read a good number in Pererius, a judicious philosopher, in his book on the natural Principles, where he reports and learnedly refutes them, embracing one, to which he protests he will adhere until he find a better. I will say something of this hereafter in passing, it being not so much true as plausible. Here I offer mine, which I have just excogitated in favour of the truth of the preceding demonstrations. The swiftness of movement of the heavy body increases from the beginning to the end by the augmentation of the elementary matter which weighs down on it, and by the continual multiplication of the shock it produces on it in descending. The demonstration will lend clearness to my statement. Let AA. be the heavens, BB. the earth, and C. its centre; D. an iron ball descending towards the earth; E. the same sunk lower down; F. still the same at the middle of its descent, G. the same near the end. HH. two lines drawn from the centre of the earth to the heavens, touching the ball D. at the two extremities of its diameter. II. two other lines drawn in the same way, touching the ball at E. KK. two other lines touching it at F. LL. two more touching it at G. It is manifest that the ball when at D., besides having its internal heaviness, has on it the matter of the elements of air and fire enclosed between the lines HH.; but being at E. there is then all the matter enclosed between the lines II., which is seen to be augmented at F. by what

the lines KK. contain in addition: and when the ball is at G., everything contained within the lines LL. weighs on it: from which it follows that the speed of the motion must increase, and besides this there is the shock which this matter makes continually as it sinks on the said ball.




The opinion of Pererius has something approaching this shock: for he will have it that the air which follows, pushes the bullet: but in this he is mistaken, in that air being inherently light and naturally striving upwards could not push the bullet downwards; any more than a boat which is hauled against the current of a river is pushed up-stream by the water, which, as it meets the prow, is diverted, and, licking the sides, flows steadily downwards: for how could it, keeping this direction, strike the stern upwards? The other part of his statement is no whit better, for he imagines that the air agitated by the motion yields more readily to the thing moved. It is quite the contrary; for air and water when agitated sustain a greater weight. Ashes are suspended in water and feathers in air when these are agitated, and sink to the bottom when they are at rest. For this reason therefore the movement should be slower towards the end, the agitation being greater.

## ESSAY VI.

*Heaviness is so closely united to the primary matter of the elements, that when these are changed one into the other they always retain the same weight.*

My chief care hitherto has been to impress on the minds of all the persuasion that air is heavy, inasmuch as from it I propose to derive the increase in weight of tin and lead when they are calcined. But before showing how that comes to pass, I must make this observation—that the weight of a thing may be examined in two ways, viz. by the aid of reason, or with the balance. It is reason which has led me to discover weight in all the elements, and it is reason which now leads me to give a flat denial to that erroneous maxim which has been current since the birth of Philosophy—that the elements mutually undergoing change, one into the other, lose or gain weight, according as in changing they become rarefied or condensed. With the arms of reason I boldly enter the lists to combat this error, and to sustain that weight is so closely united to the primary matter of the elements that they can never be deprived of it. The weight with which each portion of matter was endued at the cradle, will be carried by it to the grave. In whatever place, in whatever form, to whatever volume it may be reduced, the same weight always persists. But not presuming that my statements are on a parity with those of Pythagoras, so that it suffices to have advanced them, I support them with a demonstration which, as I conceive, all men of sense will accept. Let there be taken a portion of earth which shall have in it the smallest possible weight, beyond which no weight can subsist: let this earth be converted into water by the means known and practised by nature: it is evident that this

water will have weight, since all water must have it, and this weight will either be greater than that of the earth, or less than it, or else equal to it. My opponents will not say that it is greater, for they profess the contrary, and I also am of their opinion: smaller it cannot be, since we took the smallest weight that can exist: there remains then only the case that the two are equal, which I undertook to prove. What is shown of this particle may be shown of two, three, or a very great number—in short, of all the element, which is composed of nothing else. The same proof may be extended to the conversion of water into air, of air into fire; and, conversely, of the last of these into the first.

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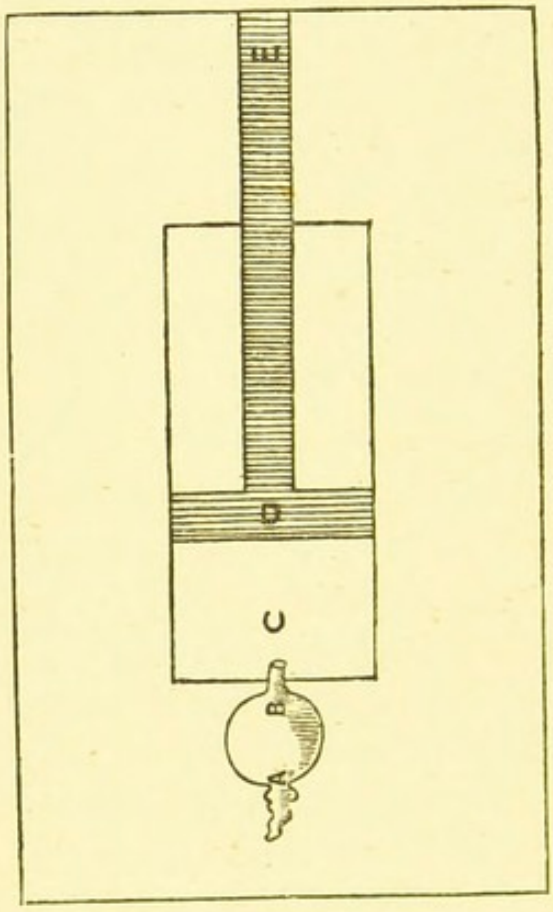
## ESSAY VII.

### *A means of knowing what volume of air is produced from a certain quantity of water.*

Philosophers have often spoken of the extension which a solid element acquires on changing into one more rare, and have endeavoured to assign its proportion: but I have no recollection to have read anything supported by a valid reason or experiment. Now, since in the preceding essay I have spoken of that ampliation, the knowledge of which opens the door to many beautiful and admirable artifices, I will not deprive the curious reader of a means which I have excogitated to bring this to the proof and know with certainty to what volume a certain quantity of water will expand on being transmuted into air; which means can be made of service and fitly applied to the other elements. Let there be constructed a wide tube of brass of suitable dimensions, well polished within, quite open at one end and closed at the other save for a very small aperture in the middle: let there be placed in it a plug such as that of a syringe, which can

slide everywhere with ease, and is so truly fitted that no air escapes. This being pushed to the bottom, let there be applied to the small aperture and tightly fastened into it a tube issuing from an Æolipyle or philosophic bellows. Let this, filled with water, be placed on a fire. The water being rarefied and transmuted into air will then issue from the small orifice, and entering the wide tube will gradually push along the plug, seeking its liberty, until all the water has been converted into air.




The volume of the tube and of the Æolipyle that is filled by it will show the expansion which this matter has experienced. If anyone wishes to know the same more easily, though not so exactly, let him take all the bowels of a pig or other animal after having cleansed them; and, having made them flat and empty of air, let him put them into a vessel full of water closed tightly by a lid with a little hole at the top to allow the water to escape: one end of the said bowels projecting through the vessel by another hole and being attached to the tube issuing from the Æolipyle, which, filled with water and set upon the fire, will blow into the gut the air into which the water will be converted. As the intestine swells, the water in the vessel will escape through the small hole in the lid, and, when collected, will show the space occupied

by the air in the intestine, to which if we add the volume of the Æolipyle we shall obtain what we desired. To these well-assured means I add the following, which seems not unpromising, to convert air into water, and ascertain the diminution of volume. Let the hole in the wide tube above-mentioned be closed, and push in the plug with great force as far as the compression of the enclosed air will permit; and, it being arrested there, lest it should recoil, let the whole instrument be exposed to a freezing atmosphere during an entire night. The air compressed within it will freeze or turn into water, only leaving the volume of the air which can remain free. By measuring the water or the ice, we shall be able to judge of the diminution. I have not made these experiments: if some enquirer forestall me, I beg of him to advise me of it as only recompense of having shown him the method, in order that I may be spared that trouble.

A. shows the Æolipyle. B. the tube issuing from it and entering the wide tube C. D. the plug which moves within it. E. the handle for pushing in and withdrawing the plug.

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## ESSAY VIII.

### *No element exhibits weight in itself, and why.*

I retrace my steps, and affirm that the examination of weights which is made by the balance differs greatly from that which is made by the reason. The latter is only employed by the judicious: the former can be practised by the veriest clown. The latter is always exact: the former is seldom without deception. The latter is attached to no circumstance of place; the former is commonly exercised only in the air, and occasionally, with difficulty, in water. It is from this that the error I have combated (that air is without weight) draws an argument which may dazzle feeble eyes, though not clear-

seeing ones. For, balancing air in air itself, and finding no weight in it, they believed that it had none. But let them balance water (which they believe to be heavy) in water itself, and they will find no weight in it either: the fact being that no element shows weight when weighed in itself. Everything that exhibits weight in air, everything that exhibits weight in water, must in an equal volume contain more weight (on account of the excess of matter) than the air, or the water, in which the equilibrium is made. From this I now deduce the cause, which few have perceived. Whatever shows weight in air (and of water the same may be said) cleaves it, thrusts it aside and forces it to give way, so that it may sink to the ground. This is called exercising its forces and its action in the air. Now it is the case that no agent acts in a medium like itself, all action presupposing some contrariety. Heat will never act on another equal heat, but the two heats will embrace and unite their actions, and by this union become a single agent instead of two as at first. If a great heat act on a lesser heat, it is because there is here a dissimilarity, and in some measure a contrariety, the lesser heat taking the title of cold when compared with the greater heat. Thus air cannot act by heaviness in air equally heavy: these two airs unite rather and make one weight. But whatever is heavier than air, by the dissimilarity and nascent contrariety of this excess or defect, will act in it, cleaving it, putting it aside, and making way for itself through it to sink downwards. And if air does not show its weight in air itself, on account of the equality of their heavinesses, with greater reason will it not show it in water, which is heavier. For even if it be placed under the water, it will not sink lower; the weight of the water which is upon it only constraining it to seek a higher place, not permitting it to abide beneath.

## ESSAY IX.

### *Air is rendered heavy by admixture of matter heavier than itself.*

It is my design to show that it is the air that mixes with the calx of tin and of lead on calcination which augments its weight, and this would be impossible if I did not remove a somewhat grave difficulty which here presents itself. For one might ask, how can what I say take place, since the examination of the weight is made by means of the balance and in air, in which air can have no weight according to the doctrine deduced in the preceding Essay? To remove this doubt I say that air in its parts can be altered and increased in weight, so that the parts thus changed and made heavier, when balanced in pure air, will make their heaviness visible. But what is this change which causes the increase in heaviness? I remark that it may happen in three distinct ways, namely by the admixture of some heavier foreign matter, by the compression of its parts, and by the separation of its less heavy portions. In the first place let us consider the first mode, and then the two others. It is certain that air is susceptible of several matters heavier than itself: such are the vapours and exhalations proceeding from the water or from the earth. A portion saturated with these matters will weigh more than an equal portion of another air which has nothing of the kind in it. Thus sea water weighs more than the fresh water from rivers, the former containing much salt, from which the latter is free. Note, I pray you, how, in foggy weather, when your high windows are first opened, the air laden with mists enters into your chamber. Do you not conclude that the former weighs more than the latter, since it cleaves it and sinks in it? Fill a globe with this nebulous air: it will weigh more than the same filled

with an air pure and without admixture. This experiment is in harmony with reason which states, If to two equal weights we add two unequal weights, the two weights will no longer be equal, and that will be the greater to which the greater weight has been added. If therefore we take for example two portions of the same air, each containing ten cubic inches of volume, and to one be added two inches of water, and to the other two inches of air, who does not see that these two portions will be equal in volume but not equal in weight, and that the one which has the water in it will be heavier? This is so manifest that I abstain from saying more concerning it, seeing, besides, that the increase in heaviness brought about in this way is of little moment to our subject. So let us pass to the others.

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## ESSAY X.

### *That air is rendered heavy by the compression of its parts.*

The second fashion by which air increases in weight is the compression of its parts: for nature has willed, for reasons known to herself, that the elements may extend or contract up to certain limits which she has prescribed to them. In this space we see a portion of an element, now closely compressed, now extended spaciously. See this pot half-full of water, under which the cook is about to make a good fire: the water will dilate until it spills over the edge; but, the fire being extinguished, it will shrink, and return to its first condition. Take this syringe in which the plug is sunk half way, and the hole in front well stopped up: push forcibly and you will reduce the enclosed air to a small bulk. Draw the plug towards you, you will not draw it out altogether, but you will extend the air to more ample dimensions than it

formerly possessed. The air being thus compressed, do you doubt that it weighs more than a free air, since in an equal space it contains more matter? If the reason given above in the eighth Essay does not satisfy you, come to the proof of it. Fill a globe with air forcibly by means of a bellows: you will find a greater weight for this full globe, than when it is empty. And by how much? By as much as the air contained in the balloon weighs (by the reason) more than the air in an equal space when it is free. Several persons have remarked this excess of weight of the full globe over the empty globe, but it has not come to my notice that any one has hitherto known the cause of it. I leave aside people of low esteem: the learned Scaliger, the true genius of Aristotle, did not know it: for in Exercitation cxxi. against Cardan, he follows the high road, holding that pure air is light, and that heaviness accrues to the globe because the air near the surface of the earth, such as is blown into the globe, is commingled with vapours and those small terrestrial bodies which are seen manifestly in the rays of the sun. But alas! what does this commingling avail him, since the examination is made in an air quite similar? Assuredly it could show no heaviness if compression did not come to its aid. And if the globe were forcibly filled with the purest air that exists in nature, nay, with elementary fire, reason insists that it would show weight, being balanced in the same air in the first case, and, in the second, in the same fire. This compression of air is a rich field in which men of intelligence will cull rare artifices. It is from it that the sieur Marin Bourgeois, of Lisieux, has derived his arquebus: which I invented myself several years ago, and before the sieur Flurance had described it: but which excels above that of Marin (I say it without vanity) by reason of yielding much more force. I could

communicate to the reader another gentle and profitable invention which I have drawn from this: but designedly I keep silence concerning it, in the hope that one day I may have the pleasure of presenting a very humble petition to his Majesty, and that he will honour me with a license to make use of it for a time to the exclusion of all others, in order that I may be in some measure repaid the charges which will be necessary to bring the said invention into use, as well as several others which I have hitherto held concealed.

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## ESSAY XI.

### *Air is rendered heavy by the separation of its less heavy parts.*

Before speaking of the third method by which air is rendered heavy, namely by the separation of its less heavy parts, I begin by stating the following truth, which cannot be gainsaid; That if from anything whatsoever the less heavy parts be abstracted, the residue will be rendered heavier: I do not say heavier than the original thing in its entirety, but only heavier than a portion of it equal in volume to this residue. Separate the silver which that shameless goldsmith mixed with the gold in the crown of King Hiero: the gold remaining will weigh more than an equal portion of the whole crown. What you do here by art, nature does by her own industry, having for sole tool heat, which serves her worthily in this work. See these salt-workers lead the sea-water by channels into their shallow pits; they know that the heat of the sun by subtilizing the water will sublime it into air, leaving the salt, the heaviest part, below for them. The alchemist, nature's very ape, wishing to imitate her, puts on the chafing-dish the infusion of his rhubarb, in order that the liquor may be

exhaled and the extract remain behind for him. But when he has need of that part which when subtilized escapes, he cunningly traps it on the way by means of the hood which he applies to his alembic. By this device he possesses himself of *aqua vitæ*, which is less heavy than the wine from which it is derived: and the wine less heavy than the dregs which remain after the distillation. Heat acts in this way on all manner of liquors, rarefying some parts, making others more dense, and always separating them on account of their weighing more or less. It produces the same effects for us in the air, and in order that you may note them, turn, I pray you, towards those fields on which the sun has darted his rays during the whole day. You imagine, I wager, that the air which touches it directly is more subtle and less heavy than it was this morning? How different is the fact! It is much more dense, and in consequence much heavier. For how has heat subtilized the air without having raised it? and how raised it without the descent of a heavier air? Nothing mounts upwards of itself; it is the fall of something else that pushes it. It has without doubt separated and raised the more subtle parts, leaving below the dregs so to speak, just as in the distillation of liquors. And if this reason persuade you not that the heated air is rendered dense and heavy, believe at least the evidence of your senses. I undertake to make you feel it so with your hands, and see it so with your eyes. Now that it is midday, do you not find this air hotter than it was a quarter of an hour after the sun had risen? It is not however that the Sun has bestowed upon it a greater degree of heat, because this he possesses invariable, and distributes in the sphere of his activity with an ever equable tenour: in such wise that piercing as it were the air with his rays without resistance, he communicates to

it all his light, and likewise all his heat, which has not itself increased although its action is increased on account of the condensation of its object: for the more subtle parts of the air being elevated little by little, the others are found more frequently here below and are gathered more together in one place. And from this greater concentration comes the greater action. This can be explained by considering the elementary fire, which although it is hot to an extreme degree does not yet burn on account of the extreme rarefaction: but heated iron burns violently; not because it is hotter (for how could it be hotter than the extreme heat?) but because it is denser, containing more particles in an equal space. So much then for touch: let us proceed to the other sense. When this morning the sun commenced to irradiate the horizon, the air by its subtlety was entirely hidden from your eyes but now, can you not see how it quivers above these furrows? It is because it has become denser, and acquired more corpulence, that it is rendered at all visible. So, I think I have acquitted myself sufficiently of what I promised, and now it is time to proceed further and say that if the simple heat of the Sun thus renders our lower air dense and heavy, driving aloft its more subtle, and keeping below its stouter and more solid parts, what will not the vehement heat do, which the throat of a furnace heated by a glowing brasier disgorges for a great space of time? A quantity of *aqua vitæ* placed upon it within a vessel will promptly vanish. Common water and all manner of liquors will be exhaled in a few hours. Air nevertheless will subsist in it (there being no other body to fill its place), but it will be an air dense and heavy as possible; an air I would almost say which is no longer air, but an altered and unnatural air, its subtle fluidity being changed to a viscid grossness. For the violence of the fire subtilizing as much air as

reaches it, will drive off to a distance an immense quantity, only leaving around itself, of all this immense quantity, a species of lees which, owing to its viscous heaviness, cannot take its flight.

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## ESSAY XII.

### *That fire by its heat can make homogeneous bodies dense.*

I know not what fatal calamity has invaded the sciences, for when an error is born with them and with the lapse of time becomes as it were fixed there, those who profess these sciences will not suffer its withdrawal. Exception has already been taken to the doctrine of the preceding Essay, and the objection has been raised that though fire can render heterogeneous bodies denser by separating their more subtle particles, these being of diverse nature, it cannot do the like with homogeneous bodies, inasmuch as it acts uniformly on all their particles, and has no other action than to expand and dilate them all equally, so that in that way air could not be made denser and rendered heavy by the force of heat. I recognise this doctrine (which is opposed to my belief), to be taken from the school of Philosophers whom I honour as great surveyors of nature, but I frankly confess that I have sworn to the words of no one of them. If the truth is with them, I receive it: if not, I seek it elsewhere. Let us see if they have found it in this matter. Homogeneous bodies, they say, are such as have all their parts of the same nature: or else are such as have all their parts of the same name and the same definition as the whole. I even admit that when fire acts on such bodies it dilates them of itself and by its own virtue: but reason teaches me, and experience confirms it, that

by accident, at it were, and after the subtilization and separation of some particles, the others which remain are denser and heavier. If that is denied me, and if the above-mentioned doctrine is rigorously upheld, as if fire could neither of its own nature nor by accident make homogeneous bodies denser, then I undertake to prove that it is false, and could produce a cloud of examples to the contrary; but the courteous reader, for whom I labour, will be content with a few. Vitriol is a homogeneous body, since its particles have the same name and the same definition as the whole; now fire acts upon it in a retort in such wise that we perceive separately its phlegm, its oil, and its colcothar; parts differing in density and in weight. Turpentine is a homogeneous body, the smallest particle of it being turpentine no less than the whole; but when it is placed in an alembic, the fire by its action dilates some of its parts, and condenses others, separating its water, its spirit, its oil, and its colophonium, the difference of which in respect of weight and subtlety is notorious. I have already spoken of wine (also a homogeneous body), how when it is subjected to the forces of fire on distillation, it is expanded and dilated in such fashion as to yield *aqua vitæ* and the *petite eau* as it is called: but the residue is the thicker, the more of that water or even of phlegm there has been removed from it. But why do I trouble myself to produce these examples, since it is evident that from such substances there may, by means of fire, be obtained salt, sulphur, and mercury, parts which differ sensibly in tenuity and weight. It is therefore untrue that fire dilates all their parts equally. I can well foresee that my opponents will attempt evasion by saying that the examples which I submit are compound substances, and that it could not be so with simple substances. Thus however I have been able to convict this maxim of falsity when taken, as it is advanced, in its generality and

extended to all homogeneous substances. Let us see if truth follows better in its track, when it is adapted to simple substances.

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## ESSAY XIII.

### *That fire can make water denser.*

Water is a simple substance beyond question. And yet fire, acting upon it, dilates some of its parts and condenses the others: although, as I said above, the first action is that proper and natural to it, and the second accidental. Pour a pipe of water into an alembic, heat it with fire according to the rules of the art, and draw from it by distillation, to begin with, one pot. It is certain that the water in this pot will be more subtle than that which you put into the alembic. If any one, driven by the desire of contradiction, gainsay it, let him go to the Chemists for a refutation, who, not being able conveniently to make their extracts with common water, are accustomed to employ distilled water, or even dew, which is but water passed through the great alembic of nature: for such water, being more subtle, penetrates more readily the substance of the simples, and extracts from them more easily their virtue and tinctures. Its greater diuretic power and its smaller weight (inseparable companion of a smaller density) will abundantly testify to all the truth of my statement. Now if the water in this pot is more subtle than the water put into the alembic, that which remains must be more dense, inasmuch as condensation of necessity follows the separation of the more subtle parts. This will be the more evident if you continue the distillation, for drawing off pot after pot until nothing further remains, the last will be sensibly denser and heavier than the first; the sensible difference coming by small degrees, which (although imperceptible) will exis

between the first and second, second and third, and so on in order to the last. And this difference will not only exist from one pot to another, but from glass to glass, and even from drop to drop: it being reasonable, since the two extreme drops differ manifestly in density and weight, that this difference goes on increasing from the beginning to the end, with the increase of the number of drops which come from the distillation. From this it is apparent, that as in heterogeneous bodies, fire separates the particles which are of diverse nature; so in homogeneous bodies it disjoins the particles which differ in tenuity, and then gravity assumes the office of arranging them in order, and assigning to each his place, even in fluid matters, whose more ponderous particles ever reach the bottom, making a path for themselves through those which are less ponderous, and sinking necessarily among them. So that if all the water which would distil from the above-mentioned pipe should fall in order into a tube of sufficient length and as thick as a quill, it is credible that the second drop would sink into the first, the third into these two, and so on consecutively to the last, which being the heaviest would traverse all its predecessors and occupy the lowest place, so that the drop which fell first would find itself at the end in the highest place. And although this continuous passage would bring some sort of mixing of these particles in its train, still it would not be such as to prevent the difference in weight of the higher and lower portions being notable. Now if anyone because he cannot actually see the sinking of the drops is inclined to doubt it, let him place and dextrously join the mouth of a phial full of water to the mouth of a like phial filled with claret wine, and he will perceive a similar phenomenon: for the water being heavier will sink into the lower phial through the wine, forcing it plainly to rise into the upper phial. Does not wine even

arrange its more subtle part at the top of the cask and its grosser part at the bottom, in virtue of the weight of one being greater than the other? Common people believe also, and not without reason, that the first glass poured from a can is more subtle and vaporous than those following. This difference which is observed in so small a vessel could easily lead one to the opinion that if a tube only an inch wide and extending several fathoms down, were constructed, and were filled with wine and allowed to stand for some time, the higher portion, if not altogether *aqua vitæ*, would approach it closely in tenuity and efficacy. A fine invention, truly, to extract spirit from wine without the aid of fire, if the thing went actually thus, and the difficulty of making the instrument did not prevent its use! All these remarks serve to lead up to a general assertion, to wit, that in all fluid substances, composite as well as simple or elementary, the higher parts always differ from the lower in subtlety and heaviness: and that this difference is distinguishable into as many degrees as their matter can be divided by their height into distinct parts. So much so that if we conceive a line drawn from the lowest of one of the fluid elements (such as the air) to the highest surface, then there will be as many distinct degrees of weight and subtlety in this element, as the line can be divided into distinct parts (I mean materially, to avoid sophism) and the uppermost part will always be more meagre and less heavy than the second, the second than the third, and so on to the end. For to attribute the same corpulence to all the parts of each element is to deny the evidence of the senses, which make us judge air (for example) to be more subtle at the top of a mountain than at the bottom, on the plain. And also when the heat of the Sun or of our fire subtilizes it beneath, it mounts upwards, without question, until it meets its like according to the degree of

subtlety it has acquired. Besides if this equality did exist everywhere in the element, there would be no reason why one piece should be below rather than above, when it is at rest. For to commit that to chance and adventure would be offensive to the incomparable wisdom of the Author of Nature, who has made nothing therein without weight, number, and measure, and established in it such order that nothing happens fortuitously and without cause. I conclude then that this arrangement comes from weight and not elsewhere. And to end this Essay, I say that everyone can now see that fire acting on the simple substance water, does not extend all its parts equally, but in dilating some, separates them, whence it follows that the others are rendered denser. Thus the maxim which is in contestation will not be true. Nay but, some will say, this must be shown of the air, on which moves the pivot of the controversy. That is their last refuge, and I am about to deprive them of it.

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## ESSAY XIV.

### *That Fire can make Air denser.*

The reasons deduced from the eleventh Essay might suffice to convince an unprejudiced mind that when fire heats air it subtilizes and separates some of its particles, and that this separation is of necessity followed by the others becoming denser and heavier. But since this truth encounters obstinate opposition, I demand, to show it more clearly, that a laboratory be assigned me in the region of the elementary fire adjoining the region of the air: and in it I will give them ocular demonstration of what they refuse to believe. For, as the vessels which we here call empty are nevertheless full of air, so they would there be full of fire. And as when we here pour water into an alembic, the air previously enclosed quits

the place, so there the fire will give place to air poured in, which being set on the furnace, will distil drop by drop into the receiver: and the first measure to be collected will be more subtle than the second, this more subtle than the third, and so to the end. What is more, the difference in subtlety and heaviness between the first and last measure will be as perceptible as the difference in the case of distilled water. Now if any one laugh at my request, let him know that the great Archimedes on a like occasion asked to be given a spot in the region of the air to apply his engines, and he would lift the whole earth. Not that he thought that what he asked could be granted (for he was neither a fool nor a madman in the opinion of the wisest), but he did it, resting on the certitude of his demonstrations and to give a clearer evidence of the truth of his assertion. My request has the same end. Whoever will see a thing approaching this, without resorting to the impossible, let him place on the furnace an alembic of size greater than common, and having attached to a tube at the highest point of the hood a bladder empty of air, let him begin to make the fire. Then the air in the alembic will dilate, and, the original space being unable to contain it, will issue forth and fill the bladder. There will now be put on another bladder precisely equal to it until it also is filled, and this changing will be continued till the end. I say that the last will be heavier than the first. Whoever doubts this, let him essay it, and follow the procedure exactly. By the degrees of this meditation my mind has been raised to greater things, of which nevertheless I will not speak as not touching this matter, and as being difficult, not only to practise, but to comprehend. I come to another demonstration by which the truth that I defend will be more than visible. Let a cannon be set directly on its breech with the muzzle upwards, and let there be thrown

into it a ball of its calibre made red-hot in the fire. It is certain that the air contained in the bore of the gun is so meagre in substance, and in quantity so small, that the ball in passing will impress on it all the degrees of its heat. Notwithstanding this, if you put your hand in the muzzle you can keep it there easily at first; but a little time afterwards you will not be able to do so. Not that the air has increased in degree of heat: it has rather decreased, and so likewise has the shot, which cools little by little: but as it is rendered denser by the separation of the more subtle parts of an abundance of air which will rapidly flow thither, it will act more powerfully, as I have elsewhere said. In the second place, the air which will be seen quivering over the muzzle (which will only be at the beginning) constrains us to admit that it has there become denser; for we cannot say that it is due to the vapours or exhalations rising from the gun: everything in it is too dry and solid to let any part of itself escape. Thirdly, if the air over the muzzle did not become denser it would not blur the objects on the other side which we see through it. And a swaying motion of the air cannot be given as an excuse, since I see distinctly the charms of that lady across the air which she agitates with her fan. And I also see clearly all sorts of objects through the air shaken by the north wind when it blows and whistles noisily. Finally, if a flock of wool well teased out is placed in the muzzle of the cannon, it will not descend; and if it is pushed some distance down, it will mount again suddenly; which would assuredly not happen if the air were not denser than at a distance from the cannon where the flock falls downwards very readily. These reasons, although not gross, are nevertheless so palpable, that they will convince all that the heat has made the air denser beyond the muzzle of the cannon. Now having made the air dense so high

up, what will it have done, I ask you, at the bottom of the cannon near the ball? Taking the ball out after it has cooled you will find it more whitish than it was before being made red-hot in the fire; as if the dense and adherent air gave it this colour, which becomes dim and vanishes with lapse of time, even in a damp place; inasmuch as the ambient air dissolves that which adheres to the ball and recalls it to its original state. As a last dish, I wish to serve up to the reader a remark which may be to his taste. Those who pursue Medicine worthily will sometimes find themselves called upon to visit asthmatics, who, gasping in bed in hot little rooms, can only draw their breath with great difficulty. Perceiving which, they cause the windows to be opened, conduct them thither, and make them inhale the external air, which gives them great relief. If you ask these gentlemen whence such a prompt alleviation comes to the sick persons, they will tell you that it is because the air of the room, being too hot, cannot furnish the heart with the necessary refreshment, which the external air accomplishes better by reason of its coldness. Now, gentlemen and honoured colleagues, having undeceived myself on this point by the preceding meditations, permit me, I pray you, now to undeceive you. It is not the heat of the air in the room which causes this gasping, on account of it not being able to refresh the heart, but rather its density which retards its course through the obstruction of the lungs, so that it cannot furnish the heart in time with matter sufficient for the generation of the vital spirits, which fresh air, being more subtle, can do. And that you may not think I advance this without reason, note this person sick with fever who lies in the same room, in which the enclosed air refreshes him sufficiently, although he has greater need of it. And if the fever attacks the asthmatic (which you will hope for

his good) and dissipates the matter which stopped up the conduits of the lungs, does not the same air then refresh the sick person sufficiently, although his need of it has augmented? Does not the same occur when the dissipation is performed by the use of Diasulphur, which Galen composes, as you well know, of sulphur, of pepper, and of mustard-seed in equal proportions? It must be then that heat has rendered the air more dense by driving off the most subtle portions—which is said to be so impossible. I comprehend already that to elude the force of so many reasons and experiments, I shall be told that the examples I have brought forward can indeed be verified in our gross and impure air, but that it would be otherwise with pure air, if there were such in nature. And assuredly I wish for nothing better to dispose me to a song of triumph. For does anyone believe that I think the sieur Brun and the others who have performed the augmentation in question, procured some purer air, by letters of exchange, from beyond the bounds of nature?

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## ESSAY XV.

### *Air decreases in weight in three fashions: the balance is deceitful: means of remedying it.*

I resume the thread of my discourse which I had somewhat interrupted to remove the objection which had been raised, and so better clear up this matter: and assert that in the three preceding Essays, namely in the ninth, tenth, and eleventh, I have declared the three different means by which air increasing in heaviness may show this when balanced in a pure and free air. Now the law of contraries insists that by three opposite means it may diminish in heaviness. These means are the separation of some heavier foreign matter: the extension of the air to more ample limits: and the extraction

of its heavier particles. But because the comprehension of the former throws sufficient light on the latter, I am spared a more ample explanation, begging only that the reader remark that this augmentation or diminution of weight of which I have spoken in the said Essays, always refers to a portion of air compared with another of equal bulk. For when we have no regard to the bulk or volume of the thing, if we examine its weight by the reason, I say that there is nothing which increases in heaviness but by the addition of matter, nor which decreases in heaviness but by the subtraction of matter: so inseparably are matter and heaviness joined, as has been shown above in the sixth Essay. But if we make the examination by the balance, we meet with a case in which without addition or subtraction of matter the thing will appear heavier or lighter: namely by shrinking, or dilatation. And it is this examination alone of which the ancients had cognisance, when they supposed that the elements in their mutual conversion increased or decreased in weight, when only the extension increased or diminished, by the sole aid of Nature so expert in doing this: not that the weight of things may not be increased or diminished artificially by dilating or compressing them. Beat a piece of iron for a long time in the cold and you will unite its parts and lessen its volume: then it will show a greater weight when placed on the balance. On which, also, if you put a ball of feathers tightly tied together, it will weigh more than the same left loose. From this I infer what was previously touched upon in passing, that the balance is so fallacious that it never indicates to us the just weight of things, except when in it there are confronted two weights of the same matter and figure, like two leaden balls. But two ingots for example, one of gold and the other of iron, which the balance shows to be equal, are not so,

nevertheless: for the iron weighs more by as much as the air weighs (according to reason) which would be contained in the excess of space occupied by the iron over the gold. Which difference I could show precisely in everything that is weighed, and reduce all to the just weight, if I had made the experiment which I explained above in the seventh Essay.

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## ESSAY XVI.

### *Formal response to the question, why Tin and Lead increase in weight when they are calcined.*

Now I have made the preparations, nay, laid the foundations for my answer to the question of the sieur Brun, which is, that having placed two pounds six ounces of fine English tin in an iron vessel and heated it strongly on an open furnace for the space of six hours with continual agitation and without adding anything to it, he recovered two pounds thirteen ounces of a white calx; which filled him at first with amazement, and with a desire to know whence the seven ounces of surplus had come. And to increase the difficulty, I say that it is necessary to enquire not only whence these seven ounces have come, but besides them what has replaced the loss of weight which occurred necessarily from the increase of volume of the tin on its conversion into calx, and from the loss of the vapours and exhalations which were given off. To this question, then, I respond and sustain proudly, resting on the foundations already laid, "That this increase in weight comes from the air, which in the vessel has been rendered denser, heavier, and in some measure adhesive, by the vehement and long-continued heat of the furnace: which air mixes with the calx (frequent agitation aiding) and becomes attached to its most minute particles: not otherwise than water makes heavier

sand which you throw into it and agitate, by moistening it and adhering to the smallest of its grains." I fancy there are many who would have been alarmed by the sole mention of this response if I had given it at the beginning, who will now willingly receive it, being as it were tamed and rendered tractable by the evident truth of the preceding Essays. For those without doubt whose minds were preoccupied with the opinion that air was light, would have rushed to oppose it. Why (they would have said) does not one extract cold from heat, white from black, light from darkness, since so much heaviness is extracted from air, a thing inherently light? And those who chanced to have bestowed their credence on the heaviness of air, would not have been able to persuade themselves that it can ever increase weight, being balanced in itself. On this account I was constrained to show that air had weight; which was recognisable by other means than the balance: and that even with the latter, a portion previously changed and made denser could manifest its weight. All this I have done as briefly as I found possible, and without advancing anything not strictly germane to this matter: to elucidate which at all points it only remains for me to state and refute succinctly some opinions which others have held or might hold; and to resolve the objections which might be made to my answer.

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## ESSAY XVII.

### *That it is not the disappearance of the celestial heat giving life to the lead, or the death of the latter, which augments its weight on calcination.*

Of all those whom I know to have written on this question, Cardan presents himself first, and asserts in the fifth book *de Subtilitate* that lead on conversion into

cerussa, or on calcination, augments its weight by one-thirteenth, afterwards giving this reason—That the lead dies, inasmuch as the celestial heat, which was its soul, vanishes, its presence having previously given it life and lightness, as its absence gives it death and heaviness. This he confirms by the example of the animals, which death renders heavier by the extinction of this celestial heat, which, according to his belief, is the soul of animals as well as of all other mixed and composite bodies. This opinion has several shortcomings (to say nothing worse). Firstly, because it attributes life to the lead. Secondly, because it maintains that the presence of celestial heat makes the lead light, and its absence heavy. Thirdly, because it gives the same reason for the increase of weight of lead on calcination, and of animals on their death. Nothing of all that is the case. For touching life, how can lead have it, since it is a homogeneous body, without distinction of parts, without organs, and without any vital effect or action? If it moves downwards, so does cerussa, which is only its dead body: if it is cooling, cerussa is cooling also. Then how could it preserve that life in the million forms it can assume and put off, remaining always lead? How in a furnace (and this is a much greater marvel) in which it can be kept molten a day, a month, or even a whole year? It would need a very tenacious soul, to suffer so much without dislodging. Everybody, too, agrees that from death to life there is no return. Yet chemists promise us that if we moisten the calx of lead and mix it with water in which barilla has been dissolved, and having dried it, place it in a crucible which has only a small air-hole open, and expose it to a hot and quick fire, we shall reduce it to its first condition. As regards his notion that the celestial heat makes substances light Scaliger raises the pertinent objection that the celestial sphere

which abounds in this heat, being the source of it, must itself be light, and in consequence univocal with the other bodies; which is ridiculous. The loss of this heat, also, cannot render them heavy, for I have proved in what precedes that nothing augments in weight but by addition of matter or by shrinking in volume, of which there is nothing here: for the heat in vanishing can add nothing to the lead, and the volume appears to be visibly augmented, the solid and compact lead being broken down into so many particles that their number borders on the infinite. It would also follow that plants become heavier when they die, their celestial heat being dissipated: but the opposite is evident to all. As to the heaviness which increases in animals on their death, its cause is far removed from that which augments the weight of lead on calcination. As long as the animal lives, its natural heat subtilizes, expands and augments the dimensions of the humours, flesh, and every part of it which is dilatable: and being lost on death, all the above, on cooling, shrinks and contracts, whence comes the increase in weight, as I have often said. What is there in lead like this? Thus the opinion of Cardan seems so frivolous, that I am grieved that a great man, who is rightly held in high esteem throughout the universe, should have declared to me a few days ago that he inclined towards it.

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## ESSAY XVIII.

### *That it is not the consumption of the aërated particles which augments the weight of the lead.*

Scaliger has bound himself up so closely with Cardan, that I cannot separate the two; so here as elsewhere he must follow him. In Exercitation ci, section 18, he states that the augmentation in weight of calcined lead comes from its aërated particles being consumed by the

fire: for the same reason, says he, as baked tile weighs more than unbaked. Oh how the superficial resemblance of things sometimes deceives fine understandings! This great man seeing that the calx of lead and the tile after treatment with fire both become heavier, thinking of the same effect, has sought only one and the same cause. The causes are very diverse notwithstanding. The tile increases in weight by decreasing in extension: the calx, because of the addition of matter. And in order to make this better understood, who does not know that tile is made of a rich and sandy earth kneaded up with water? And that the Sun absorbing its humidity leaves in it an infinite number of little cavities which were originally filled with water? When it is baked in the kiln, the heat softens it, like metals, and takes it to such a point that it almost melts, and does in fact melt if the heat is excessive. During this softening the particles shrink together, unite, and become attached to each other; the cavities disappear, and contraction thus occurs: thence comes its great weight, as I have often stated. As to lead, it melts on the fire, as is well known; and being melted, fills the vessel at all points, leaving no trace of air within itself, in accordance with the privilege that nature has granted to ponderous and fluid bodies of forcing upwards bodies which are less so, and sinking down within them. This lead taken off, loses its heat little by little: while it closes up and coagulates, shrinking into itself, and diminishing in volume, as is apparent from the dimple which is seen on the surface when it has cooled: so that it is impossible to imagine the presence of air enclosed in that heavy mass. Melt it again and calcine it, you will find it heavier: not from the consumption of aërated particles, seeing that there were none in it; but by reason of the condensed air which has united with it, as I have already said. And as a matter of fact if it lost

some aërated particles, would it not decrease in volume? On the contrary it increases. And again, why do not stones and plants increase in weight on calcination if that reason be valid? I infer from the above that the consumption of aërated particles never augments the weight of things when shrinkage does not result; which not being the case in our affair, cannot be admitted as the cause of the surplus weight of which I discourse. I add in conclusion that the air which is forced by a syringe into a globe already full of it, diminishes the weight on escaping, far from increasing it as Scaliger fancies. It is true that this is met with only in this one case.

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## ESSAY XIX.

### *That it is not soot which augments the weight of the calx.*

I read in the tenth chapter of the sixth book of the Chemical Arcana by Libavius (for I have not seen it elsewhere) that Cæsalpinus has written, that it is a remarkable thing that lead on calcination increases in weight by eight or ten pounds per cent. Then, seeking the cause, he says that it is due to the soot which the fire produces striking the vault of the furnace and by reflection falling back on the substance. This Cæsalpinus would never have advanced had he paid attention to what I am about to deduce. Firstly, the soot on being exhaled by the fire is of so rare a nature, that the seven ounces of increase found by the sieur Brun would occupy more space than all the calx he derived from the calcination. In the second place, this abundance of soot would so blacken the calx of tin or lead, that the Ladies would never borrow from it the whiteness of their complexions, as several do. And again, what would hinder the quantity of this calx from being infinitely increased, since the fire

may be continued as long as we wish, and will always furnish soot? Let us add that the sieur Brun calcined his tin on a naked and open fire so that the soot was only able to pass on one side by the dampers of the furnace and not to fall on the substance, on which also it could not descend, not being so heavy as the air contained in the vessel. As to Libavius, he has rejected this opinion of Cæsalpinus, and even says that apprentices in chemistry will laugh thereat, without however adducing much against it: having enveloped his opinion in such a mass of words that it is not easily extracted from them. Thus he wishes the following words to pass as a solution of the doubt: That transmutation alters weight; and a few lines further on: That burning augments the weight of lead. Better to see the energy of these replies, it is only needful to diversify a very little the terms of our question, the sense remaining still the same, and adapt them as follows: Why does the transmutation of lead into calx change the weight? because, he says, transmutation changes the weight. Why does burning augment the weight of lead? because, he says, burning augments the weight of lead. See I pray you if what he says of the reason of Cæsalpinus is applicable to his own. Assuredly one needs not to be a great Chemist, or a great Logician withal, to laugh at it. Receive his reasons who will, they shall never be accepted by me. But I am ashamed to discover the shame of this person, whose merit in other respects is great on account of the number of writings he has made public, which are filled with much sound doctrine.

## ESSAY XX.

### *That the augmentation of the calx of tin and lead does not come from the vessel.*

I come to opinions which have not been written, at least as far as I am aware. Since nothing touches the

tin and lead on calcination except the air and the containing vessel, those who will not acknowledge the former as the cause of the increase in weight, cannot I think with any plausibility have recourse to anything but the latter. For they might persuade themselves that in the calcination and incessant agitation of the said metals, the iron being burnt might become friable on its surface, a portion of which would mix with the calx and thus increase its weight. Just as the pearls, which the Pharmacist brays on his marble acquire a greater weight by the addition of the stony matter which, being crumbled, mixes with them; very often to the prejudice of those to whom they are afterwards given. But in order to rid them of that opinion, I represent firstly, that if the powdered iron, which is brown, became mixed to so great an extent with the tin, it would darken its calx, which nevertheless is always white. Secondly, if the vessel were consumed in this way, it would be useless after two or three calcinations: whereas it lasts for several years, although used daily. Thirdly, from a very little tin or lead one might extract the calx in abundance, it being thus easy to reduce all the vessel to powder by continuation of the fire, which is contradicted by experiment. What is more, Modestinus Fachsius has observed (as Libavius reports at the place already indicated) that in the examination of metals, the vessel, the cupel, the lead, and the metal examined, are all heavier after the examination than before exposure to the fire; although there has been a loss of much matter dissipated as fumes: which can only happen by the adhesion to all this of much of the air above-mentioned, a thing which has hitherto not been comprehended. So on this account the vessel for the calcination may be found too heavy, and I beg the sieur Brun to attend to it.

## ESSAY XXI.

### *That it is not the vapours from the charcoal that augment the weight.*

It has been reported to me (I know not if faithfully) that an intimate friend of mine, a man of profound knowledge, and of most refined and substantial judgment, to whom the sieur Brun had made the same request as he made to me, has allowed himself to be led to the belief that the augmentation of weight in question proceeds from the vapours of the charcoal, which passing through the vessel, mingle with the calx. This I maintain to be impossible. For if such vapours cannot pass through a beaker of glass, a plate of tin, an earthen pot (otherwise our broths, sauces, and soups would be infected with them), how shall they traverse an iron vessel, the material of which is so much stronger? If the most subtle air cannot penetrate it (of what use else would be my Æolipyle?) how shall these gross vapours penetrate it? and having penetrated it, what impediments will they find in the calx to be there arrested? why should they there finish their course? The heat by its vehemence expels from the tin and the lead the humidity which bound their particles together, driving off all the metallic vapours, although these are natural to them. And it will leave these foreign vapours? It seems not very probable. O truth, how dear thou art to me, thus to make me strive against so dear a friend!

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## ESSAY XXII.

### *That it is not the volatile salt of the charcoal which augments the weight.*

As soon as I had made a rough sketch of this discourse, I sent it to the person of whom I have spoken in the preceding Essay. A few days thereafter he put

into my own hands a manuscript disavowing the opinion which I have combatted. And after having opposed to my belief touching the increase of density and heaviness of heated air the reasons which I have given in the twelfth Essay, and refuted in it as well as in the two Essays following: he puts forth his opinion as I give it succinctly here. That the augmentation of which we treat comes necessarily either from the vessel, or from the air, or else from the charcoal. Not from the vessel, as it loses nothing of its weight: nor from the air, since heat can only subtilize it and make it less heavy, as he presupposes to have shown; it follows then, he says, that it is the charcoal to which the augmentation is due. And to show how that occurs, he says that charcoal contains two parts or natures, one vegetable, the other metallic; and each of these two others, one fixed, the other volatile. That the fixed part remains at the bottom of the furnace in the form of ashes, in which the fixed salt is contained and may be separated by ablution: and the volatile part ascends all around the vessel, containing in a superfluous humidity (which is due to the vegetable part) a volatile salt of metallic nature, which being raised on the wings of the humidity and meeting the air directly above the vessel (more rarefied and less heavy than the vapour which comes from the charcoal) sinks through it into the vessel, and attaches itself by a close sympathy to the fixed salt of the calx of tin, which, having taken up a certain quantity of it, and being as it were satiated, rejects the surplus: just as the salt of tartar after a certain number of cohobations cannot impregnate itself more with the volatile salt contained in brandy. Having perused his paper, I refuted this opinion in his presence by the following reasons. Since we must believe everyone on the subject of his own art, if we have nothing to the contrary, it is

reasonable that in speaking of the volatile salt we borrow the language of the Spagyrics, who alone can properly discourse of it, having discovered it first and revealed it to us, when we never thought that a volatile salt existed in nature. They recognise in vegetables (as in almost all things) two sorts of salt, one fixed, and the other volatile; the former containing a fixed spirit, and being contained in the solid parts of its subject; the other containing a volatile spirit, and being contained in the juices. The fixed is extracted, so they tell us, by calcination, remaining in the ashes. The other cannot endure fire (being in fact as in name, volatile), but flies off at the least heat with the juice containing it: or is lost by the simple drying up of the vegetable. Now, this being so, it is beyond doubt, that in charcoal there is no volatile salt at all; seeing that even in the wood of which it is made there can be none, since it is dried beforehand: and even if there were some in it, who does not see that it would of necessity be lost, when, on heating, the wood is reduced to charcoal? Indeed, although I admitted unjustly that charcoal contains some volatile salt, those who know how rare this salt is in all things, will never be persuaded that from the small amount of charcoal consumed by the sieur Brun in his calcination, so great a quantity could have proceeded. For it is not merely necessary to imagine only seven ounces of it: but also what replaced the decrease of weight caused by the loss of vapours from the tin and the aggrandisement of its volume: and besides what the smoke from the charcoal carried elsewhere, not only throughout the laboratory, but without it, whither the vapours were copiously poured through its openings. If in these fumes there were salt in proportion to that which fell into the vessel from the fumes above it, and the whole were gathered into one mass, truly the quantity of it would be monstrous.

And then, when the calx of tin should have satiated itself with salt by this imaginary sympathy, what would prevent the continued heating to deposit more on the top of it, and fill the vessel, since it descends into it by its proper heaviness? Experience refutes all this: and moreover I have proved that the air above the vessel is so dense that the salt could not descend in it. In addition, if a furnace is erected in a wall separating two chambers in such wise that the vessel is on one side, and the doors and dampers to introduce charcoal and admit air are on the other, I maintain that the augmentation will take place, although no vapours can enter the chamber in which the vessel is placed. And this I confirm by the experiment which I made at the iron-works of Jean Rey sieur de la Perrotasse, my eldest son, where I found a similar augmentation with tin which I calcined on a pig, as they call it, or an ingot of sixteen to twenty hundredweights of iron, at the moment when, issuing from the furnace, it was poured into its mould. For no one can say that the vapours from charcoal had contributed anything here. Therefore the volatile salt is not admissible in this matter.

## ESSAY XXIII.

### *That the mercurial volatile salt is not the cause of this augmentation.*

Some days after the refutation which I have just recounted, the same person wrote me another opinion of his. Namely, that the volatile salt is of mercurial nature, having no one of the principles in it entirely pure, but mixed with the others: so that in the salt there is the true fixed salt, then another less terrestrial, partaking of the nature of sulphur; then another still more subtle and penetrating, which partakes of the nature of mercury. Now, crude cold mercury easily penetrates iron, and attaches itself closely to it, both internally and exter-

nally; so that it is not unreasonable to be of opinion that the volatile salt of mercurial nature, rarefied by fire and rendered much more penetrating, should succeed in passing through the thickness of vessels, similarly heated by the fire and made more easily penetrable, and should attach itself to the calx of tin by a certain sympathy which can subsist between them, as well as between gold and crude mercury. This second opinion is sufficiently destroyed by the reasons of the preceding Essay; for having shown that in charcoal there can be no volatile salt, who does not see it cannot subsist therein in any fashion? Again, if this salt of mercurial nature penetrate the vessels, it will necessarily dissolve them, and make of them an amalgam, which does not happen in our calcination. Besides, crude mercury being exhaled as we see, has a very small heat; how then can it happen that this mercurial salt, of so subtle a nature, having penetrated the vessel should persist in the still burning calx without swiftly flying off? Again, if we are to find in each of these principles all three, I do not see why they cannot all be found in each of these again, and why we should not go on in this way *ad infinitum*. These are speculations subtle indeed, but having no foundation in nature.

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## ESSAY XXIV.

### *That it is not humidity attracted by the calx which augments its weight.*

Not long since, speaking to a learned and judicious man, it occurred to me to discover this question to him, whereupon, having thought a little, he told me his belief was that the augmentation was due to the fact that the tin by its great dryness attracted to itself much humidity, which made it so heavy. But I cannot approve this opinion for the reasons that follow. Firstly, because I

have never learned that a contrary will attract its contrary: it flees from it rather, or banishes it if it can. Then by humidity he cannot mean a bare quality, but water or air endowed with it. As for water, whence would the calx get it, having none in its neighbourhood? Will there be found in the laboratory where the calcination is performed, air more humid than the common? Will not the heat of the furnace have absorbed it? And although this calx should attract enough water or moist and nebulous air to increase its weight by a fifth or thereby as experiment proves, we should then have mortar instead of a dry calx. I will add that at the instant of calcination the calx has augmented in weight before it has the time to exercise this imaginary attraction.

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## ESSAY XXV.

### *By a single experiment all opinions contrary to mine are entirely destroyed.*

It is said of Hercules that no sooner had he cut off one of the heads of that Hydra which devastated the Lernæan marsh, than two others sprang forth. My condition is similar. The error that I combat teems with opinions, which are so many heads: if I cut off one, we see two appear. My labour is always on the increase: and I believe I should never have done if I only employed myself in cutting off one after the other. To give it the deathblow, I must gather my strength and make stiff my arm, in order that I may strike them all off at a single blow. Let him who will take heed: for now the fatal stroke will be dealt him. I have just read in Hamerus Poppius, in the third chapter of his book entitled *Basilica Antimonii* the new method which he practises to calcine antimony. He takes a certain quantity of it, weighs it, and places it in the fashion of a cone on a slab of marble,

then having a burning mirror, he opposes it to the Sun, and directs the pyramidal point of the reflected rays on the point of the cone of antimony, which straightway fumes abundantly, and in a little while, what the rays have touched is converted into a very white calx, which he separates with a knife, and conducts the rays on the remainder till all has become white: and then the calcination is ended. It is a remarkable thing (he adds) that although in this calcination the antimony has lost much of its substance, by the vapours and fumes which are copiously exhaled, yet its weight augments instead of diminishing. Now if we seek the cause of this augmentation: will Cardan say that it is the vanishing of the celestial heat? It is even infused into it more largely by the solar beams. Will Scaliger say that it is the consumption of the aërated particles? But on being broken up into calx and increasing in volume, more of these are thrust into it. Will Cæsalpinus allege soot? There is no fire to produce any. Would the vessel furnish something on its part? The rays are conducted so dextrously on the substance that they do not touch the marble. Will anyone suggest the vapours of charcoal? None is used in this affair. As to the volatile salts which have been so ingeniously brought forward, they here lose their savour and their charm. Peradventure someone will put humidity to the fore, as has quite lately been done. But whence would it come? from the marble? nay nay, that is not imaginable. From the air? still less, for this operation should succeed best in the hottest days of Summer, in the most violent ardours of the Dog-days, when everything below is so heated, that even in the shade and in the night-time the air dries soaked linen, and parches the moist earth. And, during the day, where the Sun strikes, he burns our complexions, withers the grass, scorches fruits, desiccates wood, dries up lakes,

lowers rivers, and inflames combustible things like pigeons' droppings. To seek humidity in the air wherewith to moisten our calx and make it heavier in that fashion—not by night but in the daytime, not in the shade but in the Sun, and not where he simply shines but where his beams gathered in a concave mirror are reflected with such violence that they melt and calcine the metals—to seek humidity there, I say, is to seek fire in ice and a knot in a bulrush, as they say, a thing which never can be found. Let now all the greatest minds in the world be fused into one mind, and let this great mind strain every nerve beyond its power; let him seek diligently on the earth and in the heavens, let him search every nook and cranny of nature: he will only find the cause of this augmentation in the air when the Sun's rays heat it, and render it dense and heavy, so that it then mixes with the calx as the antimony on calcination crumbles and becomes adherent in its minutest particles. And this confirms entirely the truth of my belief in the augmentation of lead and tin: which can have no other cause than the admixture of condensed air, there being no difference between the increase of weight in these two metals and in antimony, only that in the last case the air is condensed by the solar rays, and in the former by the heat of the common fire.

## ESSAY XXVI.

*Why the calx does not increase in weight infinitely.*

Having thus repulsed contrary opinions, mine alone can hold the field freely. It is true that perceiving certain objections which might encumber its path, I will now go forward to remove them. The first would seem to lead us to the absurdity to which I took exception in the case of Cæsalpinus, namely, that my opinion being granted, the calx of which I treat might increase in weight *ad*

*infinitum.* For why, one will say, should not the calx increase infinitely, the fire being infinitely continued so as always to furnish that dense and heavy air to increase it? I escape this difficulty, which might entangle some less subtle, by remarking that all matter which is increased by the addition of another matter is either solid or liquid, and that the mixing of them may occur in three fashions. For either the solid matter mixes with solid, or liquid with liquid, or liquid with solid. The mixing and increase which occurs in the first two cases has no limit. Mix with this sand and add to it some other sand, and you will go on increasing it without end. Mix with this wine and pour into it some other wine, and you never will have done. It is not the same in the third case, when a liquid is added to and mixed with a solid: such a mixed addition will not increase always, will not go on infinitely. Nature in her inscrutable wisdom has here set limits which she never oversteps. Mix water with sand or meal and they will be entirely covered by it, down to the least of their particles: pour on more, they will not take it up; and on withdrawing them from the water they will only carry what adheres to them and suffices properly to enclose them. Plunge them in again a hundred times and yet again a hundred, they will come out no better charged; and when left to themselves within it, they will leave the superfluous water and sink to the bottom of themselves: so scrupulous is nature to stop at the limits which she has once prescribed herself. Our calx is in this condition: "the condensed air becomes attached to it, and adheres little by little to the smallest of its particles: thus its weight increases from the beginning to the end: but when all is saturated, it can take up no more." Do not continue your calcination in this hope: you would lose your labour. For the rest, let not that trouble you which was said in the eleventh Essay,

namely, that I almost called that air no longer air but an unnatural air: for these are words of excess, by which I mean nothing more than that the air has been deprived of that liquid subtlety which caused it not to adhere to any substance, and has become gross, heavy, and adherent.

## ESSAY XXVII.

### *Why all other calces and ashes do not increase in weight.*

I come to another objection which might be raised. Why do not all other calces and ashes made by the force of fire increase in weight as well as the calx of tin and of lead? What privilege have these over the others? I answer that the things calcined or incinerated are of different nature. Some have much exhalable and evaporable matter, or (speaking spagyrically) much sulphur and mercury, which the fire expels to the end. Here there is much diminution and little ash, which cannot attach to itself as much of the air condensed by fire as even to make up for the decrease. Others have little exhalable and evaporable matter, or little sulphur and mercury: consequently there is little diminution, much ash (from the abundance of salt) which attracts so much of the condensed air, that not only is the diminution made good, but the weight increases largely in addition. Stones, vegetables, and animals are usually in the first named class. Lead and tin in the second. There are other things which calcination carries to such increase of volume, that even if little or no matter were lost, the weight would nevertheless be much diminished, not so much when examined by aid of the reason as when examined by the balance. Such are the Indian metal named Calaem,* and a species of crocus of iron, such as Chemists can exhibit.

* [Zinc.]

## ESSAY XXVIII.

*Whether lead increases in weight as well as tin.*

I should have ended, were it not that the sieur Brun informs me in his letter, that having noticed the augmentation of tin, he made the same experiment with lead, which he found to diminish by one ounce in the pound: which sank him still deeper in doubt, having imagined that the same increase should be there found as with tin, from the proximity of their nature and from the identity of the process of calcination. But to the experiment of the sieur Brun I oppose the experiments of Cardan, of Scaliger, and notably of Cæsalpinus already mentioned, who says it is worthy of astonishment that lead on calcination increases in weight by eight to ten pounds in the hundred. Shall I leave these persons in strife each to sustain his own experiment? I am too pacific: behold their reconciliation effected. Some lead is more pure than others, either because it comes so from the ores or because it has been previously melted. Those named above have found augmentation with the pure lead: the sieur Brun a diminution with the other.

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*Conclusion.*

Behold now this truth, whose brilliance strikes the eye, which I have drawn from the deepest dungeons of obscurity. This it is to which the path has been hitherto inaccessible. This it is which has distressed with toil so many learned men, who, wishing to know it, have striven to clear the difficulties which held it encircled. Cardan, Scaliger, Fachsius, Cæsalpinus, Libavius, have curiously sought it, but never perceived it. Others may be on its quest, but vainly if they fail to follow the road which I first of all have made clear and royal: all others being but thorny footpaths and inextricable byways which lead never to the goal. The labour has been mine; may the profit be to the reader, and to God alone the glory.

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