Int. J. Heat Mass Transfer. Vol. 9, pp. 1153–1166. Pergamon Press 1966. Printed in Great Britain

ON THE FIXATION OF WATER IN DIVERSE FIRE

JOHANN GOTTLOB LEIDENFROST
University of Duisburg

(From *A Tract About Some Qualities of Common Water*, 1756)

Translated by
CAROLYN WARES © 1966
DeGolyer Collection in the History of Science and Technology, University of Oklahoma, Norman, Oklahoma

Introduction by
KENNETH J. BELL
School of Chemical Engineering, Oklahoma State University, Stillwater, Oklahoma

INTRODUCTION

AMONG specialists in the field of boiling heat transfer, tradition has it that the first literature on boiling as a topic interesting in and of itself is by J. G. Leidenfrost, a German medical doctor, who in 1756 published *De Aquae Communis Nonnullis Qualitatibus Tractatus*—in translation *A Tract About Some Qualities of Common Water*. Within my present knowledge, tradition is correct. There are numerous references to boiling processes in earlier works on distillation, metallurgy, cosmology, etc., but these references are descriptive, superficial, and peripheral to the subject under discussion. Particularly by contrast to Leidenfrost, these other references contain no detailed or quantitative observational data.

Leidenfrost studied the boiling of small liquid masses on a hot surface. If the surface is hot enough, the masses are supported on a film of vapor generated by evaporation from the lower surface of the mass. This process is now termed the Leidenfrost Phenomenon and is a particular case of film boiling. Every housewife has provoked the Leidenfrost Phenomenon by spilling water on a hot frying pan, and the Phenomenon has undoubtedly been observed since prehistoric times. Even so, the first unambiguous reference to the Phenomenon evidently appears in Boerhaave's *Elementa Chemiae* in 1732; this is a brief comment that does not detract from Leidenfrost's primacy in the subject.

Some of Leidenfrost's experiments—those at lower surface temperatures—fell into the transition and nucleate boiling regimes, in which the liquid at least partially wets the surface. It is ironic that Leidenfrost refers to the nucleate boiling as "true motion of boiling" and does not consider the process which bears his name to be boiling at all.

It is possible to roughly calculate heat transfer rates from Leidenfrost's data. It was not possible for Leidenfrost because he was ignorant of the fact that energy is required to cause a phase change at constant temperature. The latent heat of evaporation was first shown to exist by Joseph Black about 1760.

I am tempted to make numerous other observations on Leidenfrost's work, in the light of our present convictions on heat, light, and matter in general and the rather extensive studies of the Leidenfrost Phenomenon by other workers since. But my time and the Journal's space do not permit this, and perhaps it will be more interesting for each reader to encounter afresh Leidenfrost's account.

Some further details on the *Tractatus* are necessary. The *Tractatus* is not a common book—an extensive but not exhaustive search turned up only one copy in this country, at the Yale University Library, and a microfilm of this copy has been used for all subsequent work. There are 150 pages (about 26000 words) in the original Latin version of the treatise, of which 33 are devoted to the section of interest, "On the Fixation of Water in Diverse Fire."

The treatise opens with a dedication to the Royal Academy of Scientists at Berlin. Then follow 30 pages of introductory material on the nature of water, fire, and heat and the transformations of matter. There is a summary and comparison of the views of authorities from Thales and Aristotle to Boerhaave. The section presented here comes next. This is followed by 57 pages entitled (in translation): "Concerning the Watery Solid Membranes of Bubbles." Leidenfrost gives here the details of a number of experiments he has made with soap bubbles. The experiments appear to be carefully done and the reduction of data more tightly reasoned than in the present section; we would still argue with many of his conclusions about the nature of matter.

The last major section, of 20 pages, is entitled "About The Conversion of Water into Soot." A short quotation from this section is irresistible:

> "The cause of such a conversion of water into soot is marvellous in its own simplicity. For flame burning without impediment gives pure water. However if the same is impeded in flaming through whatever hard body you wish, for example an iron stick, in its motion, the water particles binding to such a hard body and by their rotary motion suddenly ceasing are made into soot."

The treatise concludes with a 10-page summary of its teachings. Perhaps the entire translation can be published some day, but the section on the Leidenfrost Phenomenon is the most pertinent to the present audience.

Finally, I must introduce the translator. Mrs. Carolyn Wares was formerly an instructor in Latin at the University of Oklahoma and is presently a graduate student in the History of Science, associated with the DeGolyer Collection in the History of Science and Technology at the University of Oklahoma. No better qualifications for translating Leidenfrost could be asked, and no lesser would have been sufficient. Mrs. Wares carried on the task with dedication and enthusiasm, and I think all boiling specialists will join with me in thanking her for reminding us how it was in the beginning. I also thank Dr. Duane H. D. Roller, McCasland Professor of the History of Science and Curator of the DeGolyer Collection, University of Oklahoma, for his interest in the translation and his many suggestions on it, and the Army Research Office-Durham for their support of a research programme on the Leidenfrost Phenomenon at the Oklahoma State University.

KENNETH J. BELL

ON THE FIXATION OF WATER IN DIVERSE FIRE

XV

1. An iron spoon of any size, well polished within and free from rust and dirt, is heated over glowing coals until it glows with light. To this glowing spoon, removed from the coals, send through a glass tube of suitable length, of which the other end finishes in a very narrow capillary canal, one drop of very pure distilled water. However the water which I have used was certainly as pure as can be made through distillation. It dissolved whole nitrous crystals of mercury without color, nor did mercury percipitate in any way, nor was it disturbed by alkalis. Moreover the water which I mostly used runs into a protected pool, now and for 6 and some odd years. No disturbance was noticed in all this time. Even with ordinary non-distilled water one is allowed to assume a nearly similar event. In any case such a tube as I have just described is right to use so that one drop always equal to another falls from the small opening, nor does varying in the magnitude of drops make a difference in the experiment. This drop which first fell upon the glowing iron is divided into a few little globes, which nevertheless after a little while are collected in one great globe

again. At the instant when the drop touches the glowing iron, it is spherical. It does not adhere to the spoon, as water is accustomed to do, which touches colder iron. Nevertheless in the first moment of contact the glowing iron around the drop is black, indeed very black in a space which is greater, the brighter the iron, as if the matter of light and fire from the glowing iron suddenly was snatched into the water.

2. If then the spoon remains motionless, this water globule *will lie quiet and without any visible motion, without any bubbling, very clear* like a crystalline globe, always spherical, adhering nowhere to the spoon, but touching it in one point. However, although motion is not visible in the pure drop, nevertheless it delights in a very swift motion of turning, which is seen when a small colored speck, for example some black carbon, adheres to the drop. For this is turned around the drop with a wonderful velocity, and it shows the same sight as the drop of silver upon a little tub, which as long as it is polluted with particles of litharge shows its own gyration by a most rapid circular motion of these, until deeply cleansed from these it emits most beautifully the customary splendor. For if Astronomy concludes correctly from the motion of the spots of the sun alone that gyration of the sun around the axis, it is permitted so also to Chemistry to form a similar conclusion from similar phenomena. Moreover, however, this drop only evaporates very slowly. For if you turn to a pendulum indicating seconds with its oscillations, at least 34 or 35 seconds, that is it runs a little over half a minute of an hour before the whole drop disappears. Which at last exceedingly diminished so that it can hardly any more be seen, with an audible crack, which with the ears one easily hears, it finishes its existence, and in the spoon it leaves a small particle of earth.

3. While these things are done, the glowing spoon ceases to glow and becomes cooler. Therefore as soon as the first drop disappears, send another drop similar to the first through the same glass tube to that same spoon, which with similar phenomena will disappear in 9 or 10 seconds. But there is this difference; this other drop in this case is divided into more globules than the first, which return into one globe with more difficulty, but they are moved from here to there and as if dancing they produce a whistle with their motion in the spoon.

4. Meantime while the iron is cooled more, after the second drop has evaporated, then let go a third, which, with a great motion of globules greater certainly than can be called boiling, it will disappear within the space of three seconds. I observed nothing remaining of solid, earthy matter in the second and third drops, as from the first drop—unless there was a manifest impurity in the spoon.

5. If then you put in the fourth drop with the same precautions, this is no longer rolled into a globe, but adheres to the spoon and makes a damp spot in it and with a whistle surges into a true motion of boiling, and thus foaming into vapors it will depart very swiftly inside the space of one second or even swifter, and leaves nothing which is in any way sensible of earth or of solid matter.

6. If after this you send down successively the fifth, sixth, seventh, and more drops to the same spoon now cooled enough so that it can be touched with the fingers with no harm, it will be evident to the eyes that because the spoon is cooler, the drop falling imparts a greater moist spot to the spoon, and adheres to it a longer time before it is evaporated.

7. If in the place of one drop you put in the spoon glowing well several drops, for example six seven, eight, ten, it also makes a globe, but not perfect, the top depressed, nevertheless very transparent. And not less than for one drop slowly expiring without any boiling motion, thus so that some 10 drops stand through two and more minutes in the fire. And they leave a portion of earth, especially if the spoon is left over the fire, so that it doesn't cool too fast.

8. In the same way I compared a deep orichalch phial, the bottom segment of which was spherical, the inside polished. When heated until it glowed,

over burning coals, it affected the drops of water in a similar manner, as had the iron spoon.

9. If an iron or copper vase is not pure, but is mixed with iron rust, the experiment either does not succeed or not accurately, because the verdigris and rust impedes the attachment of the water, as will be seen. If this vase is not of pure enough metal, if it is heated red hot, after a great quantity of water (for example 10 drops and more) is poured in at one time, all impurity is rapidly rubbed off by the motion of this water, so that afterward the experiment can be undertaken without disturbance.

10. A little piece of ice is swiftly dissolved on glowing iron, and then it shows the same phenomena as does simple water.

XVI

From this observation various things are understood. First, it is certain that fire brings about the volatility of water, but not by boiling waters. For at first the abundance of vapor increases with the degrees of heat until we come to a certain point, namely, to that at which water boils. For when the heat increases more, beyond the point at which water is accustomed to boil, the drops of water stand there for a longer time, or, what is the same thing, they evaporate more slowly there. If then the fire is increased more, the water is exhaled much less, and the hotter the iron is and the closer to the fire, so longer do the drops adhere to it. That is, it is evaporated slower so that finally in great heat, such as that of glowing iron, it may be made attached a long time, at least through 34 seconds, and its small part supports for a long while the power of a high heat. Nor do I doubt, if in the experiment a great mass of iron of notable thickness, which therefore does not emit heat so swiftly, is used, then, sufficiently great quantities of water can assume a greater attachment in that same way as this. However, there is no opportunity to try this with large masses. In the second place it appears that the degree of fire at which the water is most of all evaporated is that at which it boils. Third it is manifest that the fire protects the immediate contact of bodies, because the water does not dampen the fiery iron, nor adhere to it. Whence the idea that the drop of water attracts a great part of the fire out from the surface of the glowing iron and it releases the iron from brilliance to motion. Fourth the very hot water endures without any boiling motion. Wherefore it is necessary that at the instant of time in which a drop falls upon the burning iron, all enclosed air is suddenly expelled. Or, what is more probable, the air in such heat is fixed and loses a part of its elasticity. From this, in such a fire the water droplet is left most clear on top, although its transparency is disturbed on account of the many bubbles in the motion of boiling. Fifth, it is correctly concluded from the perfect spherical figure of the droplet and from its whirling motion that the mutual adhesion of the water particles among themselves is greatly increased in such a fire so that in truth the cohesion of water thus is made greater in heat than in cold. But really thus the sixth of these observations suggests that water is changed into earth by a large fire, because always after the complete evaporation of the drop some terrestrial matter remains in the heated vessel. This has not escaped the notice of the distinguished investigation according to the warning of Boerhaave. For into such fire because of the very light atmosphere all the dust from the surrounding air easily flies together, and it can enter these hanging drops, unless other circumstances advocate the contrary, a subject which we will not discuss.

XVII

In order to avoid all deceit, the experiment is varied:

1. If you loose a drop of moderately distilled spirit of wine from the same glass tube onto an iron spoon glowing with light, this also is altered into a similar crystal globe as soon as it touches the surface of the iron. And in the

same way, provided that it is kept from the flame of coals or a lamp which happens to be there, even if it remains in the strongest heat of the glowing iron, nevertheless it does not seize the flame (i.e. does not catch fire). Rather in all these things it is similar to water; it holds itself and stands fixed around 30 seconds or more in this extreme heat. Finally it gradually leaps apart with a small noise, and leaves a small piece of dry earth, which is burned up a little after by the heat of the iron. And the likeness of carbon or soot glows for a short time, then it splits into white ashes.

2. While the spoon is held over the burning coals the drop of spirit of wine falling takes up the flame easily, because it attracts it from the coals, and then it holds it in the bottom of the spoon (i.e. catches fire falling through the flames into the spoon and then continues to burn in the spoon). This can be prevented if at the moment at which the drop is let loose, you remove the iron from the coals, or in some other way cover the flame of the coals. However, when once the flame takes hold, it does not stop burning until all the *true spirit* has been consumed, because it is done quickly in such a small drop. However in the moistness from this spirit which remains in the bottom of the spoon after the extinction of the flame, the resemblance of simple water is fixed in a long enough interval of time. Afterward it flies into several pieces with a noise and vanishes.

3. However when I have very carefully repeatedly distilled this *true alcohol* or spirit of wine, in very tall glass jars, I put one drop into the glowing vase, and I set it afire with the flame of a small piece of paper moved close with the hand. This alcohol burns up swiftly in deep flames, and no vestige of a water residue remains. Truly if the same alcohol is protected from the flame, it has the appearance of common water and for a long time the clear globe resisted the actions of the fire. If 8, 10, or several drops are dropped into such a spoon, they conduct themselves similarly, but then they can with difficulty be protected from the flame.

XVIII

I do not understand the spirit of wine in these phenomena. Why does a drop of it not take hold of the flame spontaneously in a very hot iron spoon and in a very warm atmosphere, when nevertheless it is inflamed easily from another burning body? In itself it is clear that the weakness alone of the air is not at fault, because in that same atmosphere flame persists, if this has been excited before with another flame. I learn however from this phenomenon that great heat does not destroy spirit of wine, nor does it change into its parts, unless the flame approaches. For according to others the spirit through flame can be changed into water, as can be read in Boerhaave's *Chemiae*. Wherefore it is no wonder that the spirit of wine can be burned from the steaming aeolipile, and therefore it is not rightly concluded from the spirit of wine to water, which I pointed out previously, contrary to Wolf (Para. VII). However many drops of spirit of wine I have burned up thus on burning iron without inflammation, always a small portion of earth remains. But this little piece contains fixed phlogiston. Wherefore at first it wholly exhales liquid, the appearance of soot or carbon springs forth for a moment, then it changes into white ashes. When on the other hand I investigated in a similar manner a drop of pure water, a portion of earth remained, which was not burned. Therefore it contained no phlogiston. However, I confess, several times (although rarely) in drops of water that I used a small portion of earth remained glowing, perhaps a little dust of the wood coals over which the iron vessel was held having gotten into the water, which is able to appear after a quick movement. Perhaps nevertheless another cause exists which I do not know, therefore I conclude nothing from this phenomenon. I am able to do so if the next experiment is performed.

XIX

I show a new method by which the *most perfect goodness of alcoholic wine* can be determined.

The learned men know that such a mark of character of great goodness is to be desired in alcohol: that which in no way is affected by heat is also thought to be of the greatest use in chemical solutions. That common method which the medicine vendors use for lighting ashes with wine spirits works upon the said defects so that the deed does not appear to be a confutation. However, the brilliant Parisienne chemmist, Gothofred the younger, describes a praiseworthy method for the whole thing in a writing for the Academie Royale Parisienne in 1718, in which the spirit of wine is evaluated by measuring the quantity of superfluous water, burning it (of course) in a narrow cylindrical vessel placed in cold water, for after the final fire the water left in a cylindrical vessel may be reduced for measurement more easily than in any other. Nevertheless not even with this very accurate proportion is it determined, although granted that in common practice his method suffices for the most part. For burning spirit of wine heats the water mixed in it, and by this same heat the greater part of it diminishes into exhalation. Gothofred himself in his other calculations in his proposed commentary thought that very pure alcohol is half part water but for my part I do not think so. Because if I held such alcohol in a torch the flame burns very slowly and from its vapor water can be collected. For alcohol, while it is burned, is not made pure, but truly is destroyed and is dissolved into its primary ingredients of mixture—water and pure phlogiston. Therefore water is not drawn from the spirit through flame, but the whole spirit is changed and converted into water, as I have said (XVIII). Similarly compared is another thing from his thesis: when quicklime begins to increase in the distilling spirit, demonstrated overabundantly by its watery proportion, so quicklime purifies in the same way the spirit of wine for a certain portion but destroys the greater portion. Therefore, so that we might be certain that nothing in the spirit of wine clings to excess water, it is necessary to arrange the thing so that at the same time that the spirit flies off the water is fixed and impeded in its evaporation.

It is so done with a degree of heat which has alighted glowing iron. For if in the sprit of wine a little water is mixed, this after the conflagration of the first is completed will cling or is moved long and evidently enough so that it can be seen under the form of little clear globes in the glowing spoon. So the pure spirit will be totally consumed so that no liquid remains. From this you are certain that the spirit of wine is pure alcohol. A drop of it dropped upon a glowing iron vessel must catch fire, for if after the flame is spent nothing remains of water, that is the best spirit nor is it able to hold any other impurity. And such a spirit in the Reaumur thermometer rising *encheiresin* more easily for giving judgment on the solutions of bodies makes the decision more certain.

XX

Similarly it happens in the same way with other and greater spirituous compositions in the described liquors. If a drop of spirit of sal ammoniac for example prepared with the spirit of wine is placed on a burning iron vessel in some way the first outside is covered, through itself probably. It is not burned, but the large globe makes much foam as if supported by tenacious bubbles and it exceeds the mass of the original drop in the vicinity of some hundred each. When, however, this drop is set afire by a flame from a flap of paper moved towards it, it burns as does the spirit of wine. After the last flame it leaves a very clear drop of water fixed for a long time in the fire. But if the spirit of sal ammoniac was prepared with urine and quicklime and water in a manner similar to that described above, this never would take fire, but nevertheless foamed and formed large bubbles, which disappeared after a while. It left a particle of water resting in the hot vessel for a long time. In several other saline liquids I tried similar things, and with few exceptions I saw the same phenomena, in the reckoning of which I will not be long, because nothing now can be taught unless the known nature of simple water is worthwhile to tell over.

XXI

If a drop of olive oil or some other fatty material is put into a very hot iron spoon it never is rolled into a ball but widely adheres to the glowing iron as if it moistened it. If it is put on a fire, in a moment of time (even without the external flame moved towards it), it emits a flame and a great denseness, in which a little while after vanishing it leaves copious black carbon in the hot vessel. Afterwards the image of a coal grows red, and shows copious white embers (earth, in truth).

XXII

Water does not boil in the heat of glowing iron (XV. No. 1) or, what is the same thing, the air leaving does not form bubbles. Both of these phenomena are possible either because the air enclosed in water is not emitted, or (if it leaves) it flees insensibly. In the first case it is necessary that air be fixed with water through fire in some violent way. In the latter clearly the viscousness of water is such in that immense heat that it may not require a bubble to rise. This also is true, as the following experience shows: if you apply a cold body—for example an iron staff—to drops of water clinging without any boiling motion in the fiery heat of glowing iron, or if you move a cold pebble towards it, or even if you drop a water drop from the burning metal swiftly into another less heated vessel, this quickly will boil, and with a boiling motion it vanishes swiftly into the air. Whence it is established that water in great heat does not give up its dry air, but retains it, since after this if the heat is diminished to the point that it can be let out, then the air (clinging fixed with the water between the spaces of water in great heat) is yielded.

XXIII

It follows that in the great heat of glowing iron not all the liquid but only the simple water and the more fixed air is made so. For the spirit of wine swiftly disappears. On the other hand the composition is inflammable, with a water portion left over (XVII). The spirit of sal ammoniac equally with wine as with urine is expanded, inflated, burned in its born essence, swiftly evaporated—truly it discharges the more fixed water contained in itself (XX). Olive oil needs to be protected from the flame. It is not changed into a globe. It is attracted by the burning iron. It is very quickly changed into carbon and ashes (XXI). But simple water alone, or that which does not cling in other liquids to that water mixture, is rounded into a globe by the fire. It does not boil, it shines and for such a paucity of matter clings fixed for a very long time. That same water there fixes the enclosed air. For by water it is done, rather than by the vehemence of the fire, thence it is clear, because the spirit of sal ammoniac in such a heat does not fix its contained air, but permits its great expansion in very ample bubbles (XX). However, we conclude that some air can be fixed because certain minerals, namely limestone then melted slabs next a red cinnabar made from lead solidifed a long time by fire then lime of antimony and perhaps several similar ones, are made heavier by fire. We know absolutely that the acquired weight from fixed air in the mixture can be determined through the Hales experiments in *Stat. Veget.*

XXIV

Therefore, it has been sufficiently shown that water made volatile is increased with degrees of heat until it comes to that point at which water boils and all very swiftly is evaporated. Then truly if the heat excites more strongly I diminish the volatility of that same water and increase its fixation by the added heat. My hope now will be to answer the same objection which can turn away that whole observation: obviously in a great heat water tends to evaporate less, not because it is more fixed when in such heat but because the expelled air does not require the atmosphere to be made lighter by the exhalation of water particles lifting into the air. For there is a certain hydrostatic law: a lighter body in a specific fluid, with a specific gravity, that has great inequality in the proportion of the weight of the fluid to the ascending body ascends with greater swiftness and force. Which thing can be seen by all: water in an atmosphere very rarefied through heat ought to be evaporated less swiftly. But in truth it is evident to me that this objection may be made of nothing in the case presented to evaluate. For (1) it has been sufficiently demonstrated by distinguished Hamberger in *Phys.* #477, that the exhalation of vapors in no way is done according to hydrostatic laws; (2) not yet have we explored to what degree the atmosphere can be rarefied in the dry heat of glowing iron; (3) in the same degree of fire mercury exhales, much heavier than water; (4) a flame in this degree of fire can be aroused, as we see, from the spirit of wine (XVII, No. 2) or from the spirit of sal ammoniac (XX) and from fat (XIX); where there is a flame, however, it is necessary that sufficient air enter there; (5) it has been shown besides (XIII) that water expires in the Boyle vacuum. And the learned Krafft observed concerning the exhalations of water in free air and in vacuum, that there is hardly any difference by reason of the quantity given in time. If therefore water in a place empty of air and coolness exhales swiftly, I do not see why it could not ascend in equal swiftness in free air if rather rare and heated. Which reasons contributed also dissolve the opposing fact, so that by nothing can the verisimilitude be overcome. Nevertheless I dig out as the whole deep basis for my argument another experiment I have investigated in which it is proven with certainty that a drop of water in the heat of glowing iron is made more fixed not because of the atmosphere's failing but from the action of the fire. Namely: either a little piece of lead or tin is put into an iron spoon glowing with light. It quickly melts there and is spread. To this liquid metal of lead or tin carefully place a drop of simple water through the glass tube described above so that it will not fall on the convex surface of the metal. You will see this drop, hanging over the lead, dispersed within 6 or 7 seconds. Which water if joined to the lead in the bottom of the iron spoon and therefore layed down in the same atmosphere, it remained more than 34 seconds. Also if one places another drop of water in that glowing vessel that is put so that it is touched to the lead and so it will not hang over it, you will observe that as the surface of the lead touches it, it flies away with a light motion, and it emits a noise as if the body were dashed against something cold, and then more swiftly with many that if it does not touch lead. Truly, very surely, no one knows the nature of these things. The heat of the melted lead is much less than of glowing iron, wherefore the melted lead in respect to the glowing iron is called a cold body. And above this cold or medium warm body, a drop of water more swiftly exhales than if it is placed above burning iron, even though the ratio of the atmosphere on both sides will be very perfectly equal. Therefore the rarity of the atmosphere is not the cause of a greater fixation of water in a larger fire.

XXV

Therefore, I dare to propose this new thermometry to the physicists and learned men of Chemistry, so that in measuring large degrees of heat it will be done equally certain as with an ordinary thermometer in measuring lesser degrees. For it is known that until now with those thermometers that we used, they indicated the degree of heat through the degree of expansion, as much as the enclosed liquid undergoes. Formerly it was established that all the liquid especially is incited into a motion of boiling and its heat is expanded as swiftly as its degree of expansion and cannot be measured better. From such a thing water and the spirit of wine boil on a small fire to the point that they are not able to measure great degrees of heat. Just then the mercury was added to these, for which often a greater fire is made before it will boil.

But in truth also mercury was despised and shunned for this too much, although from its moderate expansion the heats of metals and melted salts we might be able to find out. Therefore in its place learned Muschenbroek substituted another instrument, which he called the *pyrometer*, whose construction is such that it is a solid body, such as an iron rod, as long as its extended length shows in proper indication those degrees for various degrees of heat. Since such a very ingenuous instrument is greatly used in physical things and it can measure higher than all others, so the remaining kinds of thermometers can be altogether tested by this thermometer, and also a convenient degree scale is assigned to each. Where however a different degree must be measured in vessels of burning or melted metals or salts, because of its structure the pyrometer is applied with difficulty, in which cases I propose that a method more deserving be devised.

XXVI

1. So let the vessel be hot iron and in addition let it have the degree of heat of boiling water. Onto this a small drop of water put out boils and flys apart completely within one second, or even faster. This lowest and first degree of heat is agreed upon and measured.

2. It is established that with lead in a small trough, of which the greater the mass the better, into which you pour a pure water drop of an equal magnitude as before, the water will not boil, but is evaporated within 6 or 7 seconds.

3. The lead is excited by the heat so that it boils [the Germans say *treiben* (drive, push)]; a drop of water then let loose will not fly apart until after 14 seconds or more.

4. The iron glows so that the whole is made alight: a drop of water in that will be retained through 30 seconds.

5. If then you stir the iron in a furnace *anemio* with a big fire, a similar drop will be fixed for 34 or 35 seconds. And in this degree of heat the iron remains until melted. For if you detain for several hours a little iron vessel in this same high heat, so that it is made near to melting, nevertheless the water will not be able to be made more fixed in this unless at the most it is detained for 35 seconds.

6. If in this same iron vessel greatly heated and nearly melted you place one grain of mercury, this flies apart within about 18 seconds. However, in those that I used, three water drops were equal to two grains. Therefore two or three grains of mercury flew apart within 12 seconds. Therefore it is in that degree of fire that the fixation of water is to the fixation of mercury as 35 to 12 or as 3 to 1 in nearness. That is remarkable enough. For with the first, as they say, nothing has been learned easily—to be able to make a drop of very pure water without any mixture as in free air, with the action only of the approaching fire, ever is made repeatedly more fixed than an equal quantity of mercury.

And also with this second method many bodies of metals, earths, salts, and minerals can be explored as to the degree of heat. For as in former times we measured from space in the use of the thermometer, so here in time.

XXVII

However the fixation of water does not increase in infinity with the increasing heat. For iron and copper as long as they burn fix water. Especially in fact they did not fix it more, rather they changed that very drop of water brought together with them with such violence into an elastic state, that such impetus was perhaps not to be found elsewhere on the surface of the earth. Why do you wonder therefore that nature rejoices in changes as the beautiful cycle of all things? Certainly water at the degree of heat 32° Fahrenheit (when it forms ice) is a solid and rigid form and is the same if that heat increases. It is dissolved not into a liquid form, but is changed into elastic vapors, and grows in volatility to the degree 212 of the same thermometer, evidently when it boils. And the fixation again increases with the degree of heat of glowing-with-light iron. Then again it was fixed with the iron at a great volatility and elasticity. Who however, determines what was done formerly? Meanwhile from these which formerly I have investigated, it is plain that in the scale of heats, a new fixed point ought to be put, namely that of iron glowing-with-light. When therefore formerly none other than three fixed and constant points of heat were known, namely of salt ice, of natural congelation, and of boiling water, to these this fourth can be joined not unsuitably at the end. For nothing more certainly helps scientists than to have a constant terminal, from which you can measure others. This maxim of Archimedes: Give me the foot that I might measure (*da possim figore pendem*).

XXVIII

Readers, you do not see me advising that which I indicated in the two preceding paragraphs unless there are possible plans for a thermometer of its kind. How to put together a table of many mineral bodies having their degrees of heat wasn't permitted formerly among my other works, my leisure being small. Meanwhile I show that many chemical phenomena proven by this method are able to be explained. Pure earth and potter's earth evidently never acquire great heat. For a baked dish or similar earthen vessel, heated for a long time and very shiny on a high fire in a furnace *anemio*, is made very hot but nevertheless it is not much hotter than water in the boiling state. A drop of water is fixed at a minimum when poured into such a melting pot, so that in a moment of time it boils and is evaporated. Again that which I asserted in XXIV is confirmed by such phenomena, namely that the fixation of water does not depend on the lightness of the atmosphere for the most part. For if the baked dish melting pot and the iron vase glow on that same fire, nevertheless a drop of water boiling in the former is evaporated quickly. However, in the latter it is fixed for a long time. Next the reason is clear also why pieces of earth cannot be found. They endure the fire whole if they are pure. Obviously because they do not attract fire unless to a certain degree and that is why such convenient instruments are made from these discovered minerals. A flintstone, however, made very hot (as much as melted earth) fixed a drop of water much longer. Moreover, several times it seems to me that concerning the experiment described above on glowing iron—in various masses of heated iron (which by chance fortune had offered as an instrument for economic items) the fixation of water did not succeed and I marveled until I observed that these masses of iron had been spread over scores. For with those separated with a hammer, the undertaking soon succeeded. Wherefore scores of iron with common potter's earth and also with simple water assumed the same degree of heat, not greater. Silver has a melting heat less than that of burning iron, but never accurately determined. Alkaline salt glowing but not yet melted into a liquid will resolve distilled water, wherefore from these things and through this method nothing can be explained. But as soon as they are melted into a liquid, they make a drop of water dropped in very elastic and nearly give the impression of melted copper. Wherefore it follows that this salt is made very hot in a state of fusion, and it is clear from these things why the flowing motion of the said chemicals is seen: because it is obvious that they incite an immense fire in a brief period of time and they pass beyond extreme so that even metals are not able to dissolve because of another reason than maximum heat alone. Concerning boiling oil perhaps an exception must be set down from the general rule, since they drive off with great impetus water poured onto them. Whence nevertheless it is not probable that it has same degree of heat as melted copper. About these things and several others which I tried by this method, I am now (when it is permitted to write about these things) more certain than at that time. For more than any other thing it seems probable that the magnitude of the water drops could not always have been very perfectly equal.

XXIX

And also, while I might occupy myself more strongly with the phenomena in these descriptions of water in fire, more often I tried to find out also whether water in this state was impeded in its evaporation by fire according to the Amontonian rule. The truth remains that even though water has been more fixed nevertheless it stays at the same temperature. Easily one sees (no matter who) that through the thermometer used it can be determined when their masses and the proximity of the very hot metal will not permit their application when compared with a few water drops which have been tried. Nor have I described all of the methods which someone ingenious has supplied so that I might make use of these toward one goal. I tell only about those which succeeded. It is known that mercury cooked in water does not fly apart, even if a very strong fire is applied, unless after the water touching it has evaporated. When therefore I mixed a very small drop of quicksilver with a drop of water in a heated iron spoon, immediately the mercury was seen in infinitely small balls distributed throughout the water, as if it were dissolved. But afterwards when all the water was evaporated, the mercury showed itself conspicuously in the spoon, and it flew apart following the water. When therefore I showed formerly (XXVI, No. 6) that mercury in the heat of burning iron is more volatile than water, the same is true when it lies in water. It does not fly apart before the evaporation of this. Water is seen in its greater fixation, nevertheless, not to reach the degree of heat which is required so that mercury might be made volatile. Therefore it is seen that water fixed on glowing iron is not any hotter than boiling water, hence I conclude that the Amontonian rule probably remains true in this case also.

Similarly, I poured drops having remained for some seconds in the heat of burning iron from the iron spoon immediately into another vessel. And in that way with repeated diligence I collected a great abundance of this water consumed from that, so that I might inquire whether its sensible qualities had been altered. However I acquired nothing by this operation unless very pure and clear water which was changed through cold into ice as well as other water. It dissolved salts, it did not cling to oil. On the other hand, if from that very hot ladle I poured such drops over various refines and other bodies not easily soluble by water, I observed nothing that I could not expect from boiling water. Whence again I conclude that the rule of learned Amontonius is probably true, unless the water can be heated to a certain degree. However, my investigation is debated for truth.

XXX

Before I continue to the following things, I ought to describe another kind of experiment with common water upon a strong fire, about which at first I intended to investigate whether a small portion of earth always remains manifest after a drop is evaporated in a glowing iron spoon, and whether this earthy portion is born from the water itself or whether it ought to be ascribed to other causes. I prepared for myself some little twisted glass vessels, which had a circumference about the size of a chicken egg. Into one of these washed and purified twisted vessels I accurately dropped out one ounce of very pure distilled water. Into the mouth of the vessel I put another glass receptacle firmly without mastic but nevertheless sized to the vessel. I made it firm without mastic so that I might have less fear of the glass breaking. Thus, I set the so-constructed vessel in the said furnace. Learned Teichmeyer gives a description of it in *The Chemistries*, and more accurately B. Schultz in his posthumous work (*Der Chemischen Bersuche*, under the name taken from the covers) where it is evident that the jar or iron or baked earth was placed in a furnace *anemio* so that its aperture was not on the top but to the side of the furnace, just as the domestic arch is usually placed in the furnace. Into this jar (which in my furnace was fashioned from earth)

I placed over a small portion of mud a twisted vessel, and swiftly thus I made it firm so that it could not easily be dislodged or turned around. Before I put the vessel into the furnace I had already made a fire, so that the jar with the mud might be heated lightly. Also it had been turned correctly, set, and placed immovably. I placed an iron vessel within the furnace, and I built up the fire as large as I could so that within a brief time the pót and mud began to glow with light. And after a brief time the twisted vessel glows and quivers. While the fire increases so swiftly, a good quantity of water in the semblance of vapor is propelled with impetus into the receptacle, where it is condensed at last into water again. Especially when the heat comes to the degree when now the jar and mud and twisted vessel deeply and utterly burn, then the water which is in the bottom of the twisted vessel jumps up and (because of the described structure of the furnace one can observe it with the eyes) it is evaporated gently and slowly, and at last is very fixed. Thus, so that on this fire about a drachma through half an hour and more perhaps without any fume or vapor and without any motion of boiling appearing. When then you build up the fire so that the glass melts and seems to approach near to fusion, then the twisted vessel with a great noise suddenly flies apart and is diffused into fragments. The water from the broken vessel flowing over the glowing mud extinguishes it with a hiss. For truly if you take the broken bottom of the vessel from the furnace and consider it with attention, you will find a considerable portion of white earth.

XXXI

This experiment (XXX) succeeded correctly for me many times but it lacked success several times for it is difficult to make the twisted vessel firm so that it stands completely immovable in the furnace. For if it is agitated or moves even a little then water clings to the bottom and to the sides of the burning vessel so that this is now ruptured before it will have reached full heat and eludes the hope of the experimenter. I declined to omit this experiment, imperfect and not-repeated-enough as it is, until I thought of investigating water by a better and more certain method—in enclosed heated vessels. Perhaps nevertheless it will give an opportunity to others, something for better experimenting, for which reason I am pleased to add it. However it seems from this that now I have probably taught further above (XXVI) how water committed to a larger fire than of burning iron (for example fused copper or fused glass or beginning to be melted) does not remain more fixed, but at most the elastic vapors are perhaps changed into elastic air. For the same reason a new probability of the Amontonian law is clear: that water obviously even if it is made more fixed in such a fire is nevertheless not hotter. Because after the broken vessel extinguishes the underlying glowing sand with a hiss, plainly besides the cold water (or lighter-heated) is able to hold itself, wherefore in its nature and its heat it is not seen then to be much changed. There will be another occasion, however, for speaking about the fixed part of earth remaining in the bottom.

XXXII

Now the reason for other changes is to show in what manner water, a body in a very fluid state, is made firm. But for this end it is not yet applied to common experience. From Aristotle, whose hypotheses I have shown today in two former experiments (III), it is known that the possibility of the change of pure and simple water into a firm visible body could be demonstrated. Another is Boyle, in whom if the method is correct, it is established that water is frequently changed by distillation into true fixed earth, see his translation *On the origins of form*, experiment 9. Another is the marvellous man Helmont, who in wondrous genius teaches that all solid parts of plants and perhaps of animals as well are born from very pure water alone. He fed a tree of five pounds with the nourishment of water alone to the weight of 169 pounds, see his tract *On the complexities and mist. Elements*, section number 30.

XXXIII

Again the experiment of Boyle succeeds from no one's opinion, since it is perhaps length of patience which one requires to carry out the annoying labor. Perhaps also because on the authority of Boerhaave, who accuses it of fallacy, since he affirmed that an earthy portion left by distillation in single atmospheric parts and an abundance of terrestrial dust (which is always flying through the air of a chemist's laboratory and adhering continuously to the vessels and to the liquids) must be its origin. With such an objection the Boerhaavian idea has not very much probability, because such a portion of terrestrial dust is required for explaining the Boyle phenomenon. It never flies in quiet air, nevertheless if one wishes to investigate more deeply the truth or falsity of its presence, I urge him to commence with the distillation not in large vessels but in small, not in abundant water but in a small or medium portion of it, and not on a slow or little fire but on the biggest one on which vessels can be heated safely, in the method which I have described (XXX). Do it thusly because of an enlarged fire a greater portion of earth is anticipated and because a better atmosphere and more certain dust can be enclosed in a small vase. For me certainly there always remained in the water so tested some white earth, even much more abundant than it is permitted to deduce from the atmosphere.

XXIV

However the experiment of Helmont on the growth of plants through water alone repeated infinitely always correctly succeeds, nevertheless better in one kind of plant than in another. Learned Woodward opposed these experiences set down by Helmont with great zeal in the *English Philosophical Transactions*, year 1699, number 253, where he purposes to demonstrate that the plants do not increase from water, but from the portion of earth which usually always occupies the water. And which, if it stands in a quiet way, gently is put aside in the form of living wood. For true vegetable material has those living wooden stalks. And he shows in this that plants are nourished more richly with water, with which principle they overflow, much more as it pleases a plant to grow from such peculiar material (so created by God). In Woodward more modern physics is applied to it, as is clear from the systems here and there. Nevertheless the endeavor of the illustrious Eller outdoes it and he also shows in his communication to the Royal Academy of Berlin that this green wood matter is least common with water, but is sprung from a very subtle phlogiston mixture through the solar rays.

XXXV

It is also permitted to consider in passing that Woodward, while he denies Helmont, will not show the state of the controversy and would combine the two propositions with themselves, the one of Helmont, who asserts that the solid vegetable parts are born from water. But in the same piece in which he describes the experiment on the willow, he denies with abundance the Aristotelian *dictum* of transformation. If, therefore, Woodward thinks that the earthy parts of vegetables also are born from the earthy particles of water, it is just so with Helmont, because it is easily conceded that there is a very small quantity of earth in all plants, this which before was concealed in the water. Whence, however, is born the great long part of solids remaining in vegetables which are not earth and also do not lie in such form in water? Therefore, Woodward did not distinguish between a solid or a firm body in general, and a terrestrial body in particular. On the contrary, if he had examined that same green material as he calls earth and which he thinks is intermixed with all water, he could easily have seen that it was not earth, but richness or fat.

And also while he concedes that structures do not increase from such earthy matter, he proposes to himself that these are nourished from earth. This is a contradiction.

They are also less distinguished by him in turn than the great Boerhaave does, *a man beyond my praise, and one of those who ought to be venerated since through them it appears not to be a vile thing to be a human.* However as Boerhaave was the leader of Europe it easily happens that what he said wrongly and also that which he advanced less accurately are accepted as axioms. Among these I refer to his theory which he zealously teaches on the solid substance of vegetables and animals, to which very pure earth is joined by glue alone—not in the way in medical *institutions*, but in chemistry—and they see who wish to think that the proposition is not far from truth that in lesser animals there is no sensible earth, little in the greater and there it clings only in the bones. However, in cremated bones changed through fire the lime is not the same matter as of living fibres, but rather it fills the interspaces of the vital fibres, dead matter in that living body and remaining destitute of life; however the solid living fibres have been destroyed by the magnitude of the fire.

Bodies are born from the albumin of an egg without any addition except heat, membranes, cartilages, bones, and true solid parts. For also if I do not deny that something of earth is required in the mixture of solid parts in the vegetable and animal kingdom, nevertheless I think it can be demonstrated in respect to those left more formally than materially for the necessary firmness, and it profits little as it would be an enemy to life because it is lacking in elasticity. However in this passing note I have said that I might show that the reasonings of Woodward detract nothing from the Helmont experiments.