

XXI. *History of the Congelation of Quicksilver.* By Charles Blagden, M. D. F. R. S. *Physician to the Army.*

Read June 5, 1783.

THE late experiments at Hudson's Bay have determined a point, upon which philosophers not only were much divided in their opinion, but also entertained, in general, very erroneous sentiments. Though many obvious circumstances rendered it improbable, that the term of mercurial congelation should be five or six hundred degrees below 0 of FAHRENHEIT'S scale, as had been at first supposed; yet scarcely any one ventured to imagine that it was short of 100°. Mr. HUTCHINS, however, has clearly proved, that even this number is far beyond the truth; and that quicksilver freezes in a degree of cold not exceeding that which sometimes occurs in the northern parts of Europe, and frequently in the more rigorous climates of Asia and America. It now appears, that quicksilver, so far from containing any essential principle of fluidity, does not differ from some of the other metals, in its melting point, nearly so much as they differ among themselves; and as it is malleable in its solid state, and after calcination recovers its metallic form by heat alone, without the addition of inflammable matter, there can be no doubt but it must be ranked among the perfect metals, which, therefore, arranged accord-

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ing to their specific gravity, are these four: platina*, gold, quicksilver, and silver.

In the general progress of science, it is always useful at intervals, and especially when any considerable advance has been made, to look round and contemplate the prospect left behind. Thus our actual situation is more distinctly comprehended, and a better judgement may be formed of what remains to be done. For this reason, I thought it might not be unacceptable to the Society, if I were, on the present occasion, to lay before them an account of the different observations and experiments I have been able to collect, relative to the congelation of quicksilver; especially as many of these are recorded in books not easily procured, and in languages little understood by the learned of this country. I shall begin with the various attempts which have been made to render this metal solid by frigorific mixtures, and then enumerate the many instances in which that effect has certainly been produced by the natural cold of the air.

P A R T I.

§ 1. IT was undoubtedly M. JOSEPH ADAM BRAUN, Professor of Philosophy in the Imperial Academy at Petersburg, who first, upon decisive evidence, established the fact, that quicksilver can be made solid by a diminution of its heat: but,

* Unless, indeed, the irreducible black powder, obtained by M. TILLET, should be thought to place platina among the imperfect metals. See Mem. de l'Ac. Roy. des Scienc. 1779, p. 404, &c.

as in most discoveries, much depends upon accident, so, on this occasion, M. BRAUN undertook the experiments for a very different object from that which presented itself in the course of them, and at the suggestion of another person. This gentleman was Dr. JOHN ERNEST ZEIHNER, Professor of Mechanics in the same Academy, who having repeated FAHRENHEIT'S experiments with frigorific mixtures in Germany, before he came to settle at Peterburg, wished to try whether they might not be prosecuted further in the great natural cold which sometimes prevails in that city. Illness prevented Dr. ZEIHNER from carrying his ideas into execution; he therefore communicated them to Professor BRAUN, who was already much conversant in thermometrical experiments, and engaged him to take up the subject of artificial cold whenever the weather should be favourable for this purpose. A proper opportunity occurred on the 14th of December, 1759, O. S. the thermometer sinking in the open air so low as -34° of FAHRENHEIT'S scale, which we now know to be within a few degrees of the point at which mercury freezes. M. BRAUN accordingly prepared a frigorific mixture with *aqua fortis* and pounded ice, by means of which his thermometer was reduced to -69° , lower, by almost 30 degrees, than it had fallen in any preceding experiments of this nature.

Part of the quicksilver had now really congealed, yet so far was M. BRAUN from entertaining a suspicion of that sort, and so entirely were his views directed to another object, that he acknowledges he had well nigh desisted from all further trials, content with having thus much exceeded such eminent philosophers as FAHRENHEIT, MUSSCHENBROECK, and REAUMUR.

Animated, however, by the hope that a still greater degree of cold might be produced, he entered upon the experiment
anew;

anew; and all his pounded ice being expended, he was fortunately obliged to substitute snow in its place. With this fresh mixture he had the satisfaction of seeing the mercury in his thermometer sink to -100° , and in successive experiments to -244° and -352° . Surprised at so unexpected an event, he drew the instrument out of the mixture, and carefully examined its bulb, to see if it had received any injury; but he found it perfectly entire, and moreover perceived a much more unexpected phenomenon, that the quicksilver was fixed, and remained immoveable above 12 minutes. On repeating the same experiment with another thermometer, graduated no lower than -220° , all the mercury sunk into the ball, and became solid as before, not beginning to re-ascend till after a still longer interval of time.

From these appearances the professor very justly concluded, that the quicksilver in both instruments had been fixed or frozen by the cold; but as the evidence was not yet complete, he only ventured to propose the congelation of mercury as a *probable truth*, at the next meeting of the Academy held three days afterwards; and in the mean time was making preparations to acquire more palpable proofs of the fact. The thermometers ordered with this view were not ready till the 25th of December O. S. when, in company with the celebrated ÆPINUS, Professor of Physics, he performed the experiment with similar materials, and as soon as he found the quicksilver immoveable, broke the bulb of his thermometer. Now all his doubts were removed; he obtained a solid shining metallic mass, which extended under the strokes of a pebble, in hardness rather inferior to lead, and yielding a dull dead sound like that metal. Professor ÆPINUS was occupied at the same time in similar experiments, employing both thermometers and simple tubes

tubes of a large bore; with which last he remarked, that the quicksilver in them fell sensibly on freezing, and assumed a concave surface; likewise, that the congealed pieces would sink in fluid mercury; all evident proofs of its great contraction. These observations were frequently repeated during the winter, with some variety in the circumstances and phænomena, by Professor BRAUN and many other persons; I find M. LOMONOSOV, Professor of Chemistry, the Apothecary-general MODEL, Mess. KRASE, HIMSEL, and POISSONNIER, quoted on different occasions as witnesses, they having all either performed or assisted at the experiments. Such evidence one would have imagined sufficient to place the fact beyond all controversy, and render the congelation of mercury one of the most acknowledged truths in natural philosophy.

It may not here be improper to remark, as an additional proof how much we are indebted to accident for discoveries; that if Mr. BRAUN had chanced to begin with a spirit thermometer instead of a mercurial one, we might very possibly to this day have remained ignorant that quicksilver would freeze. For since, to judge from Mr. HUTCHINS's experiments, the former would have sunk but a few degrees in the frigorific mixture, it is not improbable, that the professor, discouraged by such a failure of success in the immediate object of his pursuit, would have relinquished all further attempts of this nature.

When the season for experiments requiring cold was past, Professor BRAUN employed himself in drawing up a general account of such as he had then made, which he communicated to the Petersburg Academy on the 6th of September, 1760, O. S. and printed soon afterwards as a separate dissertation*. Of this so copious an extract, by Dr. WATSON, is already in-

* De admirando frigore artificiali dissertatio.

serted in the Philosophical Transactions*, that it would be improper for me to dwell upon any further particulars. I will only observe, that though many of the circumstances mentioned by M. BRAUN were not first remarked by himself, yet the dissertation is composed entirely in his name, all the other gentlemen very generously giving up their part to him who made the original discovery, and undertook to put the whole in a fit dress to appear before the world.

Five years afterwards, Professor BRAUN again addressed the publick on the same subject, under the title of "Supplements" to his former dissertation †. Here he declares, that since the first discovery he has suffered no winter to elapse without making similar experiments, and never failed of success in freezing the quicksilver, whenever there was a proper degree of natural cold, which he states at -10° , in order for the experiment to be complete, though some commencement of congelation might be perceived when the temperature of the air is as high as $+2^{\circ}$. He confirms all his former observations, and adds many others to illustrate them; among which two are very important, as coming nearer than any yet known to ascertain the real contraction that quicksilver suffers in becoming solid. At the same time it must be confessed, he has not rectified any of his former mistakes: he retains the same groundless opinions relative to the freezing point of the quicksilver, the prodigious cold generated by his mixtures, and the explanation of various phenomena, which depend upon very different principles, from those to which he assigns them.

* Vol. LII. p. 156.

† Supplementa de Congelatione Mercurii, Nov. Comment. Acad. Scient. Imperial. Petropol. tom. XI. p. 302. & seqq.

The general state of M. BRAUN's experiments is, that with the above-mentioned frigorific mixtures, and once, when the natural cold was at -28° , with rectified spirits and snow, he congealed the quicksilver, and discovered most of its properties in a solid state, especially that it is a real metal, which melts with a very small degree of heat. But not perceiving the necessary consequence of its great contraction in freezing, though aware of the fact, he perpetually confounded the diminution of its volume from this cause with that which is simply the effect of cold. Hence he considered, as the commencement of congelation, what was, in reality, its extreme term, or the utmost contraction which the whole would suffer in becoming solid. To this, indeed, he scarcely ever attained, owing to the various impediments that occurred from adhesion of the quicksilver in the thermometrical tube, hollows left in the bulb as it froze, portions of the mercury remaining uncongealed, and many other causes. All these being by their nature very irregular, his supposed freezing point came to be extremely uncertain, and several anomalous appearances were occasioned, which could not possibly be explained upon his mistaken supposition; but, notwithstanding such errors, the greatest part of our present knowledge on the subject of mercurial congelation is to be found in the writings of M. BRAUN, who may therefore justly be stiled the father of this branch of science.

In his supplementary treatise, the Professor engages to continue his researches, and to lay the result of them before the Academy, if they should lead to any thing new. But he did not live to accomplish his design. His original dissertation was re-printed, and the supplement first published, in the XIth tome of the *Novi Commentarii Academiae Scientiarum Petropolitanae* for the year 1765. This volume did not appear till 1767,

and Professor BRAUN died the following year. These two treatises contain the substance of all his observations; but several further particulars, relative to the discovery, may be collected from the Philosophical Transactions*, the History of the French Academy of Sciences †, and other literary publications of that period.

§ 2. Professor BRAUN, in his first dissertation, expressed a very commendable wish, that the experiment of congealing quicksilver might be repeated in other countries, and laments, in the supplement, that nothing of this kind had been done. It was not, however, till the year 1774, that his assertions received any sort of confirmation out of Russia, and then by a mode of experiment which did not seem to promise much success. M. JOHN FREDERIC BLUMENBACH, then a student of Physic at Gottingen, now Professor of Medicine in the same University, observing the intense cold that prevailed there in the month of January that year, took the opportunity of exposing some quicksilver to its action. As the original account of this experiment was given only in a single number of the Literary Journal of Gottingen ‡, in the German language, I will here translate it as exactly as possible, several of the circumstances being very remarkable.

“On the 11th of January,” says M. BLUMENBACH, “at half after five in the evening, I put three drams of quicksilver in a small sugar-glass, and covered it with a mixture of equal parts of snow and Egyptian sal ammoniac. This mixture was put loose into the glass, so that the quicksilver lay

* Vol. LI. p. 670.

† Hist. de l'Académie des Sciences, 1760, p. 26.

‡ Göttingische Anzeigen von gelehrten Sachen. Stück 13. Jan. 29, 1774.

“ perfectly

“ perfectly free, being only covered by it as with pieces of ice ;
“ the whole, together with the glafs, weighed fomewhat above
“ an ounce. I hung it out at a window three ftories high,
“ upon a fmall roof facing the weft, fo that the glafs was
“ freely expofed to the north-weft ; and I mixed with the fnow
“ upon which it flood two drams more of fal ammoniac. The
“ fnow and fal ammoniac in the glafs foon froze in the open
“ air to a mafs like ice : no fenfible change, however, appeared
“ in the quickfilver that evening ; but at one in the morning it
“ was found frozen folid. It had divided into two large and
“ four fmall pieces ; of the former, one was hemifpherical
“ and the other cylindrical, each feemingly rather above a dram
“ in weight ; the four fmall bits might amount to half a fcruple.
“ They were all with their flat fide frozen hard to the glafs, and
“ no where immediately touched by the mixture ; their colour
“ was a dull pale white, with a bluiſh caſt, like zinc, very
“ different from the natural appearance of quickfilver. I wiſhed
“ much to break the glafs immediately, and to try how theſe
“ bodies would bear the hammer ; but defiring rather to have
“ witneſſes of ſuch a rare phænomenon I refrained. The ſpirit
“ of wine, in an excellent thermometer made by BRANDER,
“ flood at this time 10° under 0 of FAHRENHEIT’s ſcale, which
“ was the cold of Upfal in 1740. Next morning, the 12th,
“ about ſeven o’clock, I found that the larger hemifphere be-
“ gan to melt, perhaps becauſe it was moſt expofed to the air,
“ and not ſo near as the others to the fal ammoniac mixture
“ which lay beneath. In this ſtate it reſembled an amalgam,
“ ſinking to that ſide on which the glafs was inclined, but
“ without quitting the ſurface of the glafs, to which it was
“ ſtill firmly congealed ; the five other pieces had not yet un-
“ dergone any alteration, but remained frozen hard, as I had
“ obſerved

“ observed them in the night. I now made haste to call some
 “ of my friends, who came in time to see evidently all this
 “ with me. They were the younger Dr. VOGEL, and Mess.
 “ WEBER, WAGNER, and GRAUMANN. Toward eight o'clock
 “ the cylindrical piece began to soften in the same manner as
 “ the former, and the other four soon followed. About eight
 “ they fell from the surface of the glass, and divided into many
 “ fluid shining globules, which were soon lost in the inter-
 “ stices of the frozen mixture, and re-united in part at the
 “ bottom, being now exactly like common quicksilver.”

In this experiment at Gottingen it is not a little surprising, that the cold should have proved sufficient to effect such a congelation of the quicksilver. A mercurial thermometer would probably have stood lower than M. BLUMENBACH's of spirits; but how much cannot possibly be determined, without knowing the strength of the spirits, and after what method it was graduated. At all events, the difference could scarcely have amounted to ten degrees, which would still give the temperature of the air at least 20° above the freezing point of quicksilver. Sal ammoniac with melting snow produces 32 degrees of cold; may we suppose, that it has a power of increasing the cold nearly as much when it and the snow are previously cooled below that point*? But if so, why was not the quicksilver congealed till after a period of several hours, since other frigorific mixtures begin to act almost immediately? Besides, there was not here the appearance of action, which consists in a solution of the snow, instead of its freezing into a mass. Or had the cold been greater where the mercury was placed than at M. BLUMENBACH's thermometer? The whole experiment remains

* See Bishop WATSON's *Chemical Essays*, vol. III. p. 138.

involved in such obscurity, that some persons have supposed the quickfilver itself was not frozen, but only covered over with ice, to which opinion, however, there are great objections.

It is worthy of remark, that Gottingen, though situated in the same latitude as London, and enjoying a temperate climate in general, becomes subject at times to a great severity of cold. This of the 11th of January, 1774, is one instance. I find others when the thermometer sunk there to -12° , -16° , or -19° ; and at Cattlenburg, a small town about two German miles distant, to -30° *. By watching such extraordinary occasions, experiments on the freezing of quickfilver might easily be performed in many places where the possibility of them is at present little suspected. The cold observed at Glasgow in 1780 would have been fully sufficient for that purpose †.

Dr. BLUMENBACH's description of the solid quickfilver differs so much from Professor BRAUN's, with respect to its colour and general appearance, as to require a particular explanation. Their disagreement, I imagine, was occasioned by a diversity in the circumstances of their experiments. Quickfilver crystallizes in becoming solid. In this property it resembles other metallic substances, as appears from many facts, and is elegantly exemplified in those curious cups which are formed by exposing proper masses of melted metal to the cold air till the outer part be sufficiently hardened to constitute a solid coat, and then letting out the internal fluid part, so as to leave a hollow in the middle. This concavity is found every where beset with metallic crystals, scarcely yielding in beauty and regularity to

* LAXMANN's *Sibirische Briefe*, p. 98. *Nov. Commentar. Petrop.* tom. VII. p. 396.

† *Viz.* — 23° on the snow, — 14° in the air. *Phil. Trans.* vol. LXX. p. 456.

the finest configurations of salts. In like manner, with regard to quicksilver, Professor BRAUN himself observed, that whenever it had congealed but imperfectly, and the fluid part was poured off, the solid surface which came in view was extremely rough, as if composed of many small globules. One of Mr. HUTCHINS's late observations exceedingly illustrates this matter; for he remarks, that when the fluid mercury was decanted off, in his tenth experiment, "the internal surface of the frozen quicksilver shewed very uneven, with many radii going across, some of which had heads resembling pins." Now in Professor BLUMENBACH's experiment, the quicksilver lying loose, except the flat side that touched the glass, could crystallize without impediment, and hence assumed a rough, and consequently a dead-white surface; whereas in those made by Mr. BRAUN, with tubes and thermometers, the metal being so much confined by the smooth glass, its surface was rendered of a high polish, not distinguishable in point of splendor from that of fluid mercury. Perhaps also, M. BLUMENBACH's quicksilver might have been made to look duller by some dirt or moisture collected upon it from the sal ammoniac and snow.

§ 3. In the notification of Professor BRAUN's experiments by the French Academy of Sciences, a sort of request is inserted, that proper persons might be sent by the Russians into Siberia, and by the English to Hudson's Bay, for the purpose of repeating them with every advantage of natural cold. I do not find that the Russians have taken any notice of this, and indeed with regard to them it would be almost superfluous, as scarcely a winter passes at Petersburg in which the weather is not severe enough for the ready congelation of mercury by artificial means. But in this country, the Royal Society, ever attentive

to the improvement of science, desired their late secretary Dr. MARY to make the necessary application to the Hudson's Bay Company; in consequence of which Mr. HUTCHINS, who was then in London, and going out with a commission as governor at Albany Fort, offered to undertake the experiments, and executed them in a very complete manner, as appears from the account published in the LXVIth volume of the Philosophical Transactions. In the months of January and February 1775, he twice froze quicksilver at Albany Fort; and in the first of these experiments, having broken his thermometer, he found, that the metal flattened by a fall of about six inches, bore to be hammered, gave a "deadish" sound like lead, and was finely polished on the surface. As Mr. HUTCHINS adopted exactly the method of Professor BRAUN, he observed the same phenomena, encountered the same difficulties from the sticking of the quicksilver in the tube, and cracking of his thermometer, and was equally at a loss with regard to the point of congelation. Still, however, this was the fullest confirmation that M. BRAUN's Dissertations had ever yet received; and it may be considered as a prelude, by which Mr. HUTCHINS acquired the experience that enabled him to succeed so perfectly in his last most decisive and satisfactory experiments.

§ 4. The account of Mr. HUTCHINS's success at Hudson's Bay was read before the Royal Society at the commencement of the severest winter that had been known for many years in Europe. Two gentlemen of different countries embraced this opportunity to attempt the congelation of quicksilver. The first was Dr. LAMBERT BICKER, Secretary to the Batavian Society at Rotterdam, who, on the 28th of January, 1776, at eight in the morning, made an experiment to try how low he could

could reduce the thermometer by artificial cold, the temperature of the air being then $+2^{\circ}$. He could not bring the mercury lower than -94° , at which point it stood immoveable; and upon breaking the bulb he saw with certainty that the outer part of the quicksilver had lost its fluidity, and was thickened to the consistence of an amalgam; it fell out of the bulb in little bits, which bore to be flattened by pressure, without running into globules like the inner fluid part.

Next day, when the thermometer stood at $+8^{\circ}$, he repeated the experiment with all possible exactness, after M. BRAUN'S manner; but could not obtain a greater descent of the mercury than to 80° under 0, and did not again break his thermometer.

The first account I saw of these experiments was in an extract from the *Altona Post**, in the German language, which Sir JOSEPH BANKS had the goodness to procure from Göttingen at my request; but as they are confirmed by Professor VAN SWINDEN in his *Observations on the cold of 1776* †, there can be no doubt of their authenticity; and they afford a proof that the freezing point of quicksilver cannot be lower than -94° , as it sunk only to that degree, and yet was in part congealed.

§ 5. The other gentleman who tried the effect of this severe cold in 1776 upon mercury was Dr. ANTHONY FOTHERGILL, at Northampton, and the account of his experiment may be seen in the *Philosophical Transactions* for that year ||. His

* *Altonaer Reichspostreuter*, 1776. N^o 24. Feb. 24.

† *Observations sur le froid rigoureux du Mois de Janvier, 1776*, p. 277.

|| Vol. LXVI. p. 589.

frigoric mixture appears to have been made with the vitriolic acid; and the natural cold of the air at Northampton that day, the 30th of January, was $+9^{\circ}$. It is scarcely possible to determine how far he succeeded. The quicksilver of his thermometer sunk into the bulb, and it, as well as some in a phial, contracted what Dr. FOTHERGILL calls a film on the top; but unless the scale of his instrument went below -40° , or some solid crystals were formed, such as M. BRAUN and others observed at the commencement of congelation, nothing can be collected with certainty from this experiment.

Though the cold of this hard winter was not sufficient, either in England or Holland, for the convenient performance of experiments on the congelation of quicksilver, yet in many parts of the European continent, not farther north, it was fully intense enough for that purpose. The very morning when Dr. BICKER succeeded so imperfectly at Rotterdam, the thermometer sunk to -22° at Rudolstadt, situated a degree more to the southward; and it had been the preceding day as low as -18° at Berlin*.

§ 6. I find no further attempts to freeze quicksilver till the year 1781, when Mr. HUTCHINS resumed this subject with such brilliant success. The preceding experiments had done little more than prove that quicksilver might be rendered solid by cold, and shew what sort of substance it was in that state. Nothing satisfactory had been ascertained with regard to its freezing point, or the degree of a thermometer at which it ceases to be a melted and becomes a solid metal. It must not be supposed, however, that the gentlemen who were engaged

* Beschäftigungen der Berlinischen Gesellschaft Naturforsch. Freunde. B. II. p. 575, 576.

in these researches neglected such a principal object of enquiry; on the contrary, Professor BRAUN himself, as hath been already mentioned; took infinite pains to investigate it, but, for want of perceiving the consequences of the metal's great contraction in becoming solid, went very wide of the truth. This source of error did not escape the penetration of other philosophers, several of whom declared their opinion that the degree of cold necessary for the congelation of quicksilver could hardly be determined by freezing a thermometer filled with that fluid. But Mr. CAVENDISH and Dr. BLACK were the gentlemen who suggested an adequate method of obviating the difficulty, so as to ascertain the point in question with certainty and precision. Reasoning on the well-known fact, that a quantity of water continues at the same temperature from the moment it begins to freeze till the whole is become solid, they very justly concluded that the same would hold good with regard to quicksilver; and Mr. CAVENDISH confirmed this inference by experiments with metals of easy fusion, in which he found a thermometer keep at the same degree all the time they were passing from a fluid to a solid state. Hence it was proposed, that a small thermometer should be placed in some quicksilver to be frozen; which sinking pretty regularly till the congelation began, and remaining stationary till it should be complete, would thus shew the degree of cold at which this effect takes place.

It is not at all surprizing, that both the above-mentioned gentlemen should have suggested the same method, without the least communication of each other's sentiments. Most discoveries follow so closely upon a certain state of advancement in every science, that, under the present general diffusion of knowledge, similar ideas must be expected to occur about the same time to such persons as are engaged in similar pursuits. When the fruit is come to a certain degree of ripeness, more than one

man may have strength enough to shake it off. Were we sure that philosophy would continue to be regularly cultivated, perhaps it might with truth be affirmed, that the utmost efforts of genius amount only to a power of anticipating discoveries which would necessarily be made in the course of a few years by the common progress of mankind. The principles of this experiment for determining the point of congelation in mercury being already before the world, it was most probable, that the consequences to be deduced from them would not escape gentlemen of such acknowledged sagacity, whenever they might happen to apply their attention to that subject.

Though the methods proposed by Mr. CAVENDISH and Dr. BLACK were fundamentally the same, yet there was some difference in the apparatus they recommended; and as the former gentleman got *his* executed in London and sent out to HUDSON'S Bay, it was that which Mr. HUTCHINS employed in performing most of his experiments. A circumstantial detail of these has so recently been read before the Society, that it would be tedious for me here to recapitulate the particulars. They have not only confirmed the preceding observations relative to the solid state into which quicksilver can be brought by cold, its metalline splendor and polish when smooth, its roughness and crystallization where the surface was unconfined, its malleability, softness, and dull sound when struck; but have also clearly demonstrated, that its point of congelation is no lower than -40° , or rather -39° , of FAHRENHEIT'S scale; that it will bear, however, to be cooled a few degrees below that point, to which it jumps up again on beginning to congeal; and that its rapid descent in a thermometer through many hundreds of degrees, when it has once past the above-mentioned limits, proceeds merely from its great contraction in the act of freezing. These

and the other consequences deducible from Mr. HUTCHINS's experiments have been so exactly pointed out by Mr. CAVENDISH, the real author and first mover of the whole business, that nothing remains for me but to add a few supplementary remarks.

Most of the appearances which perplexed M. BRAUN in his experiments admit of such a ready solution from these of Mr. HUTCHINS, that it would be superfluous to dwell upon each particular; there is one, however, which must be mentioned, because an erroneous explanation has been given of it by a very eminent Swedish philosopher*. Professor BRAUN observes, that by a certain kind of management he could effect the congelation of quicksilver with very weak aqua fortis. For this purpose he filled several different glasses with snow, into which he successively poured the dilute acid of nitre, and immersed his thermometer. The mercury, which would sink only to -148° in the first glass, came in the fourth to the term of complete congelation. It is no wonder that the professor, with the ideas he entertained, should think this extraordinary; but we now clearly understand, that the cold in the first glass was sufficient to freeze part of the quicksilver, but did not last long enough to render the *whole* solid; in the second glass a further part froze; in the third still more; till at length only such a quantity was left as the fourth mixture could fully congeal.

Another phenomenon, of which M. BRAUN gives a very unsatisfactory solution, is the sinking of the quicksilver in his thermometers after they were taken out of the frigorific mixture. This, I suppose, proceeded entirely from its beginning to melt in the warmer air, and consequently subsiding to fill up vacuities in the stem or ball, in the same manner as hap-

* Kongl. Vetensk. Acad. Handlingar vol. XXXIII. p. 119.

pened very remarkably in some of the experiments at Hudson's Bay, and in various meteorological observations to be described hereafter.

Mr. HUTCHINS mentions, in the remarks upon his ninth experiment, that the internal surface of the congealed quickfilver, after the superincumbent frigorific mixture and fluid metal had been decanted off, "was every where very rough, and of a "dull white, resembling that of a filver spoon in common "use;" likewise, that "the lump shewed very weak cohesion, "crumbling to pieces under the strokes of a hammer, and had "not the usual polish." These differences he ascribes to an effect produced, by the spirit of nitre in the mixture, upon the quickfilver, with which it came into immediate contact; but I am rather of opinion, that they were occasioned chiefly by the unconfined crystallization of the mercury in the open gallipot employed for this experiment, similar appearances having always been observed when it congealed under such circumstances.

It is here necessary to take notice, that the thermometers sent out to Mr. HUTCHINS, even those made with quickfilver, did not exactly agree together. From his *Table of Comparison* we may collect, what subsequent experiments have confirmed, that -40° upon the two small ones with ivory scales, answered to -44° or -45° upon one of the large thermometers, that which had the ball on top, and to -42° or -43° upon the other. Now as this last was the instrument which Mr. HUTCHINS plunged into the quickfilver he attempted to freeze in his eighth experiment, and which sunk only to -40° , it is no wonder that he did not then perceive any marks of congelation, the cold really wanting two or three degrees of being sufficient for that effect.

The old spirit-thermometer called *H* was erroneous no less than 7° or 8° near the freezing point of water, which served as a kind of compensation in the lower parts of the scale, so as to make it agree tolerably well with those of mercury in the greatest cold of the frigorific mixtures.

Although in two of Mr. HUTCHINS's thermometers the quicksilver sunk exceedingly low, to -450° or near -500° , there is reason to believe he did not in any instance obtain the extreme term of contraction, since Professor BRAUN, in some of his last experiments, brought the mercury in one thermometer to -544° , and in another to -556° *. Hence it would seem, that Mr. HUTCHINS had always some part of the quicksilver left unfrozen, or some vacuity remaining, either in the stem or the ball of his instruments; and as no objection appears against those experiments of M. BRAUN's, we must conclude, that quicksilver, in becoming solid, contracts about a 23d of its whole bulk. When the principal object in view is to determine the quantity of this contraction, it will be most expedient to perform the experiment in the least degree of cold which will permit the quicksilver to be entirely frozen, that it may not be so likely to stick fast in the tube; but care must be taken to congeal the whole of the mercury in the stem as well as that in the bulb.

Among the numerous improvements in natural knowledge which have been made within a short period of years, perhaps none tends to illustrate more phænomena of nature than the late discovery, that a considerable quantity of heat disappears when bodies pass into a state of fluidity or elastic vapour, and re-appears when they are converted back again to their original condition. This remarkable effect of such changes, I believe,

* Nov. Comment. Petrop. tom. XI, p. 313.

was first observed at Glasgow, about twenty years ago, by Dr. BLACK and Mr. IRWIN, who endeavoured to determine its most material circumstances by various experiments. Since that time Dr. BLACK has constantly taught it in his chemical lectures; and considering the heat which disappears as still remaining in the fluid or vapour, but deprived for the time of its property of being communicated to other bodies; and thereby becoming sensible, he calls it *latent* heat; a term sufficiently expressive of his manner of conceiving the fact.

In the year 1772, the celebrated Professor WILCKE inserted, in the Transactions of the Royal Academy of Sciences at Stockholm *, a paper professedly on the subject of *the cold produced by snow in melting*, which being written in the Swedish language is less known in this country than it deserves. He seems not at all acquainted with what Dr. BLACK had done, but speaks of it as his own discovery, originating in an accidental attempt to melt away a quantity of snow by the affusion of hot water; when he found the process go on so slowly, and so little effect produced, that he determined to investigate the cause of so unexpected an event. After a series of experiments with this view, he came to the following conclusion; that snow, in melting, constantly absorbs a certain and equal quantity of heat, which is employed entirely in giving it fluidity. To render such a property more intelligible, M. WILCKE propounds a particular theory of an elastic fluid between the particles of bodies; and he proceeds to various illustrations and deductions, all highly ingenious.

Two principal methods have been adopted to prove this loss of heat; one, by adding ice at the freezing point to a certain proportion of water at a known degree of heat, and observing how much the temperature of the mixture comes out below

* Kongl. Vetenskaps Acad. Handlingar vol. XXXIII. p. 97.

that which should have resulted according to the common laws of the distribution of heat among bodies; the other, by observing how much faster water near the freezing point acquires sensible heat, than an equal quantity of ice melting under similar circumstances. It is obvious, that both these methods tend not only to prove the fact, but likewise to discover the quantity of heat so absorbed; and that the latter also, if the operation be reversed, will shew the quantity of heat evolved, when a fluid congeals or becomes solid. In this way Dr. BLACK estimates the heat in question to be equal to 140 degrees upon FAHRENHEIT'S scale; Professor WILCKE, by a great variety of experiments with different proportions of snow and water, brought it out pretty uniformly about 130; and Mr. CAVENDISH, as he hath lately informed us, finds it amount to 150, and chooses to call the process a generation of heat or production of cold*.

As the method put in practice by Mr. HURCHINS to settle the freezing point of quicksilver depends entirely upon this generation, re-appearance or evolution of heat, by means of which the congealing quicksilver is kept at the same temperature as long as any considerable portion of it remains fluid, I thought some account of such an interesting discovery would not here be misplaced. It now becomes an important object of attention, in examining the properties of bodies, to investigate the quantity of heat produced or lost at their melting and vaporific points. So little, however, has hitherto been done in this respect, even with those bodies that are changing from fluid to solid every day before our eyes, that it is no wonder we are yet unable to determine it in a substance which has so seldom been seen in a solid state. But from the very quick congelation of quicksilver when placed in a frigorific mixture, as shewn by its

* Mess. LAVOISIER and DE PLACE, from some late experiments with their new apparatus, fix it at 135.

rapid descent in the thermometer, and from its readiness to melt again upon an abatement of the cold, apparent in all the experiments, and particularly noticed by M. BRAUN, there is reason to believe, that the quantity of heat employed in giving it fluidity is not very considerable. When water, which has been cooled below $+32^{\circ}$, begins to freeze, a certain part of it, proportioned to the degree of cooling, shoots at once into ice; that is, ice continues to be formed till so much heat be evolved as is requisite to bring the whole up again to $+32^{\circ}$. Now I am inclined to suspect, that in several of Mr. HUTCHINS's experiments the first jump of the quicksilver down from a little below the point of mercurial congelation, depended on a similar principle of the sudden freezing of such a proportion of the mercury as corresponded to the number of degrees it had been cooled below that point; hence, if the degree to which it bore to be cooled before it began to congeal, and the contraction it suffers in congealing, were both known, its quantity of latent heat, to speak in Dr. BLACK's language, might readily be found. From a rude and vague computation of this sort, I am led to believe, it is not half that of water; and if so, quicksilver seems to differ much in this respect from other metals; for tin is said, from Mr. IRWIN's experiments, to require, in order to melt, a quantity of heat which, if set loose and rendered sensible, would raise the thermometer 500 degrees.

Besides the instruments contrived particularly to try the freezing point of quicksilver, two spirit-thermometers also were sent out to Hudson's Bay, principally with a view to some collateral circumstances of the experiment. My intention in recommending them was to discover what degree of cold the freezing mixture produced; and to obtain a more exact comparison of the relative contractions of mercury and alcohol, by

marking their simultaneous descents on a more extended scale, or as long as both of them should continue to contract regularly. The specific gravity of the alcohol employed to make these thermometers was found to be 0,8254, in a temperature of $58^{\circ}\frac{1}{2}$; and they were graduated on the principle of two fixed points, one, the real freezing point of water fixed by actual experiment; the other, the 122d degree above 0, determined by comparison with a standard mercurial thermometer. This interval being divided into 90 parts, the degrees so found were measured downward as well as upward on the scale, with a proper allowance for inequality in the bore of the tube. A subsequent experiment shewed that the freezing point had been rightly marked upon these instruments; but that, in consequence of a common fault in constructing thermometers, of not heating the contents of the tube so much as those of the ball, the point of 122° was marked on both of them lower than it ought by the space of two degrees; so that 122° on the scale indicated only 120 degrees above 0 of real heat. Any detail of the observations that were made to settle the relative contractions of quicksilver and spirits by means of these instruments would be improper at present; it is sufficient to mention, that on one of them the 29th degree below 0, and on the other the 30th, were found to correspond with -40° of the small mercurial thermometers, or more precisely with the point that would have been -39° upon an exact standard instrument.

The other object for which the spirit-thermometers were proposed, is more immediately connected with the congelation of mercury. All former experiments with frigorific mixtures had left us absolutely in the dark with regard to the degree of cold that was really produced. By these instruments it is now determined, that the greatest effect of a mixture of
snow

snow and smoaking acid of nitre, even with the advantage of such natural cold as congealed the quicksilver exposed to it, was only to diminish the heat to such a degree as would correspond with -45° or -46° of a standard mercurial thermometer; and consequently that the cold obtained by FAHRENHEIT in the first experiments with such mixtures, which BOERHAAVE states at -40° *, cannot be exceeded but by a very few degrees. This result is the more surprizing, on account of BRAUN's positive assertions, that his thermometer both of rectified spirits and essential oils descended 150 or at least 100 degrees below 0†. But, since that gentleman was strongly impressed with an opinion of the excessive cold necessary to freeze quicksilver, so much as to shew evident perplexity at finding his spirit-thermometers sink less than those of mercury, I should, from this circumstance alone, be inclined to place most confidence in the experiments made at Hudson's Bay, in which no hypothesis was adopted, and therefore no prejudice can be apprehended. Indeed Mr. HUTCHINS's observations with regard to the degree of cold generated by his freezing mixtures, are so regular, uniform, and numerous, as hardly to leave a doubt that it does not exceed -35° or -36° of his spirit-thermometers. And this is, of itself, a very great additional proof, that the freezing point of quicksilver cannot be much lower than Mr. HUTCHINS determines it, since the mixture was incapable of diminishing the heat more than six or seven degrees further. The advantages arising from a knowledge of the cold produced, were so apparent in these experiments at Hudson's Bay, with respect to many circumstances, both in the congelation of the quicksilver itself,

* BOERHAAVE. Element. Chæmiæ, tom. I. p. 164.

† Nov. Comment. Petrop. tom. XI. p. 290. 316. 317. From 260° to 300° of DE L' ISLE's scale.

and in the action of the frigorific mixtures, that I should suppose spirit-thermometers will always be employed in future, whenever any thing of this kind is attempted.

§ 7. It is not a little extraordinary, that since the death of Professor BRAUN, now near fifteen years ago, all attention to the congelation of mercury should in a manner be laid aside on the spot where it was originally discovered. A dead silence on the subject seems to have prevailed at Petersburg till this present winter; when Dr. MAT. GUTHRIE, F. R. S. Physician to the Cadet Corps of Nobles, having heard the matter much canvassed during his late visit here, resumed the consideration of it on his return to that metropolis. The only intelligence I have yet received of Dr. GUTHRIE's experiments or conclusions, is contained in one of his letters to Dr. GARTHSHORE of this Society, who has obligingly favoured me with the following extract.

“ Having found,” says Dr. GUTHRIE, “ in my late journey
 “ to Britain, that it remained a matter of doubt, whether mer-
 “ cury in its pure state, unmixed with heterogeneous matter,
 “ had ever been or was capable of being congealed; I am glad
 “ to be able, from the result of several experiments, to inform
 “ you, that the purest mercury known to the chemists is capa-
 “ ble of congelation, and in that state will bear the hammer.

“ I have done something also toward determining the point of
 “ its congelation, by determining what it is not, *viz.* 150° of
 “ REAUMUR's thermometer.

“ A fine thermometer, made by NAIRNE, graduated 150°
 “ of REAUMUR, that is, 337° of FAHRENHEIT below the
 “ freezing point” [or -305°] “ sunk entirely into the bulb,
 “ while the mercury in which it was plunged remained per-
 “ fectly liquid, nay had not as yet grown thick and gritty, a
 “ phenomenon

“ phenomenon that always precedes congelation, as I have
“ found in my experiments; nor had there as yet been formed
“ in the inside of the tube containing the mercury to be frozen
“ (and the thermometer to determine the point of congelation
“ with which I stirred it) an incrustation of the metal, another
“ indication of approaching congelation, which ever begins on
“ the side of the tube, and gradually increases till it has
“ reached the center, and a solid cylinder is produced.

“ From this you may form a judgement of the impurity of
“ the mercury which some pretend to have seen congealed with
“ natural cold; for here 150° of REAUMUR was not found a suf-
“ ficient degree of cold to freeze it, and surely no such absence
“ of heat, or any thing near it, has ever been, or ever could
“ be, observed on the face of the habitable globe.

“ I shall only add, that my experiments were conducted on
“ the plan of my learned friend Dr. BLACK, and spiritus nitri
“ fumans Glauberi with snow, were employed to produce an
“ artificial cold, while the thermometer of REAUMUR stood at
“ 20° below 0 in the open air” [that is, -13° of FAHREN-
HEIT’s scale.]

Though the consequences here deduced by Dr. GUTHRIE from his experiment are undoubtedly erroneous, as appears from a sufficient number of other facts, yet it is not at all surprizing, that they should have seemed to him just; for the error arises from a circumstance, which could not be foreseen with certainty, and occurred in several of Mr. HUTCHINS’s experiments as well as in Dr. GUTHRIE’s. To understand this, it must be considered, that when we attempt to ascertain the freezing point of water, by keeping a thermometer immersed in it while it is changing into ice, the instrument employed for this purpose is not made of water, but of a different fluid, not subject to be peculiarly affected.

affected by that particular degree of cold. In order, therefore, to render the experiment with quicksilver perfectly analogous, it would be necessary not to make use of a mercurial thermometer; but to substitute such a one as is capable of sustaining a greater intensity of cold. For otherwise, if it should happen, from any circumstance, that the quicksilver in the thermometer should begin to freeze before that in which it is plunged, the whole experiment must evidently be fruitless, as the former would sink, perhaps, many hundreds of degrees in the instrument, by its own contraction in becoming solid, while the surrounding mercury still retained its fluidity. Now this was precisely the case in Dr. GUTHRIE's experiment; the thermometer, with which he stirred his quicksilver, congealed, it would seem from the great descent, almost entirely, though he could not perceive in the quicksilver so agitated the least appearance of change to a solid state. Thus, likewise, in several of the experiments at Hudson's Bay, the mercury in the enclosed thermometer was found to freeze before that in the cylinder. Hence it is manifest, that the continuance of fluidity in a quantity of quicksilver does not secure a thermometer of that metal immersed in it from freezing.

The cause of this phenomenon is extremely uncertain. Possibly the point of congelation may not be exactly the same in all quicksilver under all circumstances. Foreign admixtures may occasion a difference in this respect; and it does not follow, that the effect of such, in certain proportions, must necessarily be to make the mercury congeal sooner, since, in the case of the fusible metal, the melting point of tin is brought lower by the addition of two metallic substances, both of which separately require a stronger heat than it for their fusion.

But as quicksilver bears to be cooled some degrees below its freezing point, before it begins to form solid crystals, the phe-

nomenon in question may depend upon that circumstance: for if, from whatever cause, the mercury in the thermometer should begin to congeal as soon as it was cooled down to -39° or -40° , whilst that which surrounded it would sustain a cold of -43° or -44° without becoming solid; it is evident, that the whole of the former might be congealed, and yet no part of the latter, though the real freezing point of both were the same, that is, though the surrounding quicksilver as soon as it came to shoot its crystals would rise immediately to -39° , the point at which that in the thermometer froze.

As this is undoubtedly the most obscure part of our knowledge relative to the congelation of quicksilver, I endeavoured to illustrate it by some experiments on the freezing of water. The purest water I could obtain bore to be cooled to $+21^{\circ}$, no less than eleven degrees below the temperature to which it instantly rose as soon as the crystals of ice shot through it. This was distilled water very recently boiled; it is a mistake, therefore, that boiling necessarily renders water not so capable of being cooled below the freezing point. In proportion as the water was less pure, it seemed to congeal the sooner; and the kind of impurity which had the most effect appeared rather to be extraneous matter diffused through the water, so as to trouble its transparency, than such as was chemically dissolved in it*. The smallest particle of ice, also, whenever the water was below the freezing point, either added from without, or by any means formed in it, would instantly cause a crystallization, by which the whole came immediately up to $+32^{\circ}$. Likewise a crack in the bottom of the containing glass vessel

* I take this to be the reason that boiling has been thought to render water incapable of being cooled below the freezing point. In most kinds of water, the application of heat occasions the precipitation of earthy substances which were before held in solution; hence the water comes to be in the state of having extraneous matter diffused through it, and therefore readily congeals.

effectually prevented the water from being cooled below the freezing point, as ice constantly formed on the bottom, perhaps in consequence of the early generation of some minute portions of it in the crack. But independently of these circumstances, neither stirring, agitation, a current of fresh air on the surface, nor the contact of any extraneous body not colder, would cause the water to shoot into ice, even after it was cooled many degrees below the freezing point, notwithstanding the repeated assertions of authors to the contrary.

How far these facts may be applicable to the above mentioned instances, where the thermometer froze before the quicksilver in which it was immersed, can scarcely be determined unless more particulars were known. They shew, however, that the congelation would not necessarily be brought on by stirring the quicksilver, as practised by Dr. GUTHRIE; and point the way to various conjectures upon this difficult phenomenon, the discussion of which must be reserved for a future opportunity.

This source of error in the method for settling the point of mercurial congelation, may easily be obviated by a small change in the apparatus. Nothing further is necessary than to employ thermometers made of alcohol, essential oils, or such other fluids as will bear the requisite cold without freezing. Probably the former of these will be found most convenient: and although the contraction of other fluids does not exactly keep pace with that of quicksilver, yet as the relative proportions can be readily determined, experiments with them may at all times be reduced to the mercurial standard, being not only the most familiar, but likewise that which seems to correspond best with equal increments and decrements of heat.

There is one way also in which mercurial thermometers may be employed to ascertain the freezing point of quicksilver; I mean
by

by plunging them into some of that metal which has been frozen and is now melting. This was put in practice very successfully by Mr. HUTCHINS in his ninth and tenth experiments. It answers to the method of determining the common point of congelation upon a thermometer by melting ice, well known to be more steady and certain in its temperature than freezing water. If, however, the point of mercurial congelation be not exactly the same in different portions of the metal, it is evident, that no reliance could be placed on such an experiment; and it can scarcely be executed but with the greatest advantage of natural cold.

As Dr. GUTHRIE was mistaken in supposing he had proved that quickfilver did not congeal till it was cooled under $-50^{\circ}5'$, his suggestion of impurity in the mercury employed by others is clearly without foundation. The instances to which he refers, when that metal froze by the natural cold of the air, are rendered certain and unexceptionable, from a great variety of concomitant circumstances, confirmed by the most credible testimony, as shall presently be shewn*.

§ 8. This account of mercurial congelation by artificial means would remain incomplete, were I not to mention that at Hampstead, on the 26th of February last, the temperature of the air being then above $+20^{\circ}$, Mr. CAVENDISH, by an ingenious artifice of diluting the nitrous acid to a proper degree,

* Since this was written, Dr. GUTHRIE has sent a more perfect account of his experiments. They agree in the main with Mr. HUTCHINS's, and the difficulties which occurred to him may be solved on the same principles. It seems not improbable, that the thermometer with which the Doctor stirred his quickfilver had, by some accident, in the course of a long experiment, come into contact with the frigorific mixture, and so been set freezing.

funk the quicksilver in his thermometer to 110° , and consequently froze it in part. He then interrupted the experiment to try the cold of his frigorific mixture by a spirit thermometer, and found it nearly as great as Mr. HUTCHINS had ever produced at Hudson's Bay, that is, about equal to -45° of a standard mercurial thermometer.

P A R T II.

NO other experiments have been instituted, as far as hath come to my knowledge, for the purpose of rendering quicksilver solid by frigorific mixtures; therefore I now proceed to a new series of facts, which serve partly to confirm the former, and partly to shew their application. Though the congelation of mercury, abstractedly considered, must be allowed to form a very curious and important epocha in the history of that metal, yet it is as having a reference to thermometers, by teaching us what dependence can be placed upon those instruments, fixing our ideas with regard to the different diminutions of heat, and enabling us to form a juster estimate of climates, that it chiefly becomes interesting to the human race. The subsequent part of this narrative will demonstrate, that quicksilver has very frequently become solid by natural cold; that in a few instances the effect was so palpable and obvious as to strike with immediate conviction; but that in most it has never been even suspected till the present time, the strange appearances which often occurred being imputed by the observers to

any other rather than the real cause, though they are now found to carry with them a force of internal evidence which establishes the truth beyond all doubt.

In enumerating these facts, I shall continue to pursue a chronological order. They are in general of such a kind as could scarcely become an object of attention, till thermometers had acquired some degree of accuracy. This did not happen till near the year 1730, and the first observations which prove the freezing of quicksilver were made within four or five years of that period: so intimately are improvements in philosophy connected with the perfection of instruments!

§ 1. When the Empress ANNA IWANOVNA had ascended the throne of Russia, she resolved to carry into execution one of the favourite ideas of her illustrious uncle, PETER the Great, by sending out proper persons to explore and describe the different parts of her vast dominions, and enquire into the communication between Asia and America. Three professors of the Imperial Academy were chosen for this expedition; Dr. JOHN GEORGE GMELIN, in the department of Natural History and Chemistry; M. GERARD FREDERIC MULLER, as general Historiographer; and M. LOUIS DE L'ISLE DE LA CROYERE, for the department of Astronomy; draughtsmen and other proper assistants were appointed to attend them. In the summer of the year 1733 they departed from Petersburg; and though a principal object of their commission was unavoidably neglected, from the difficulty of transporting the necessary supplies of provisions to Kamchatka, yet it was the tenth year of their travels before the survivors returned to Europe.

The thermometrical observations made in the course of this memorable survey of the Russian empire were communicated

to the world by Professor GMELIN. His first account of them appeared in the preface to his *Flora Sibirica* *, where a few of the most remarkable are adduced as proofs of the excessive rigour of the Siberian climate; but they were afterwards given at full length, with a more satisfactory detail of circumstances, in M. GMELIN's journal of his travels †, published by himself some years after his return. An abstract of them was also inserted in the Petersburg Commentaries for the years 1756 ‡ and 1765 §, taken, after the professor's death, from his original dispatches, in possession of the Imperial Academy. All the accounts agree together tolerably well; but as the journal is more immediately from the author, in his native language the German, and commonly contains most particulars, I thought it right to adhere principally to that work in the following narration of the facts ||.

It was at Yeniseisk, lat. $58^{\circ}\frac{1}{2}$ N. and long. 92° E. of Greenwich, that M. GMELIN first observed such a descent of his thermometer as, we now know, indicated the mercury to have been frozen. This happened in the winter of 1734 and 1735. “Here,” says the professor ¶, “we first experienced the truth of what various travellers have related, with respect to the extreme cold of Siberia; for, about the middle of December, such severe weather set in, as, we are certain, had never been known in our time at Petersburg. The air seemed as if it were frozen, with the appearance of a fog, which did not

* P. lxxi—lxxiii.

† Reise durch Sibirien.

‡ Nov. Comment. Petrop. tom. VI. p. 425.

§ Ibid. tom. XI. p. 320.

|| See also Mem. de l'Acad. des Sciences, 1749, p. 7.

¶ Reise, Theil. I. p. 355.

“ suffer the smoke to ascend as it issued from the chimnies.
“ Birds fell down out of the air as if dead, and froze immediately, unless they were brought into a warm room.
“ Whenever the door was opened, a fog suddenly formed round it.
“ During the day, short as it was, parhelia and haloes round the sun were frequently seen, and in the night mock moons and haloes about the moon. Finally, our thermometer, not subject to the same deception as the senses, left us no doubt of the excessive cold; for the quicksilver in it was reduced” [on the 5th of Jan. O. S.] “ to -120° of FAHRENHEIT’S scale, lower than it had ever hitherto been observed in nature.”

Thus far Professor GMELIN. Little did he conceive that, though his thermometer was not subject to the same deception as the senses, it lay exposed to another source of error which defeated all his conclusions: for as soon as the cold became sufficiently great to produce any congelation of the quicksilver, it ceased to be a measure of the temperature; instead, therefore, of 120° below 0, the cold most probably did not exceed the point of mercurial congelation, or -39° , but by a very few degrees, the great descent of the quicksilver, as it depended upon its contraction in the act of freezing, only affording a proof that it had really suffered this change.

We must here observe, that Dr. GMELIN’S thermometers were constructed by M. JOSEPH NICHOLAS DE L’ISLE, brother to the gentleman who went upon this expedition, on the principle invented by himself, and which still bears his name. At present such thermometers are always made by determining two fixed points, of which the uppermost, or that of boiling water, is assumed as 0, and the lowermost, or the point of melting ice, as 150° , the scale being counted downward; but in their original construction, when the utility of fixed points

was

was less understood, M. DE L'ISLE took the degrees of his scale from decrements in the bulk of the quicksilver equal to ten thousandth parts of its whole volume at the heat of boiling water. By this method the freezing point seems to have fallen about the 152d degree* ; and accordingly Professor GMELIN, whenever he has occasion to express his observations in the numbers of FAHRENHEIT'S scale, reduces them on that supposition. It is not easy to discover the exact time when the present method of reckoning M. DE L'ISLE'S degrees commenced ; but so early as in M. BRAUN'S experiments it is expressly stated, that the freezing point of his thermometer was only 150° †. There can be no doubt, however, both from theory and from WEITBRECHT'S ‡ and GMELIN'S observations, that in the thermometers used during this Siberian journey, the degree at which water congealed was nearly as low as 152° ; it is according to this proportion, therefore, that I shall compute all Dr. GMELIN'S observations, adhering to the common rule for such as have been made subsequent to his time with DE L'ISLE'S thermometer.

The next instance of mercurial congelation to be found in GMELIN'S journal exhibits a very striking example of the force of prejudice. It happened at Yakutsk, lat. 62° N. and long. 130° E. in the winter of 1736 and 1737, and is thus related by the professor §. “ This winter was unusually mild here, “ nevertheless we endured at times very severe cold, being frost-
“ bitten in a sledge within the space of six minutes, notwith-
“ standing all our precautions. One day, also, a certain per-

* GMELIN'S Reise. Theil. III. p. 143.

† Nov. Comment. Petrop. tom. XI. p. 299.

‡ Ibid. tom. X. p. 303.

§ Reise. Theil. II. p. 457.

“son, who has some reputation in the learned world on account of his observations in natural philosophy, informed me by a note, that the quickfilver in his barometer was frozen. I hastened immediately to his house, to see this hitherto incredible wonder of nature. Not feeling by the way the same effects of cold as I had experienced at other times in less distances, I began, before my arrival, to entertain suspicions about the congelation of his quickfilver. In fact, I saw that it did not continue in one column, but was divided in different places as into little cylinders which appeared frozen, and in some of these divisions between the quickfilver I perceived an appearance like frozen moisture. It immediately occurred to me, that the mercury might have been cleaned with vinegar and salt, and not sufficiently dried. The person acknowledged it had been purified in that manner. This same quickfilver, taken out of the barometer and well-dried, would not freeze again, though exposed to a much greater degree of cold, as shewn by the thermometer. We were assured by the inhabitants, that the severest cold of this winter did not approach what they had suffered other years; and yet the thermometer fell several times to 72° below 0 of FAHRENHEIT’s scale, which would be thought, in Germany at least, a very intense frost.”

The gentleman to whose observation DR. GMELIN here shews so little respect, seems to have been no other than one of his associates in the commission, M. DE L’ISLE DE LA CROYERE*, probably the first person upon earth who saw quickfilver reduced to a solid form by cold, and ventured to credit the testimony of his senses. As to the objection, that the same mercury did not freeze with a greater degree of cold, it is of no avail; for M.

* See DR. HENSEL’s Letter, Phil. Transact. vol. LI. p. 673.

GMELIN had not any other means of estimating this but by the descent of his thermometer, which could be depended upon no farther than to the point of mercurial congelation. Nay, it is not improbable, that the more violent the cold, the less would the quicksilver appear to sink below that point, from the quicker freezing and adhesion of the small thread of mercury in the thermometrical tube. Besides, a part of the quicksilver exposed to the air might easily be frozen, and yet no appearance of such a change be perceived, if the mass did not any where separate or divide. And the fact, that it actually did freeze several times during the winter is put beyond all doubt, by the sinking of the thermometer so many degrees below the term at which that effect begins to take place. The absurd idea, that quicksilver appears to congeal in consequence of water it contains, was derived, I believe, originally from a whim of RAYMOND LULLY'S. It has been the usual refuge of those gentlemen who thought proper to deny that mercury could be made solid by cold; but is too destitute of support to merit confutation.

Professor BRAUN mentions, on two different occasions*, that the Petersburg Academy have in their possession some observations made in Siberia, which seem to shew the congelation of mercury by natural cold; but that little credit was given to them, because it had at other times been found to retain its fluidity when the cold was much more intense. Probably the observations here meant are M. DE L'ISLE DE LA CROYERE'S; for that gentleman certainly transmitted papers to the Academy, according to which the mercury became solid as soon as it fell about 200° below 0 of his brother's thermometer †, cor-

* Nov. Comment. Petrop. tom. VIII. p. 363. and tom. XI. p. 269.

† Phil. Transact. vol. LI. p. 673.

responding with -25° of FAHRENHEIT'S scale. This estimation, though now found to be many degrees less than the truth, yet approaches it so near as to impress a very favourable idea both of M. DE L'ISLE'S talent for observation, and of his superiority to vulgar prejudices. It is to this same gentleman that we are indebted for an account of the astonishing severity of the climate in the north-easternmost extremities of Asia. Yakutsk itself, lying further in that direction than any other place where M. GMELIN resided, evidently partakes of the same rigorous cold, if a winter in which quicksilver froze several times was esteemed unusually mild by the inhabitants.

Another set of observations, in the course of which the mercury frequently congealed, were made by Professor GMELIN at Kirenga Fort, lat. $57\frac{1}{2}$ N. long. 108 E. in the winter of 1737 and 1738. His thermometer on different days stood at -108° , -86° , -100° , -113° , and several intermediate degrees. Some extraordinary appearances, which very much perplexed him in these observations, not only admit of a ready solution from Mr. HUTCHINS'S determination of the freezing-point of quicksilver, but also confirm it with wonderful precision.

On the 27th of November (O. S.) after the thermometer had been standing two days at -46° , the professor found it sunk at noon to 108° . He adds*: "I had scarcely noted down this observation, when suspecting some mistake, I ran back and examined it again. I saw the quicksilver now at 102° , and it continued rising so fast, that in the space of half an hour it had reached to -19° ." The explanation of this phenomenon, which appeared so odd to M. GMELIN, is very evident. When the intense cold set in, the quicksilver froze in

* Reise. Theil. II. p. 619.

his thermometer, and stuck in the tube at -46° , that is, a few degrees below the true point of mercurial congelation. But the weather becoming milder two days afterwards, the small thread of quicksilver in the tube soon melted, and consequently subsided. Possibly it came much lower than Dr. GMELIN happened to observe it at noon; for then the great body of quicksilver was undoubtedly in motion, ascending rapidly as it expanded by melting, till it came up to the degree that corresponded with the temperature of the air. Therefore, instead of a change in that temperature from -46° to -108° in a few hours, and from -108° up to -19° in half an hour, which would have been really astonishing, this observation only shews that the cold, after having continued two days as much below -39° as was sufficient to freeze mercury, at length abated 20 or 30 degrees, perhaps very gradually; no greater alteration than frequently takes place in most extra-tropical climates.

A similar instance occurred at Kirenga Fort a few days afterwards*, explicable in the same manner.

Again, on the 29th of December (O. S.) Dr. GMELIN found his thermometer, which had been standing at -40° early in the morning, sunk down to -100° at four in the afternoon. He subjoins the following remark †. “I observed some air
 “ in the thermometer, separating the quicksilver for the space
 “ of about six degrees. Yesterday evening I took notice of a
 “ similar appearance, except that the air was not then collected
 “ into one place, but lay scattered in several. I considered it
 “ as an accidental fault in the instrument, and attempted to
 “ expel it by means of a steel wire, but could not bear the
 “ cold. In the barometer, also, some very small air-bubbles

* Reife. Theil. II. p. 625.

† Ibid. p. 631.

“were perceived. Next morning only a very few minute air-bubbles remained in the quickfilver of the thermometer, which had then risen to -44° , and not the least vestige of them was to be seen in the barometer.”

It cannot be doubted, but these appearances proceeded from a congelation of the mercury in Professor GMELIN's instruments. His thermometer shewed by its descent that the cold was sufficient for this effect; and the disappearance of those supposed air-bubbles as the frost abated, demonstrates that they were nothing more than interstices formed by minute portions of congealed metal resting irregularly upon one another, and which, therefore, were gradually obliterated as the solid bits melting down united into one mass. Now this observation is of consequence, not only as proving that the quickfilver congealed, but likewise as pointing out, with great exactness, the degree of cold necessary for its congelation. For since, when only a few very minute bubbles were left, the mercury reached up to -44° in the thermometer, its freezing point could not be below that degree, because some of it continued still solid, but must be a little higher, just so much as would answer to the expansion produced by the melting of that very small proportion of metal which remained frozen.

From Dr. GMELIN's attempt to extricate the supposed air by means of a wire, it would seem, that the tubes of his thermometers were open at top. This idea is in some measure confirmed by a passage in the preface to his *Flora Sibirica* *, where he mentions, that upon arriving at, or quitting a place, he used to try whether his thermometer would rise to 0 in boiling water, and, if there appeared any deficiency, corrected it by the addition of fresh quickfilver; which he would scarcely have

* P. lxxiv.

done, had the tube been sealed, on account of the great risk of spoiling the instrument in breaking it open so often. Perhaps the above-mentioned appearance of air bubbles, from the divided state of the frozen quicksilver, may have depended in part upon this exposure to the atmosphere, as well as upon the large size of M. DE L'ISLE'S original thermometers. Under these circumstances we cannot suppose that the instruments were very exact.

Dr. GMELIN on several other occasions observed, that the quicksilver in his thermometer looked as if air was interspersed in it. Whenever this happened, it always subsided many degrees below what we now understand to be the point of mercurial congelation *. The professor, totally at a loss to explain such a phenomenon, imputes it sometimes to a fundamental fault in his instrument, but which he could never discover, and at other times to an imaginary effect of the intense cold, in expelling or extricating air from the pores of the quicksilver, to be absorbed as the cold abated. On the 9th of January, 1738, O. S. the mercury sunk at once to -114° , after having been stationary two whole days at -45° †.

The last observations of M. GMELIN'S, in which quicksilver froze, were made upon his return homeward in a part of Siberia, much nearer the confines of Europe. During the month of December, 1742, as he was passing over that branch of the Ural or Riphæan mountains which runs between Verchoturie and Solikamsk, about the 59th degree of N. lat. and scarcely 60 degrees E. of Greenwich, his thermometer sunk to -41° , -70° , and at length into the bulb, though it was graduated

* Reife. Theil. II. p. 634.

† Ibid.

to 96° below 0° *. The same appearance of air bubbles which he had so frequently remarked in such great descents of the thermometer, puts it beyond doubt that the quicksilver was frozen. This event furnished a very striking proof of the force of habit in reconciling men to hardships, which in their common course of life are thought intolerable. Professor GMELIN, who had now been nine years in Siberia, not only bore to travel in this excessive cold, but also, in order to ascertain the height of the mountains he traversed, employed himself in observing a barometer, whilst the quicksilver was freezing in his instruments.

These are the principal of Dr. GMELIN's thermometrical observations. He collected many more, part of which were destroyed by fire or other accidents, and the remainder seem to contain no further information. They were considered by him as demonstrating the cold of Siberia to exceed that even of the most northern parts of Europe near 100 degrees, an opinion which has since been almost universally adopted; whereas we have, in fact, no proof that the difference of climate amounts to so much as the variation between one winter and another. At Yeniseisk, where the cold was so intense in 1735, it does not seem to have ever been sufficient to freeze a thermometer in the winter that M. GMELIN spent there four years afterwards; and it will soon be shewn that quicksilver has congealed more than once in Europe. All that we are authorized to conclude, therefore, with respect to the Siberian climate, is, that the cold there not unfrequently exceeds the degree indicated by -39° of a standard mercurial thermometer.

* Reise. Theil. IV. p. 512—515.

§ 2. This was the period of scientific enterprise. Soon after the intelligent Academicians of Petersburg had penetrated into Siberia by order of the Russian Monarch, another potent Sovereign sent out those philosophical expeditions, by which the opinion of our illustrious countryman, respecting the figure of the earth, was so honourably confirmed. As it became necessary, for the determination of this question, to measure a degree at the arctic circle, the gentlemen who undertook it were unavoidably exposed to a great severity of cold. About the time when the quicksilver was exhibited frozen to Professor GMELIN, near the extremity of Asia, without overcoming his prepossession, M. MAUPERTUIS and his associates saw the liquor congeal in their spirit-thermometer at Torneå in Lapland*. Their mercurial thermometer sunk at the same time to -37° of M. DE REAUMUR'S scale; which, if the instrument was exactly graduated according to that philosopher's original idea, would undoubtedly shew that the quicksilver froze, as it corresponds with 51° of FAHRENHEIT. But the inaccuracies in constructing M. DE REAUMUR'S thermometers have been so great, that I think no dependence can be placed upon this observation, especially as it does not appear to have been attended with any extraordinary phænomenon.

The same objection holds good with regard to the observations made by M. GAUTIER at Quebec, from the year 1743 to 1749, an extract from which is inserted in the Memoirs of the French Academy of Sciences †. The account given of his thermometer is too indefinite to allow any certain inference to

* OUTHIER Voyage au Nord, p. 145.

† Mem. de l'Ac. des Scienc. 1744, p. 135; 1745, p. 194; 1746, p. 88.; 1747, p. 466.; 1750, p. 309.

be drawn; but as the quicksilver several times contracted so much as to leave a visible vacuity in the top of the bulb, and the scale seems to have reached near to its point of congelation, I am rather of opinion that it actually froze. If so, Quebec, situated in lat. 47° , is the most southern place in which such a great degree of natural cold has hitherto been observed.

§ 3. We come now to an instance of what, however often it may have happened, has hitherto never been suspected, the congelation of quicksilver in Europe by natural cold. The observations which prove this fact are recorded in the Transactions of the Royal Academy of Sciences at Stockholm, whence I have extracted the following account, from the original Swedish.

In January, 1760, the weather was remarkably cold in Lapland. On the fifth of that month different thermometers sunk to -76° , -128° , or lower*. Again, on the 23d and following days, they fell to -58° , -79° , -92° , and below -238° into the ball †. This great descent of the mercury was observed in four places, Torneå, Sombio, Iukasierf, and Utsioki, all situated between the 65th and 70th degrees of N. lat. and the 21st and 28th of eastern longitude, by M. ANDREW HEL-LANT, œconomical Inspector of Lapland, whose remarks on the phænomenon afford of themselves sufficient evidence, that the quicksilver was frozen. “During the cold weather at Sombio,” says he ‡, “as it was clear sun-shine, though scarcely “the whole body of the sun appeared above the low woods that “terminated our horizon, I took a thermometer which was

* Kongl. Vetensk. Acad. Handlingar, vol. XX. p. 314.

† Ibid. vol. XXI. p. 312.

‡ P. 314.

“ hanging before in the shade, and exposed it to the rising sun
 “ about eleven in the forenoon, to see whether, when that lu-
 “ minary was so low, it would produce any effect upon the
 “ instrument. But to my great surprize, upon looking at it
 “ about noon, I found that the mercury had entirely subsided
 “ into the ball, though it was standing as high as -61° at ele-
 “ ven o'clock, and the scale reached down to 238° below 0. I
 “ could not perceive or think, that the air had changed so sud-
 “ denly to such an extraordinary degree of cold. I therefore
 “ brought my thermometer, the only one I had left, within
 “ doors, and held it before the fire, when it quickly ascended
 “ to the usual height in a warm room. Upon being carried
 “ out again into the open air, and placed in the shade, it sunk,
 “ as in the forenoon, to -61° . Afterwards I exposed it once
 “ more to the sun-shine; but the sun having already begun to
 “ disappear behind the horizon, the quicksilver did not subside
 “ into the bulb as before. I then returned with the instrument
 “ into the room, and held it in my hands before the fire, upon
 “ which the quicksilver fell back into the ball, where it left a
 “ vacuum or hollow bubble about the size of a pepper-corn.
 “ When I inclined the thermometer, this bubble ran round the
 “ ball; and after a few minutes the quicksilver rose again to
 “ its former height.

“ I repeated these experiments several times at *Nebuloslock*” [a
 settlement about ten miles distant from *Sombio*] “ the same af-
 “ ternoon and the following day, by carrying the thermometer
 “ out of the cold sometimes to the fire, and sometimes into a
 “ warm hut; when the same thing happened, that the quick-
 “ silver always subsided into the ball. After my return to *Tor-*
 “ *nea*^o in April, I attempted to perform them again; but when
 “ the cold was only a few degrees below the freezing point of

“ water, I could never make the mercury sink, either by hold-
“ ing it before the fire, or carrying it into a warm room, though
“ the experiments always succeeded when the thermometer
“ previously stood at -58° or lower.

“ People who were well clothed, and in brisk motion, or
“ driving with rein-deer, could bear this cold an hour or two ;
“ but such as travelled with horses in a sledge soon found it ne-
“ cessary to have recourse to the farm-houses to warm them-
“ selves. On going out from a hot room, some of the first
“ inspirations were rather heavy and difficult ; but the breath-
“ ing soon became easier. It felt dreadfully cold ; but still I
“ could not perceive, from the sensation alone, that the wea-
“ ther was so extraordinarily severe as it appeared to be by the
“ instruments. Having spent an hour at the house of one of
“ my friends, in the afternoon of the 25th of January, and
“ finding on my return that a thermometer, graduated to
“ -58° , had sunk into the ball, I could not at first believe it
“ had happened from the cold, but thought the instrument
“ must be broken, till other thermometers that were hanging
“ near it convinced me of the truth.”

Several reflexions present themselves on the perusal of these observations. The phenomena fairly shew, that there was a sufficient degree of cold to congeal the quicksilver in Mr. HELLANT's thermometers, which sometimes sunk regularly into the bulb, but commonly stuck fast in the tube till it was heated by the sun, the fire, or a warm room, and thus made to subside. The continuance of this cold was very remarkable ; it lasted no less than three days, with sufficient intensity to freeze mercury ; a circumstance almost unparalleled any where, and the more extraordinary, because M. HELLANT, during twenty-three years that he had made observations in Lapland, never

before saw the thermometer so low as to indicate a congelation of the mercury. But it was not in Lapland alone that the season was uncommonly severe. At this same time the frost was nearly, if not quite, intense enough at Petersburg to freeze quicksilver, as appears from the remarks of M. BRAUN, who was then engaged in his experiments. And it is a curious coincidence of events, that on the very day when the congelation of mercury by artificial means was first clearly established in Russia, nature should be performing the same operation before the eyes of an attentive and philosophical observer in a neighbouring kingdom, who yet had not sufficient sagacity to divine her secret.

Two circumstances, however, struck M. HELLANT, which might have led him immediately to suspect the truth. The first was, that such a degree of cold as this prodigious descent of the thermometer seemed to indicate, bore no sort of proportion to the general stile of the weather in that country. What could be more incredible, than that the cold, which had never been known before to sink the thermometer below -40° , should on one particular occasion exceed that point by hundreds of degrees, more than double the whole variation of temperature between summer and winter? Such an event would shew a want of balance in the system of nature, with respect to heat and cold, so very different from the apt adjustment of its other parts, as to be inadmissible but upon the most decisive proofs. Any reflecting person, therefore, would be more inclined to believe, that the instruments employed had ceased to be measures of the temperature, from some cause or other, than that the extremes of cold should be subject to such anomalous excesses.

The other circumstance of which M. HELLANT takes notice, that the sensation attending this cold by no means corresponded with its effect upon a thermometer, pointed directly to the same conclusion. When that instrument is at 40° below 0, the beams of houses crack with a loud explosion, trees split and are killed, birds fall down dead out of the air, and it is with the utmost difficulty that man, notwithstanding all his resources, can preserve the extreme parts of his body from being destroyed by the frost. Now, if the cold had been increased as far beyond this degree, as *it* differs from the heat of boiling water, could M. HELLANT have exposed himself to the open air with impunity? The analogy of all we know of cold declares the contrary: and though the power of animals and vegetables to resist variations of temperature has been found much greater than was formerly imagined, I think it would not be rash to affirm, that in any part of our globe where the cold was carried to such excess, the whole system of organized bodies must perish.

Yet these obvious inferences seem to have never occurred to M. HELLANT. Even the unexpected descent of his thermometer on being exposed to heat, strange and inexplicable as it must have appeared, and contradictory to all the notions he entertained, did not suggest to him a doubt of the instrument's marking the real temperature of the air. But we now know that it ceased to do so after the cold had increased a few degrees below -39° ; that all the unusual phenomena turned upon the congelation of the quicksilver; and that the severity of this season, though greater than usual in Lapland, did not exceed that of common winters by any such remarkable difference.

The vacuum or hollow bubble, observed after the quicksilver had fallen back into the ball, shews how very much it had contracted by the congelation. This bubble moved upon inclining the

thermometer, because the external part of the frozen mass in the ball having melted before the internal, though not in sufficient quantity to fill its whole capacity, ran round it freely in a fluid state, the empty spot always rising to the top.

§ 4. Early in the spring of 1761, the Abbé CHAPPE D'AUTEROCHE, in his journey to Tobolsk for observing the transit of Venus, passed through Solikamsk, a town of Siberia, situated in $59^{\circ}\frac{1}{2}$ N. lat. and 57° E. of Greenwich. On this occasion he takes notice*, that the thermometer had sunk there the preceding winter to -124° ; which, if the general stile of the Abbé's remarks will allow sufficient dependence to be placed upon it, would necessarily shew that the quicksilver was then frozen.

§ 5. M. ERICH LAXMANN, late Professor of Mineralogy and Chemistry at Petersburg, was resident in 1765 at Barnaul in Siberia, lat. 53° N. and long. 81° E. as minister to the German congregation of the Kolyvan Province. On the first day of that year, he saw the thermometer down so low as -58° †; whence it is probable, that some part at least of the quicksilver was congealed. As no concomitant circumstances are recorded with this fact, it would scarcely have been worth mentioning, were it not to introduce an account of some instruments, which became afterwards the subject of very curious observations. For M. LAXMANN, during his abode in this remote country, employed his leisure in the construction of barometers and thermometers, an art in which he acquired great skill. These he afterwards distributed, free of expence, to all

* Voyage en Siberie, p. 24. and 93.

† LAXMANN's Sibirische Briefe, p. 97.

parts of Siberia where they were likely to be used, with the most laudable and exemplary zeal to diffuse some rays of science through those dark and uncultivated regions*.

All the particulars here mentioned are extracted from M. LAXMANN's *Siberian Letters*; a scarce book, because, on his return to Europe, he bought up every copy he could find, as they had been published without his consent by Professor SCHLÖZER of Gottingen.

§ 6. The benefits accruing from the travels of learned men, could not escape the penetration of the wise Empress who now reigns in Petersburg. Soon after her establishment on the throne, she ordered an expedition of the same nature as that in which Professor GMELIN had been engaged above thirty years before. Among the gentlemen who undertook this second philosophical survey of the Russian Empire, was Dr. PETER SIMON PALLAS, one of the most eminent naturalists and skilful observers of the present age. The journal of his travels is published by himself in the German language, and comprehends a rich store of curious and useful information. In general his winters were not spent in the coldest parts of Asia; twice, however, he resided at Krasnoyarsk lat. $56^{\circ}\frac{1}{2}$ N. long. 93° E. and the last time, in 1772, had an opportunity of witnessing the most remarkable instance of the congelation of mercury by natural cold that is yet known to the world.

“The winter,” says M. PALLAS †, “set in early this year, and was felt in December with uncommon severity. On the 6th and 7th of that month happened the greatest cold I have ever experienced in Siberia; the air was calm at the time,

* Sibirische Briefe, p. 29.

† Reise durch verschiedene Provinzen des Russischen Reichs, Theil. III. p. 417.

“ and seemingly thickened, so that, though the sky was in
“ other respects clear, the sun appeared as through a fog. I
“ had only one small thermometer left, on which the scale
“ went no lower than -70° ; and on the 6th in the morning I
“ remarked that the quicksilver in it sunk into the ball, except
“ some small columns which became solid and stuck fast in the
“ tube. By the temperature of a room not much warmed,
“ into which I brought the thermometer from the gallery of
“ my house, these congealed columns immediately fell down;
“ but it was more than half a minute before the mercury came
“ into motion out of the ball. I repeated this experiment fre-
“ quently, and always with similar success, sometimes one and
“ sometimes more threads of frozen quicksilver remaining behind
“ in the tube. When the ball of the thermometer, as it hung
“ in the open air, was warmed by being touched with the fin-
“ gers, the quicksilver rose; and it could plainly be seen, that
“ the solid frozen columns stuck and resisted a good while, and
“ were at length pushed up with a sort of violence. In the
“ mean time I placed upon the gallery on the north side of my
“ house about a quarter of a pound of clean and dry quick-
“ silver in an open bowl; within an hour I found the edges
“ and surface of it frozen solid, and some minutes afterwards
“ the whole was condensed, by the natural cold, into a soft
“ mass very much like tin. While the inner part was still
“ fluid, the frozen surface exhibited a great variety of *branched*
“ wrinkles; but in general it remained pretty smooth in freez-
“ ing, as did also a larger quantity of quicksilver which I after-
“ wards exposed to the cold. The congealed mercury was more
“ flexible than lead; but upon being bent short it was found
“ more brittle than tin, and when hammered out thin it seemed
“ somewhat granulated. If the hammer had not been per-

“fectly cooled, the quicksilver melted away under it in drops ;
“and the same thing happened when the metal was touched
“with the finger, by which also the finger was immediately
“benumbed. In our warm room it thawed on its surface gra-
“dually, by drops, like wax on the fire, and did not melt all
“at once. When the frozen mass was broken to pieces in the
“cold, the fragments adhered to one another, and to the bowl
“in which they lay. Although the frost seemed to abate a
“little toward night, yet the congealed quicksilver remained
“unaltered, and the experiment with the thermometer could
“still be repeated. On the 7th of December I had an oppor-
“tunity of making the same observations all day ; but some
“hours after sun-set a north-west wind sprung up, which raised
“the thermometer to -46° , when the mass of quicksilver
“began to melt.”

Before this observation of Dr. PALLAS's, no person had seen or handled quicksilver frozen by natural cold, so as to submit the fact to the public with competent evidence ; but the circumstances here related are so pointed and consistent, that even those who had doubted of M. BRAUN's experiments were now staggered, and began to believe. Indeed, it was scarcely possible to suppose any mistake, when Dr. PALLAS had two whole days to repeat and vary the experiments at his leisure. But besides removing all doubts upon the congelation of quicksilver, these observations tended to shew, within certain limits, the degree of cold necessary for that effect. It was evident that the freezing point must be somewhere above 70° , because the thermometer's graduation reached only so low, and yet some part of the mercury always congealed in the tube ; and as the solid masses did not begin to melt till the thermometer rose to -46° , *that* seemed to be nearly the point at which it passes from

from a solid to a fluid state, and very possibly was so upon this instrument, a difference of several degrees being often found in thermometers so low down on the scale as -40° , from inaccuracies in their construction.

The crystallization of quicksilver, also, became manifest on this occasion. Hence, when hammered out thin, it shewed a granulated texture. The branched wrinkles too, which formed on its surface whilst it was congealing, could scarcely have proceeded from any other cause, and suggest a general idea of the manner in which it shoots. That quicksilver should crystallize so much more visibly than most other metals, will not appear surprising, if we consider how little the cold is below its freezing point. Such substances as require, in order to melt, a degree of heat much above that of our atmosphere, experience so great a change of temperature upon being taken off the fire, that they become solid hastily, and as it were in confusion; whereas quicksilver, having never probably been exposed to a degree of cold much exceeding that of its melting point, its particles have had full leisure to arrange themselves regularly, in exact conformity to the laws of their mutual attractions. As in Professor BLUMENBACH'S and Mr. HUTCHINS'S experiments, so here, I imagine, some slight roughness of the surface was occasioned by this crystallization; in consequence of which M. PALLAS compared his frozen quicksilver to tin, rather than to bright silver, the appearance it always assumes when congealed in smooth glass.

Another property of quicksilver, very important to be known, was observed perhaps no where so distinctly as on this occasion at Krasnoyarsk; I mean its tendency to adhesion in freezing. Thus, Dr. PALLAS says the fragments of the congealed mass stuck to one another, and to the bowl in which they

they lay. So likewise Mr. HUTCHEMINS found the frozen quicksilver adhering to his cylinders and gallipot; Professor BLUMENBACH to his glass vessel; and similar facts occurred to other observers. Hence the deceptions, already so often mentioned, from the sticking of the mercury in the stems of thermometers. And this cause of error can scarcely ever fail to take place; for if quicksilver congealing in wide open vessels adheres to them wherever it touches, how can it be expected to remain loose when frozen in a narrow tube? Now, since quicksilver, under these circumstances, retains the same appearance as while fluid, from the polish given to its surface by the smooth glass, it is no wonder that such frequent mistakes have been made relative to the height of the thermometer, both in experiments with artificial cold, and in meteorological observations. At the same time it must be confessed, that such a tendency to adhere, in a metal which contracts so much in becoming solid, is not a little difficult to explain, unless we may suppose it to be the immediate effect of the crystallization.

Quicksilver, with all its other qualities of a perfect metal, seems from Dr. PALLAS's, and indeed most of the experiments, not to be completely malleable, but rather apt to break under the hammer. Perhaps it has never been sufficiently cooled to possess its metallic properties in perfection; for with respect to its melting point it may be considered as having always been hot, that is, heated near to fusion, a state in which other metals undergo a very sensible change in their properties. But when mercury congeals in vessels which confine its surface, it seems to become more malleable than under a loose crystallization.

Dr. PALLAS's travels lasted from the year 1768 to 1773, during which time *this* of the beginning of December, 1772,

seems to be the only instance that occurred of the freezing of quicksilver. From this circumstance, and his occasional hints about the climate, I am inclined to suspect, that the cold was not so great in Siberia at this period, as when Dr. GMELIN was there from 1734 to 1742. In Europe, likewise, the winters in general appear to have been severer about the time of GMELIN'S expedition than lately. It was in this very town of Krasnoyarsk that Professor GMELIN resided during the famous hard winter of 1739 and 1740; but unfortunately he has informed us of the cold there only by its grosser effects, I believe because his thermometers had been accidentally destroyed.

§ 7. Nearly 500 miles south-eastward of Krasnoyarsk is the town of Irkutsk, the capital of a Siberian Province on the vast Baikal Lake, and situated in lat. 52° N. and about the 104th degree of E. longitude. At the former of these places, the cold recorded by Dr. PALLAS began to abate on the 7th of December in the evening; and more than a day after, that is, on the 9th in the morning, it became so intense at Irkutsk as to freeze quicksilver. An account of this phenomenon, sent by Lieutenant-general VON BRILL, the governor, was published by Dr. PALLAS in his *Journal**, and M. GEORGI, one of the associates in this expedition, afterwards collected some further particulars †. It appears, that about four in the morning, the quicksilver was found frozen in the barometer and thermometer, its upper surface being irregularly broken. In the former of these instruments the mercury stood at 28 inches 7 lines, and the broken appearance extended through a space of about

* *Ibid.* p. 419.

† *Bemerkungen einer Reise im Russischen Reich*, B. I. p. 29.

5 lines from the top downward: when it came to melt a few hours afterwards, it rose to 29 inches 7 lines, which difference of height was probably in part at least an effect of the contraction it undergoes in freezing, its greater specific gravity in the congealed state making it stand proportionably lower. In the thermometer, part of the mercury had stuck at -44° ; and immediately under -59° an empty space was left, equal to 11 degrees of the scale. This observation, therefore, determines almost precisely the freezing point; for none of the quicksilver could have adhered in the tube so high as -44° , unless it had congealed before it sunk below this point, and consequently before the cold exceeded this degree. And that the mercury was really frozen became evident afterwards; for about eleven in the forenoon, as the air grew warmer, it was found to have all subsided into the bulb, the small threads in the tube melting down into the vacuity left there, out of which it did not rise again till near two hours had elapsed.

The accuracy of this thermometer at Irkutsk, apparent from its precision with respect to the point of congelation, would be matter of surprize, had we not been informed, that both it and the barometer were among the instruments constructed by M. LAXMANN during his residence in Siberia. M. GEORGI mentions them as being in the possession of Dr. WACHSMANN, the public physician of Irkutsk, by whom probably the observations were communicated to the governor.

§ 8. As the cold of America is well known to exceed that which prevails under the same latitudes in Europe, we must expect to find quicksilver freezing spontaneously in parts of that continent which do not lie very far to the northward. Accordingly, besides the instance of Quebec formerly mentioned,

this effect takes place frequently in Hudson's Bay, even at Albany Fort, where the latitude is not one degree greater than in London. Mr. HUTCHINS, in his different situations at Hudson's Bay, has been constantly attentive to meteorological observations. During his former residence at York Fort, situated near the middle of the Bay in lat. 58 N. he was not provided with any thermometer graduated more than 70 or 90 degrees below the cypher: but he remarked, that "the quicksilver frequently sunk into the bulb," especially after having been stationary at -55° or -57° , and that it afterwards used to ascend to "about -30° , indicating a greater degree of "heat than before it fell*." These phænomena were clearly owing to its congelation, adhesion in the tube, and subsequent liquefaction as the air grew warmer. When Mr. HUTCHINS went afterwards to Albany Fort, and had procured instruments with more extensive scales, he observed the same appearances still more distinctly. His thermometers froze twice in the winter of 1774 and 1775, and three times in that of 1777 and 1778; and in every instance, except one, the mercury sunk hundreds of degrees just as the cold began to abate†. The last of these observations is rendered remarkable by the descent of the quicksilver to -490° , the greatest ever known by natural cold, and probably very near its extreme term of contraction by freezing. In 1782, also, Mr. HUTCHINS's thermometers, together with some quicksilver in a phial, again congealed in the open air, and exhibited similar phænomena, as appears from the account of his experiments.

Fortunately in these instances of intense cold at Albany Fort, attention was paid not only to the mercurial thermometers, but

* MS. Journal.

† Ibid.

likewise to one made of spirits, whose relative movement has been ascertained by comparison. This instrument, while the others were three, four, or almost five hundred degrees below 0, never sunk further than to a point which corresponds with -46° of a standard mercurial thermometer. Hence it would have been easy to infer, both that the quicksilver actually congealed on these occasions, and that the degree of cold necessary for such an effect does not exceed -46° . That the most intense cold of Hudson's Bay, during a series of several years, went so little below the point of mercurial congelation, well deserves to be noticed; and as it seems to be seldom greater in Siberia, at least in the parts visited by GMELIN or PALLAS, the effects being not more violent, perhaps we are authorized to conclude, that the extreme of artificial cold, produced by snow and nitrous acid, corresponds pretty exactly with the extreme of natural cold in the most rigorous climates which can well be inhabited.

Mr. HUTCHINS's meteorological journal at Hudson's Bay confirms what he mentions in the remarks on his third experiment, that in the coldest weather the wind is to the southward of W. which must evidently depend on some local circumstance of that country. During my own residence in America, I thought I could distinctly perceive that the coldest winds, vulgarly called *north-westers*, did not blow exactly from that quarter; but at Rhode Island, for instance, came from N.W. by W. or a point still more to the westward; at New York were rather nearer the W. than the N.; and at Philadelphia almost due N.W. On tracing lines in these directions from Albany Fort, Rhode Island, and Philadelphia, they are found to meet among the great ridges of mountains which separate Hudson's Bay from Lake Superior; whence a suspicion arises, that these mountains

mountains may be one source of the excessive cold which particular winds occasion in North America. But another circumstance is to be taken into consideration, that when the fury of these N.W. winds abates, they very regularly draw round to the westward and southward. Now the *maximum* of cold must be, when the cold wind has blown as long as it can, and the succeeding warmer wind has not yet had time to undo any of its effect; consequently at the period when it is veering round from the northward toward the sun. Accordingly we find, that when the cold has become sufficiently great at Albany Fort to congeal quicksilver, the wind not only came from the southward of W. but commonly, also, blew with very little force; indeed the intensest frosts in all countries seem to take place with light airs or a calm.

§ 9. There are, in the possession of the Royal Society, several other meteorological registers from our different settlements on Hudson's Bay; but none of those which I have seen contain any striking extraordinary appearances, to shew that the thermometer was frozen; and the descent alone, within certain bounds, determines nothing, for these instruments were formerly constructed with so little accuracy, as to be often marked 8 or 10 degrees too low at the point of mercurial congelation. I conclude, therefore, that when Mess. WALES and DYMOND were at Prince of Wales's Fort in the winter of 1768 and 1769, the quicksilver always retained its fluidity, though it once sunk to -45° of their thermometer*. This Fort lies in lat. 59° ; but on account of the abovementioned ridges of mountains, and perhaps for other causes, the southern parts of the Bay may be quite as cold as the northern.

* Phil. Transact, vol. LX. p. 153.

§ 10. We must now return to Europe. In the beginning of the year 1780, M. VON ELTERLEIN, of Vytegra, froze quicksilver by natural cold, and sent an account of his experiment in a letter to the late Professor GÜLDENSTÄDT, then at Petersburg. I obtained from Gottingen a copy of this letter, in the original German, by the friendship of Sir JOSEPH BANKS; and, translated, it is as follows:

“ On the 4th of January, 1780, the cold having increased
“ to -34° that evening at Vytegra, I exposed to the open air
“ three ounces of very pure quicksilver, in a China tea-cup,
“ covered with paper pierced full of holes. Next day, at eight
“ in the morning, I found it solid, and looking like a piece of
“ cast lead, with a considerable depression in the middle. On
“ attempting to loosen it in the cup, my knife raised shavings
“ from it as if it had been lead, which remained sticking
“ up; and at length the whole separated from the bottom
“ of the cup in one mass. I then took it in my hand to
“ try if it would bend; it was like stiff glue, and broke
“ into two pieces; but my fingers immediately lost all feeling,
“ and could scarcely be restored in an hour and an half by rub-
“ bing with snow. At eight o'clock a thermometer, made by
“ M. LAXMANN of the Academy, stood at -57° ; by half after
“ nine it was risen to -40° ; and then the two pieces of mer-
“ cury, which lay in the cup, had lost so much of their hard-
“ ness that they could no longer be broken or cut into shavings,
“ but resembled a thick amalgam, which, though it became
“ fluid when pressed by the fingers, immediately afterwards
“ resumed the consistence of pap. With the thermometer at
“ -39° , the quicksilver became fluid. The cold was never less
“ on the 5th than -28° , and by nine in the evening it had
“ increased

“increased again to -33° . In the morning the wind was “N.N.E. and afterwards N.W.”

This experiment of M. VON ELTERLEIN's deserves attention in many respects. It ascertains the freezing point of mercury with such wonderful exactness, from the melting of the solid pieces when the thermometer came up to -39° , as to furnish a valuable corroboration of Mr. HUTCHINS's experiments, and at the same time very much to enhance our opinion of M. LAXMANN's skill in the construction of instruments. When the thermometer was thought, early in the morning, to be standing at -57° , I imagine, that part of the quicksilver being frozen adhered in the tube. The ductility of the solid metal must have been considerable, from its yielding to the knife in the form of shavings; yet, as in most other instances, it shewed some degree of brittleness when force was applied to it in the mass. Crystallizing without impediment, it assumed an appearance which M. VON ELTERLEIN rather compares to that of lead than of silver. Its tendency to adhesion became evident from the necessity of employing an instrument to separate it from the tea-cup; and its contraction in freezing was demonstrated by the depression observed in the middle of the solid mass. This single experiment, therefore, exemplifies, in a very beautiful manner, most of the properties hitherto discovered in quicksilver, when it passes from a fluid to a solid form.

Vytegra, or Witegorfk, is situated in lat. 61° N. and long. 36° E. upon a river of the same name. It has acquired some celebrity from one of the many useful projects which occupied the active mind of Czar PETER the Great. He proposed to cut a canal from the river Vytegra which discharges itself into the Lake Onega, to the river Kovsha which joins the Belosero, or White Lake, in order to form a communication between those

two great bodies of water; but the undertaking was unfortunately interrupted by his death*.

§ 11. The last instance I have been able to find of the congelation of quicksilver by natural cold, occurred no longer ago than the beginning of the year 1782, in Iemtland, one of the northern provinces of Sweden. M. JOHN TÖRNSTEN, Engineer-extraordinary, is the gentleman to whom we are indebted for this observation. His letter on the subject, dated from Brunflo in Iemtland, lat. $63^{\circ}\frac{1}{2}$ N. and long. 15° E. is inserted in the Swedish Transactions for 1782 †, together with some remarks upon it by Professor WILCKE.

“ During twelve years,” says M. TÖRNSTEN, “ that I have
 “ resided here in Iemtland, the cold had never but once brought
 “ the thermometer so low as -36° , till the last day of Decem-
 “ ber, 1781, when it fell in the evening to -54° . The fol-
 “ lowing new-year’s day it was sunk to -56° at eight in the
 “ morning, and by ten to -62° . Here it continued stationary
 “ several hours, but at half past four in the afternoon it was
 “ observed at -116° , and by eight the same evening it had
 “ risen to -31° . Although the quicksilver,” continues M.
 TÖRNSTEN, “ thus fell to -116° on the first of January in
 “ the afternoon, I am of opinion that its descent ought not to
 “ be ascribed to a proportionable increase of cold, but on the
 “ contrary proceeded from the sudden change to milder wea-
 “ ther, which came on that afternoon. For the preceding
 “ evening, when the thermometer was standing at -54° , I
 “ remarked, that, upon bringing it into a warm room, the
 “ quicksilver fell on a sudden entirely into the ball, which was

* Büfching’s *Erdbeschreibung*, Theil I. p. 669.

† *Kongl. Vetensk. Acad. Nya Handlingar*, tom. III. p. 80.

“ about 130 degrees below 0. This experiment I repeated fe-
 “ veral times with success, but observed the following differ-
 “ ence, that if I had not kept the thermometer in the heat long
 “ enough for the quicksilver to begin to rise again after it had
 “ sunk into the ball, it never ascended above the 130th degree
 “ by continuing in the cold, but upon being carried back into
 “ the warm room it contracted still more in the ball by a quan-
 “ tity which, however visible, could not be measured. On
 “ the other hand, if the instrument had been kept in the room
 “ till the mercury had risen above -54° , it became stationary
 “ at that degree in the open air. Now, though I did not, on
 “ the 1st of January, bring the thermometer within doors be-
 “ fore it had sunk of itself to -116° , yet *this* fall likewise
 “ seems to have been occasioned by the change to milder wea-
 “ ther which was then taking place. For at eight in the even-
 “ ing, when the external cold was at -31° , I found that hoar-
 “ frost formed on the ball and stem of the thermometer as be-
 “ fore, upon its being brought into a warm room; but the
 “ mercury did not sink, on the contrary it began immediately
 “ to rise.

“ Some quadrupeds perished by the intense cold, and a great
 “ number of small birds were found dead. Nevertheless, the
 “ people did not neglect going to church on this high holiday,
 “ and I have not heard that any one was frost-bitten who went
 “ out with proper cloathing.”

M. TÖRNSTEN certainly judged right when he concluded,
 that the fall of the thermometer to -116° rather indicated a
 diminution than an increase of the cold. Though he knew
 nothing of the cause, yet his observation led him to a just in-
 ference, in which he displayed more sagacity than M. HELLANT
 on a similar occasion. All the phænomena which so much perplexed
 these

these gentlemen are explicable in the following manner. When the air becomes sufficiently cold to freeze quicksilver, that metal must be standing about -39° , or, in the common way of marking the boiling point, somewhere between -40° and -50° , in the tube of a thermometer exposed to it. As the small thread of mercury in the tube must be more easily affected by the cold, it will probably congeal before any other part, and stick fast about the abovementioned degrees. The remainder of the mercury will then go on to freeze, and as it suffers such a great contraction in becoming solid, must leave a considerable vacuity in the bulb of any common thermometer. Consequently, when the cold, from whatever cause, comes to be less than is required for keeping the metal in a solid state, the small thread that was frozen in the tube immediately melts, and sinks down into the vacuity of the bulb, where the whole mass remains, till by its gradual liquefaction it expands again into the tube, and becomes a just measure of the temperature. This agrees exactly with what M. TÖRNSTEN observed. In the evening of the 31st the quicksilver congealed in his thermometer, and part of it stuck in the tube at -54° , but subsided into the vacuity left in the bulb, as soon as it was exposed to heat. When the instrument had been kept in the warm room till the quicksilver re-ascended into the tube, it froze and adhered again in the open air, and the same phænomena were repeated. If M. TÖRNSTEN be exact in saying it always became fast at -54° , the circumstance is curious, and may have depended upon some particular state of the tube in that part, or upon the first shooting of the mercury after it had been cooled to a certain degree below its freezing-point. But when the thermometer was carried back into the open air before any of the quicksilver had risen out of the bulb, the effect of the cold could not be to

force it up into the tube, and therefore no such appearances were observed as in the former case. With regard to M. TÖRNSTEN'S remark, that when the whole mass of quicksilver remained in the ball it still contracted upon the application of heat, the fact is so improbable, and would be perceived with such difficulty, that I have no doubt but he was misled by some prepossession. In like manner on the 1st of January, when the thermometer, having been stationary some hours at -62° , sunk in the afternoon to -116° , it happened unquestionably from the melting and subsiding of a thread of frozen mercury, which had adhered in the tube of the instrument as high as the former degree. None of these effects could be produced when the thermometer had risen to -31° , because the cold was not then sufficient to congeal the quicksilver. In this easy and simple manner, does our knowledge of the freezing point of mercury enable us to account for phænomena, which were thought so anomalous as to elude every kind of explanation. Even so lately as last year, one of the most eminent philosophers in Europe, Professor WILCKE of Stockholm, made a vain attempt to solve the difficulties by a strained application of his doctrine relative to the various specific quantities of heat in bodies, and their different attractions for the matter of heat*.

It would now be superfluous to add, that the real cold at Brunflo was by no means what the thermometer seemed to indicate, but probably very little exceeded -39° , or the degree of mercurial congelation, had not M. TÖRNSTEN'S observations been lately represented, even in this country, as exhibiting an instance of cold actually carried to such a disproportionate and enormous excess.

* *Ibid.*

Thus is the history of the congelation of quicksilver, both by natural and artificial cold, brought down to the present period. All the facts I have collected are here delivered: it is not improbable, however, that there may be others which have escaped me, especially such as are very recent, or have never been published*; but the number already found is greater than

* Accordingly, having gone to Paris after this paper was read, and there mentioned, at a meeting of the Academy of Sciences, our late experiments on the congelation of quicksilver, I was informed, that M. CAZALET had succeeded in rendering it solid at Bourdeaux; and soon afterwards the following account of his experiment came out in the Paris Journal, which, for obvious reasons, I shall give in the original French.

“ M. CAVENDISH, de la Société Royale de Londres, a fait dans le mois de
“ Février dernier, l'expérience de la congélation du mercure, à Hampstead, situé à
“ deux milles de Londres. Ce Savant est sur le point de publier son mémoire à
“ ce sujet. Le travail du chymiste Anglois ayant été annoncé à l'Académie
“ Royale des Sciences, dans une de ses séances, son Directeur, M. CADET DE
“ GASSICOURT, a *revendiqué* en faveur de M. CAZALET, la congélation du mer-
“ cure dans un climat beaucoup plus temperé que Londres, le chymiste François
“ l'ayant obtenue à Bordeaux.

“ M. CAZALET, regardant l'acide nitreux concentré comme, de tous les fels,
“ celui qui produit la dissolution de la glace avec plus de facilité, annonça en 1779,
“ dans une de ses leçons publiques à Bordeaux, qu'il croyoit à la possibilité de la
“ congélation du mercure, par ce moyen, dans les Provinces méridionales mêmes;
“ mais il n'eut occasion de faire son expérience qu'au mois de Septembre de
“ l'année dernière.

“ Il prit de la glace pilée, passée à travers un crible, la mit dans un baril dont
“ le fond étoit un plat de porcelaine percé de plusieurs trous pour faciliter l'écoule-
“ ment de la glace fondue; il plaça plusieurs tubes remplis de mercure au centre
“ de la glace, qu'on arrosa d'esprit de nitre fumant fait par le procédé de WOLFE.
“ On rapprochoit la glace des tubes à mesure que la dissolution s'en opéroit; il
“ fallut 120 livres de glace pour produire la congélation du mercure. Les tubes
“ retirés et cassés les uns après les autres, le mercure se trouva en filets comme

“ de:

than I expected on beginning the search. By such a connected view of the different observations and experiments in any one branch of science, we are furnished with the best opportunity of discriminating what is certain from what is doubtful, and acquire as distinct ideas as the actual state of knowledge will admit. On the present subject of mercurial congelation, the conclusions have in general been noticed, as the premises occurred. Though Mr. HUTCHINS's experiments did not stand in need of any confirmation, yet still it is pleasant to see their principal result, the freezing point of quicksilver, established by such a body of collateral evidence as, taken together, is absolutely irresistible. But besides the information obtained relative to quicksilver itself, we have been able to correct several vulgar prejudices. The difference between cold climates no longer appears so prodigious, nor the resisting powers of animals and vegetables so astonishing and inconceivable. That extensive scale of heat, which represents its diminutions by artificial means as continued down so many hundreds of degrees below the greatest produced by nature, however specious in prospect, proves to be destitute of foundation. The use of quicksilver for thermometers is at length fully ascertained. From the boiling point, to 39° or 40° below 0, it must be considered as unexceptionable, all suspicion of its irregular contraction within those

“ de l'argent. Bientôt la chaleur de l'atmosphère lui rendit sa fluidité. Cette
 “ expérience intéressante a le double mérite d'avoir été devinée par un chimiste Fran-
 “ çois, et exécutée par un autre; il a suppléé à ce que le climat opposoit d'obstacles,
 “ un moyen tout-à-fait ingénieux, et qui exigeoit des connoissances en physique.” See
 Journal de Paris, 15 Juill. 1783, p. 814.

The peculiarity of M. CAZALET's process consists in the largeness of the quantities on which he operated, and the provision he made for the useless liquor, produced by the melted ice, to run off as fast as it was formed. He certainly congealed the mercury, but did nothing to ascertain its freezing point, which he seems not even to have had in contemplation.

bounds being removed, by such a complete explanation of the cause upon which its anomalous descent in the lower part of the scale depends. On this principle there might, perhaps, be some propriety, in constructing thermometers of mercury, to fix the cypher at its point of congelation, and thence reckon the degrees of heat upwards.

The principal advantage, however, of thus passing in review all former accounts, is to furnish an important lesson to authors, which can never be too strongly inculcated, that their accuracy must be brought to the test of future discoveries. As knowledge advances, their errors, their misrepresentations, their suppression of the truth, or fictitious additions to it, shall all be infallibly detected, and heap upon their head proportionable ignominy; while the simple and candid narrative, the exact and unbiassed relation of facts, will acquire redoubled lustre from the fiery trial. Let every author recollect, that the day is impending, when some unforeseen improvement, affording means to sift falsehood from truth, however artfully blended, shall finally decide whether he is to be reprobated with the base herd of deceivers, or ranked among those faithful votaries of science, whose names will be delivered down with honour to posterity.

