XX. *Obſervations on Mr.* Hutchins's *Experiments for determining the Degree of Cold at which Quickſilver freezes.* *By* Henry Cavendiſh, *Eſq. F. R. S.*

Read May 1, 1783.

THE deſign of the following paper is to explain ſome particulars in the apparatus ſent by me to Mr. HUTCHINS, the intention of which does not readily appear; and alſo to endeavour to ſhew the cauſe of ſome phenomena which occurred in his experiments; and point out the conſequences to be drawn from them.

This apparatus was intended to determine the preciſe degree of cold at which quickſilver freezes: it conſiſted of a ſmall mercurial thermometer, the bulb of which reached about 2½ inches below the ſcale, and was incloſed in a glaſs cylinder ſwelled at bottom into a ball, which, when uſed, was filled with quickſilver, ſo that the bulb of the thermometer was intirely ſurrounded with it. If this cylinder is immerſed in a freezing mixture till great part of the quickſilver in it is frozen, it is evident, that the degree ſhewn at that time by the incloſed thermometer is the preciſe point at which mercury freezes; for as in this caſe the ball of the thermometer muſt be ſurrounded for ſome time with quickſilver, part of which is actually frozen, it ſeems impoſſible, that the thermometer ſhould be ſenſibly above that point; and while any of the quickſilver in the cylinder remains fluid, it is impoſſible that it ſhould ſink ſenſibly below

below it. The ball of the thermometer was kept conſtantly in the middle of the ſwelled part of the cylinder, without danger of ever touching the ſides, by means of ſome worſted wound round the tube. This worſted alſo ſerved to prevent the acceſs of the air to the quickſilver in the cylinder, which, if not prevented, would have made it more difficult to have communicated a ſufficient degree of cold. The diameter of the bulb of the thermometer was rather leſs than one-fourth of an inch, that of the ſwelled part of the cylinder was two-thirds, ſo that there was no where a much leſs thickneſs of quickſilver between the ball and cylinder than one-ſixth of an inch. The bulb of the thermometer was purpoſely made as ſmall as it conveniently could, in order to leave a ſufficient ſpace between it and the cylinder, without making the ſwelled part thereof larger than neceſſary, which would have cauſed more difficulty in freezing the quickſilver in it. Two of theſe inſtruments were ſent for fear of accidents.

One of the moſt ſtriking circumſtances in the experiments which have been made for freezing mercury, is the exceſſively low degree to which the thermometers ſunk, and which, if it had proceeded, as was commonly ſuppoſed from the freezing mixture having actually produced ſuch a degree of cold, would have been really aſtoniſhing. The experiments, however, made at Peterſburg afforded the utmoſt reaſon to ſuppoſe, and Mr. HUTCHINS's laſt experiments have put beyond a poſſibility of doubt, that quickſilver contracts in the act of freezing, or in other words, that it takes up leſs room in a ſolid than in a fluid ſtate; and that the very low degree to which the thermometers ſunk was owing to this contraction, and not to the intenſity of the cold produced: for example, in one of Mr. HUTCHINS's experiments a mercurial thermometer, placed in the

the freezing mixture, sunk to 450° below nothing, though the cold of the mixture was never more than – 46; so that the quicksilver was contracted not less than 404° by the action of freezing.

If a glass of water, with a thermometer in it, is exposed to the cold, the thermometer will remain perfectly stationary from the time the water begins to freeze till it is intirely congealed, and will then begin to sink again. In like manner, if a thermometer is dipped into melted tin or lead, it will remain perfectly stationary, as I know by experience, from the time the metal begins to harden round the edges of the pot till it is all become solid, when it will again begin to descend; and there was no reason to doubt that the same thing would obtain in quicksilver.

From what has been just said it was concluded, that if this apparatus was put into a freezing mixture of a sufficient coldness, the thermometer would immediately sink till the quicksilver in the cylinder began to freeze, and would then continue stationary, supposing the mixture still to keep cold enough, till it was intirely congealed. This stationary height of the thermometer is the point at which mercury freezes, though in order to make the experiment convincing, it was necessary to continue the process till so much of the quicksilver in the cylinder was frozen as to put the fact out of doubt.

If the experiment had been tried with no further precautions, I apprehended that considerable difficulties would have occurred, from want of knowing whether the cold of the mixture was sufficiently great, and when a sufficient quantity of the quicksilver was frozen; for, in the first place, there would be no judging when a sufficient quantity was frozen without taking out the apparatus now and then to examine it, which could not

be done without a lofs of cold; and what is ftill worfe, if before the experiment was completed the cold of the mixture was fo much abated as to become lefs than that of congealing mercury, the frozen quickfilver would begin to melt, and the operator would have no way of detecting it, but by finding that great part of his labour was undone. For this reafon two other mercurial thermometers were fent called A and B by Mr. HUTCHINS, the fcales of which were of wood, for which reafon I fhall call them, for fhortnefs, the wooden thermometers, as I fhall call the two others the ivory ones, their fcales being of that material; they were graduated to about 600° below nothing, and their balls were nearly equal in diameter to the fwelled part of the cylinders, in order that the quickfilver in both fhould cool equally faft; and it was recommended to Mr. HUTCHINS to put one of thefe into the freezing mixture along with the apparatus: for then, if the cold of the mixture was fufficient, both thermometers would fink faft till the quickfilver in the cylinder began to freeze, when the ivory thermometer would become ftationary, but the wooden one would ftill continue to fink, on account of the contraction of the quickfilver in its ball by freezing; but if this laft thermometer, after having continued to fink for fome time after the ivory one had become ftationary, ceafed at laft to defcend, it would fhew, that the mixture was no longer cold enough to freeze mercury; for as long as that was the cafe, the wooden thermometer would continue to defcend by the freezing of frefh portions of quickfilver in its ball, but would ceafe to do fo as foon as the cold was at all lefs than that. As I was afraid, however, that the quickfilver might poffibly freeze and ftick tight in the tube of this thermometer, and prevent its finking, which would make the cold of the mixture appear too fmall
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when in reality it was not, one of theſe thermometers inſtead of having a vacuum above the quickſilver as uſual, was made with a bulb at top filled with air, in order that the preſſure might ſerve to force down the quickſilver.

If the degree of cold at which mercury freezes had been known, a ſpirit thermometer would have anſwered better; but that was the point to be determined.

Another advantage which I expected from the wooden thermometer was, that it would afford a gueſs when a ſufficient quantity of the quickſilver in the cylinder was frozen; for if the cold was continued long enough to make that thermometer ſink to near 400° below nothing, I ſuppoſed, a very viſible portion of the quickſilver would be frozen.

It muſt be obſerved, however, that in Mr. HUTCHINS's experiments the natural cold approached ſo near to the point of mercurial congelation, and in conſequence the freezing mixture retained its cold ſo long as to make theſe precautions of not ſo much uſe as they would otherwiſe have been.

As it appeared, from Mr. HUTCHINS's table of compariſon, that theſe thermometers did not agree well together, they were all examined after they came back, except the ivory thermometer F, which was broke before it arrived. This loſs, however, is of little conſequence, as it appeared from the above-mentioned table, that F and G agreed well together. The boiling and freezing points were firſt examined in the preſence of Sir JOSEPH BANKS, Dr. BLAGDEN, Mr. HUTCHINS, Mr. NAIRNE, and myſelf, when the diviſions on the ſcale anſwering thereto were found to be as follows:

Boiling

		Boiling point.		Freezing point.
A	-	220,3	-	29,9
B	-	218,8	-	30,9
G	-	215,3	-	32

The boiling point was tried in the manner recommended in the report of the Committee of the Royal Society, printed in the Philoſophical Tranſactions for the year 1777, and allowance made, as there directed, for the height of the barometer at that time. In fixing the freezing point alſo allowance was made for the temperature of the room in which it was tried.

The great difference in the poſition of the boiling point on theſe thermometers ſeems owing only to care not having been taken to keep the quickſilver in the tube of the ſame heat as that in the ball, which is a circumſtance that was very little attended to when they were made; and I am afraid is not ſo much obſerved at preſent as it ought to be, and which in A and B, whoſe tubes contained upwards of 900° of quickſilver, cauſed an exceſſively great error, and much more than it did in G, which contained fewer degrees in its tube.

In order to ſee whether the inequalities of the bore of the tube were properly allowed for, a column of quickſilver, about 100° long, was ſeparated from the reſt; and it was examined, whether its length comprehended the ſame number of degrees on the ſcale in different parts of the tube; when no ſenſible error could be found in this reſpect in G, and none worth regarding in B. The thermometer A, by reaſon of its being conſtructed with a bulb filled with air at top, could not be examined in this manner; but there is no reaſon to think, that it was faulty in this reſpect.

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From what has been faid it appears, that 183°,3 on the fcale of G are equal to only 180° on a thermometer adjufted as recommended by the Committee, and therefore 72° are equal to $70°\frac{2}{3}$; fo that the point of $-40°$ anfwers really to $-38°\frac{2}{3}$; that is, the cold fhewn by this thermometer at the temperature of about $-40°$ is $1°\frac{1}{3}$ too great. In like manner it appears, that the cold fhewn at that temperature by B is $4°\frac{1}{3}$, and by A $6\frac{1}{3}$, too great.

On the whole, thefe thermometers feem to have been carefully made, their difagreement being owing only to a faulty manner of adjufting the boiling point, and to not allowing for the temper of the air in fettling the degree of freezing; and as thefe points were examined after they came back, the experiments made with them are juft as much to be depended on as if they had been truly adjufted at firft.

Thefe inftruments were made in the year 1776, and were intended to have been fent to Mr. HUTCHINS that year, through the hands of the late Dr. MATY, who promifed to recommend the experiment to him; but, by not being got ready time enough to be fent that year, and a miftaken fuppofition that Mr. HUTCHINS was to come back the next fummer, they were prevented from being fent till 1781; when Sir JOSEPH BANKS was informed by Mr. WEGG, that there was a gentleman at Hudfon's Bay who was willing to undertake any experiments of that kind; and that the Hudfon's Bay Company would be at the expence of any inftruments neceffary for the purpofe. Then, as Sir JOSEPH thought the abovementioned apparatus well adapted to the purpofe, I gladly embraced the opportunity of fending it. It appears, however, from the letter inferted by Mr. HUTCHINS, that Dr. BLACK, without being acquainted with

with what I had done, recommended nearly the fame method of determining the degree of cold at which mercury freezes.

Befides the abovementioned inftruments, there were fent to Mr. HUTCHINS two fpirit thermometers and a thermometer marked C, made at the expence of the Hudfon's Bay Company. The two fpirit thermometers were made at the recommendation, and under the infpection of Dr. BLAGDEN, and were of great ufe, as they ferve to afcertain feveral circumftances relating to the experiments, which could not otherwife have been determined. The intention of the thermometer C will be mentioned in the courfe of this paper.

Before I enter into the examination of Mr. HUTCHINS's experiments, it will be proper to take notice of a phenomenon which occurs in the freezing of water, and is now found to take place in that of quickfilver, and which occafioned many remarkable appearances in thefe experiments.

It is well known, that if a veffel of water, with a thermometer in it, is expofed to the cold, the thermometer will fink feveral degrees below the freezing point, efpecially if the water is covered up fo as to be defended from the wind, and care is taken not to agitate it; and then, on dropping in a bit of ice, or on mere agitation, fpiculæ of ice fhoot fuddenly through the water, and the inclofed thermometer rifes quickly to the freezing point where it remains ftationary *.

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* Though I here fay conformably to the common opinion, that mere agitation may fet the water a freezing, yet fome experiments, lately made by Dr. BLAGDEN, feem to fhew, that it has not much, if any, effect of that kind, otherwife than by bringing the water in contact with fome fubftance colder than itfelf. Though in general alfo the ice fhoots rapidly, and the inclofed thermometer rifes very quick; yet I once obferved it to rife very flowly, as, to the beft of my remembrance, it took up not lefs than half a minute before it rofe to the freezing point;

This ſhews, that water is capable of being cooled conſiderably below the freezing point, without any congelation taking place; and that, as ſoon as by any means a ſmall part of it is made to freeze, the ice ſpreads rapidly through the remainder of the water. The cauſe of the riſe of the thermometer, when the water begins to freeze, is the circumſtance now pretty well known to philoſophers, that all, or almoſt all, bodies by changing from a fluid to a ſolid ſtate, or from the ſtate of an elaſtic to that of an unelaſtic fluid, generate heat; and that cold is produced by the contrary proceſs. This explains all the circumſtances of the phenomenon perfectly well; for as ſoon as any part of the water freezes, heat will be generated thereby in conſequence of the abovementioned law, ſo that the new formed ice and remaining water will be warmed, and muſt continue to receive heat by the freezing of freſh portions of water, till it is heated exactly to the freezing point, unleſs the water could become quite ſolid before a ſufficient quantity of heat was generated to raiſe it to that point, which is not the caſe; and it is evident, that it cannot be heated above the freezing point, for as ſoon as it comes thereto, no more water will freeze, and conſequently no more heat will be generated.

The reaſon why the ice ſpreads all over the water, inſtead of forming a ſolid lump in one part, is, that as ſoon as any ſmall portion of ice is formed, the water in contact with it will be ſo much warmed as to be prevented from freezing; but the water at a little diſtance from it will ſtill be below the freezing point, and will conſequently begin to freeze.

point; but in this experiment the water was cooled not more than one or two degrees below freezing; and it ſhould ſeem, that the more the water is cooled below that point, the more rapidly the ice ſhoots, and the incloſed thermometer riſes.

If it was not for this generation of heat by the act of freezing, whenever a vessel of water, exposed to the cold, was arrived at the freezing point, and began to freeze, the whole would instantly be turned into solid ice; for as the new formed ice is not sensibly colder than water beginning to freeze, it follows, that as soon as all the water in the vessel was cooled to that point, the least addition of cold would convert the whole into ice; whereas it is well known, that though the whole vessel of water is cooled to, or even below, the freezing point, there is a long interval of time between its beginning to freeze and being intirely frozen, during all which time it does not grow at all colder.

In like manner, it is the cold generated by the melting of ice which is the cause of the long time required to thaw ice or snow. It is this also which is the cause of the cold produced by freezing mixtures; for no cold is produced by mixing snow with any substance, unless part of the snow is dissolved.

I formerly found, by adding snow to warm water, and stirring it about till all was melted, that the water was as much cooled as it would have been by the addition of the same quantity of water, rather more than 150° colder than the snow; or, in other words, somewhat more than 150° of cold are generated by the thawing of snow; and there is great reason to think, that just as much heat is produced by the freezing of water. The cold generated was exactly the same whether I used ice or snow *.

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* I am informed, that Dr. BLACK explains the abovementioned phenomena in the same manner; only, instead of using the expression, heat is generated or produced, he says, latent heat is evolved or set free; but as this expression relates to an hypothesis depending on the supposition, that the heat of bodies is owing to their

I have formerly kept a thermometer in melted tin and lead till they became ſolid; the thermometer remained perfectly ſtationary from the time the metal began to harden round the ſides of the pot till it was intirely ſolid; but I could not perceive it to ſink at all below that point, and riſe up to it when the metal began to harden. It is not unlikely, however, that the great difference of heat between the air and melted metal might prevent this effect from taking place; ſo that though I did not perceive it in thoſe expriments, it is not unlikely that thoſe metals, as well as water and quickſilver, may bear being cooled a little below the freezing or hardening point (for the hardening of melted metals and freezing of water ſeems exactly the ſame proceſs) without beginning to loſe their fluidity.

Mr. HUTCHINS's five firſt experiments were made with the apparatus, and in the manner above deſcribed. In the firſt experiment the ivory thermometer, incloſed in the cylinder, ſunk to $-40°$, where it remained ſtationary for about half an hour, though the wooden thermometer, placed in the ſame mixture, kept ſinking almoſt all the while. At the end of that time the apparatus was taken out of the mixture to be examined, and the quickſilver in the cylinder was found frozen. It ſeems evident, therefore, that the true point at which mercury freezes is 40° below nothing on the thermometer F, which was that made uſe of in the experiment. It cannot be lower than that,

their containing more or leſs of a ſubſtance called the matter of heat; and as I think Sir ISAAC NEWTON's opinion, that heat conſiſts in the internal motion of the particles of bodies, much the moſt probable, I choſe to uſe the expreſſion, heat is generated. Mr. WILKE alſo, in the Tranſactions of the Stockholm Academy of Sciences, explains the phenomena in the ſame way, and makes uſe of an hypotheſis nearly ſimilar to that of Dr. BLACK. Dr. BLACK, as I have been informed, makes the cold produced by the thawing of ſnow 140°; Mr. WILKE, 130°.

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for if it was, the thermometer could not have remained ſo long ſtationary at that point, while ſurrounded with freezing quickſilver; and it cannot be higher, as the thermometer could not ſink below the freezing point, while much of the quickſilver, with which it was ſurrounded, remained unfrozen.

To thoſe who have attended to the former part of this paper it is needleſs ſaying, that the reaſon why the wooden thermometer continued ſinking ſo long after the ivory thermometer became ſtationary is, that as the former was placed in the freezing mixture, the quickſilver in its ball froze, and therefore it continued deſcending during the greateſt part of that half hour, by the continual freezing of freſh portions of quickſilver in its ball, and the contraction occaſioned thereby; whereas the latter, which was placed only in freezing quickſilver, did not freeze.

There is a circumſtance, however, in this experiment, the reaſon of which does not ſo readily appear; namely, on putting back the apparatus into the freezing mixture, after it was taken out to be examined, the thermometer ſunk to $-42°$; but in about four or five minutes returned back to $-40°$. The like happened on removing the apparatus into a freſh freezing mixture, and it then remained about ten minutes before it returned to $-40°$. It ſeems probable from this, that the quickſilver in the cylinder became intirely frozen about the time that it was firſt taken out to be examined, and that it then grew 2° colder than the freezing point; and that this degree of cold was not ſufficient to make the quickſilver in the incloſed thermometer freeze, ſince mercury, as was before ſaid, will bear being cooled a little below its freezing point without freezing. What confirms this explanation is, that the ſpirit thermometers ſhew that the cold of the mixture was actually much the ſame as that ſhewn by the ivory thermometer.

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In the ſecond experiment, tried with the ſame apparatus, the ivory thermometer quickly ſunk to $-43°$; but, in about half a minute, roſe to $-40°$, where it remained ſtationary for upwards of 17′. It appears, therefore, that in this experiment the quickſilver was cooled 3° below the freezing point, without loſing its fluidity; it then began to freeze, and the incloſed thermometer immediately roſe to $-40°$: ſo that this experiment, beſides confirming the former, ſhews, that quickſilver is capable of being cooled a little below the freezing point without freezing; and that it ſuddenly riſes up to it as ſoon as it begins to loſe its fluidity.

In this experiment the cold was carried far enough to freeze the quickſilver in the ivory thermometer, which was not the caſe in the former: for after it had remained 17′ ſtationary at $-40°$, it began to ſink again, and in about a minute ſunk to $-44°\frac{1}{2}$; it then ſunk inſtantaneouſly to $-92°$, and ſoon after remained fixed for an hour and a quarter at 95°; being then left without examination for three-quarters of an hour, the mercury was found to have ſunk into the ball, the ſpirit thermometer ſhewing at that time that the mixture was rather above the point of freezing, whereas before it had been below it. It appears, therefore, that the quickſilver in the thermometer, after having deſcended to $-44°\frac{1}{2}$, froze in the tube, and ſtuck there; but, being by ſome means looſened, ſunk inſtantly to $-92°$, and again ſtuck tight at $-95°$, till at laſt the mixture riſing above the freezing point, the quickſilver in the tube melted, and ſunk into the ball, to ſupply the vacuum formed there by the frozen quickſilver. A ſimilar accident of the quickſilver freezing in the tube of the thermometer, and ſticking there, and then melting and ſinking into the ball as the weather grew warmer, has been found by Dr. BLAGDEN to

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have happened to ſeveral gentlemen whoſe thermometers froze by the natural cold of the atmoſphere, and with reaſon cauſed much perplexity to ſome of them.

In this experiment the apparatus was not taken out to be examined till the ivory thermometer had ſunk to − 95°; it was then found to be frozen ſolid.

The third experiment was tried while the former was carrying on, and was made by putting the other apparatus, namely, that with the thermometers G and B, into the firſt mixture made for the former experiment, and which may conſequently be ſuppoſed to have loſt great part of its cold. The ivory thermometer quickly ſunk to − 43°, where it remained ſtationary for near 12′. The apparatus being then taken out to be examined, the quickſilver in the cylinder was found fluid, but thick and in grains, like crumbs of bread. The apparatus was then put back into the mixture; and, on obſerving the thermometer, it was found to have riſen to − 40°, where it remained ſtationary about 40′; being then examined, the quickſilver was found ſolid.

It appears, therefore, that the cold of the mixture was ſufficient to cool the quickſilver in the cylinder about 3° below the point of freezing, but did not make it freeze till, on taking out the apparatus, the agitation ſuddenly ſet it a freezing, and produced the appearance deſcribed by Mr. HUTCHINS. This immediately made the incloſed thermometer riſe; ſo that when it was re-placed in the mixture and obſerved, it ſtood exactly at the freezing point. It appeared, by the ſpirit thermometer, that the cold of the mixture, at the time the apparatus was firſt taken out to be examined, was only 2° below the point of freezing, which agrees very well with this explanation.

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This experiment, therefore, affords a fresh confirmation that the point of mercurial congelation is $-40°$ on these thermometers; and that quicksilver will bear being cooled a little below that point without freezing.

As in these two experiments the quicksilver in the cylinder and ivory thermometer bore being cooled a few degrees below the freezing point without freezing, it is natural to conclude, that the same fluid in the wooden thermometer should do so too; and it may, perhaps, be supposed that, in consequence of it, this thermometer, after having sunk a little below the point of freezing, ought suddenly to have risen up to it, which was not observed. But there is great reason to think, that though the quicksilver in it did bear cooling in this manner, it would not have occasioned any such appearance: for suppose that it is cooled below the freezing point, and then suddenly freezes, its bulk will be increased, on account of the heat generated thereby; but then it will be diminished on account of the contraction in freezing; so that, unless the expansion by the heat generated exceeds the contraction by freezing it will cause no rise in the thermometer. I do not, indeed, know how much the heat generated by freezing in quicksilver is, but in water it is about 150°, and the contraction by freezing is at least as much as its expansion by 400°; so that, unless the heat generated by freezing is two or three times as great in quicksilver as in water, the thermometer ought not to rise on this account.

In the fourth, fifth, sixth, and seventh experiments a new phenomenon occurred, namely, the ivory thermometer sunk a great deal below the freezing point without ever becoming stationary at $-40°$. In the fifth experiment, tried with the apparatus G, it quickly sunk to $-42°$, and then, without remaining stationary at any point, sunk in half a minute to

− 72°, and soon after remained fixed at − 79°. While it was at − 79°, the apparatus was twice examined, and the quicksilver found fluid; but being again examined after having been removed into a fresh mixture, it was found solid.

It seems likely from hence, that the quicksilver, in the cylinder was quickly cooled so much below the freezing point as to make that in the inclosed thermometer freeze, though it did not freeze itself. If so, it accounts for the appearances perfectly well; nor does there seem any thing improbable in the explanation, except that it is contrary to what happened in the three first experiments; but the degree to which fluids will bear being cooled below the freezing point without freezing seems to depend on such minute circumstances, that, I think, this forms no objection. It must be observed, that the cold of the mixture appeared by the spirit thermometer to be five or six degrees below the freezing point; so that if the quicksilver in the cylinder was as cold as the mixture, and I have no reason to think it was not, it is not at all extraordinary that the thermometer should have froze; the only thing extraordinary is, that the quicksilver in the cylinder should have borne that cold without freezing.

The same phenomenon occurred in the sixth and seventh experiments, on putting the same apparatus into the freezing mixture.

In the fourth experiment the ivory thermometer sunk quickly to − 42°; but soon after rose half a degree, probably from the cold of the mixture diminishing; it then, after having remained six or seven minutes at those two points, sunk very quick to − 77°. It does not appear, at what time the quicksilver in the cylinder began to freeze, as it was not examined till long after the thermometer had sunk to − 77°, when it was

found ſolid; but from the reſemblance of this to the three former experiments, I think it much moſt likely, that it did not begin to freeze till after the thermometer had ſunk to – 77°.

In the fifth experiment the wooden thermometer was partly frozen before it was put into the freezing mixture, and the ivory one was at – 40°. On putting them into the mixture, they both roſe; the latter, half a degree; the former, many degrees; which ſhews that the part of the mixture in which they were placed was rather warmer than the freezing point, though that in which the ſpirit thermometer was placed was colder; but as there ſeems nothing to be learnt from this, it is not worth while entering into a detail of the circumſtances.

Though theſe experiments do not ſerve to ſhew what the freezing point of quickſilver is, yet they do not at all contradict the concluſion drawn from the three former.

If theſe experiments only had been made, I ſhould have been inclined to ſuppoſe, that quickſilver froze with a leſs degree of cold in vacuo than in the open air, as the quickſilver in the ivory thermometer was in vacuo, and that in the cylinder was not; but, as in the three former experiments, the event was different, the quickſilver in the cylinder there freezing firſt, I have no reaſon to think that this is the caſe.

Though in the ſixth experiment the thermometer in the apparatus G froze without the quickſilver with which it was ſurrounded freezing, yet in trying the apparatus F in the ſame mixture, this did not happen; but, on the contrary, it afforded as ſtriking a proof that the point of freezing quickſilver anſwers to about – 40° on this thermometer as any of Mr. HUTCHINS's experiments; for, on taking out the apparatus after it had been two minutes in the mixture, the quickſilver in the cylinder was found frozen ſolid, the incloſed ther-

mometer ſtanding at 40° or 41° below nothing. After having been expoſed for near an hour to the air, which was then very little above the point of freezing quickſilver, only a ſmall quantity of the ſurface was become fluid; the reſt formed a frozen globe round the ball of the thermometer, reſembling poliſhed ſilver, and in 17′ after this only a ſegment of a globe of frozen quickſilver, with a concavity on the inſide, formed by the ball of the thermometer, was obſerved, the thermometer all this while continuing the ſame as before, namely, at 40° or 41° below nothing; ſo that in this experiment the ball of the thermometer was ſurrounded for more than an hour with quickſilver, which was viſibly frozen and ſlowly melting, and during all which time it continued ſtationary at 40° or 41° below nothing.

It muſt be obſerved, however, that in the firſt and ſecond experiments, which were both tried with this apparatus, the freezing point came out exactly −40°, whereas in this it ſeemed about half a degree lower; the reaſon of which, in all probability, is, that the tube of this thermometer was not ſo well fitted to its ſcale but that it had a little play, which would make the freezing point appear near half a degree higher or lower, according as the tube was puſhed up or down.

Though the foregoing experiments leave no reaſonable room to doubt, that this is the true point at which quickſilver freezes, yet Mr. HUTCHINS has, if poſſible, made this ſtill more evident by his two laſt experiments; as, in the firſt of them, he froze ſome quickſilver in a gally-pot immerſed in a freezing mixture, ſo that the quickſilver was in contact with, and covered by, the ſnow and ſpirit of nitre; and in the latter in the open air, by the natural cold of the weather, and then dipping the ball of the thermometer into the unfrozen part, obſerved

what degree it ſtood at. Theſe experiments agree with the former in ſhewing the freezing point to be $-40°$ on the two mercurial thermometers; and alſo ſhew what degree on the ſpirit thermometers anſwers thereto, namely, $29°\frac{3}{4}$ or $28°\frac{1}{2}$ on D, and $30°$ on E; for in theſe two experiments the ſpirit thermometers alſo were dipped into the frozen quickſilver.

In all the experiments, therefore, tried with the thermometer G, the freezing point came out $-40°$. In thoſe tried with F, it came out either $-40°$, or about $-40°\frac{1}{2}$; ſo that as it appears, from Mr. HUTCHINS's table of compariſon, that F ſtood at a medium a quarter of a degree lower than G, the experiments made with that thermometer alſo ſhew the freezing point to be $-40°$ on G; and as it appeared from the examination of this thermometer after it came home, that $-40°$ thereon anſwers to $-38\frac{2}{3}$, on a thermometer adjuſted in the manner recommended by the Committee of the Royal Society, it follows, that all the experiments agree in ſhewing that the true point at which quickſilver freezes is $38°\frac{2}{3}$, or in whole numbers $39°$ below nothing.

From what has been ſaid it appears, that the point at which quickſilver freezes has been determined by Mr. HUTCHINS in different ways, all perfectly ſatisfactory, and all agreeing in the ſame reſult. In the three firſt experiments the thermometer was ſurrounded by quickſilver, which continued freezing till it became ſolid. In the ſixth experiment the quickſilver with which it was ſurrounded continued ſlowly melting till the whole was diſſolved; and in both caſes the thermometer remained ſtationary all the while at what we have juſt ſaid to be the freezing point. In the ninth and tenth experiments, the ball of the thermometer was dipped into quickſilver, previouſly frozen and beginning to melt, as uſually practiſed in ſettling the

freezing point on thermometers, and agreed in the ſame reſult, the quickſilver in the laſt experiment being frozen by the natural cold of the atmoſphere; and in the former, by being immerſed in, and in contact with, a freezing mixture; ſo that this point appears to be determined in as ſatisfactory a manner as can be deſired; and the more ſo, as it ſeems impoſſible that experiments ſhould be made with more care and attention, or more faithfully and circumſtantially related than theſe have been. The ſecond and third experiments alſo ſhew, that quickſilver, as well as water, can bear being cooled a little below the freezing point without freezing, and is ſuddenly heated to that point as ſoon as it begins to congeal.

On the contraction of quickſilver in freezing.

All theſe experiments prove, that quickſilver contracts or diminiſhes in bulk by freezing; and that the very low degrees to which the thermometers have been made to ſink, is owing to this contraction, and not to the cold having been in any degree equal to that ſhewn by the thermometer. In the fourth experiment the thermometer A ſunk to – 450°, though it appeared by the ſpirit thermometers that the cold of the mixture was not more than 5° or 6° below the point of freezing quickſilver. In the firſt experiment alſo, it ſunk to – 448°, at a time when the cold of the mixture was only 2°½ below that point; ſo that it appears, that the contraction of quickſilver, by freezing, muſt be at leaſt equal to its expanſion by 404° of heat *. This, how-

* The numbers here given are thoſe ſhewn by the thermometer without any correction; but if a proper allowance is made for the error of that inſtrument it will appear, that the true contraction was 25° leſs than here ſet down, and from the manner in which thermometers have been uſually adjuſted, it is likely, that in the following experiment of Mr. HUTCHINS, as well as thoſe of Profeſſor BRAUN, the true contraction might equally fall ſhort of that ſhewn by obſervation.

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ever, is not the whole contraction which it suffers; for it appears, by an extract which Mr. HUTCHINS was so good as to give me from a meteorological journal, kept by him at Albany Fort, that his thermometer once sunk to 490° below nothing, though it appeared, by a spirit thermometer, that the cold scarcely exceeded the point of freezing quicksilver. There are two experiments also of Professor BRAUN, in which the thermometer sunk to 544° and 556° below nothing, which is the greatest descent he ever observed without the ball being cracked. It is not indeed known how cold his mixtures were; but from Mr. HUTCHINS's, there is great reason to think that they could not be many degrees below − 40°. If so, the contraction which quicksilver suffers in freezing is sometimes not much less than its expansion by 500° or 510° of heat, that is almost $\frac{1}{23}$d of its whole bulk, and in all probability is never much more than that.

It is very likely, however, that the contraction which quicksilver suffers in freezing is no very determinate quantity; for a considerable difference may frequently be observed in the specific gravity of the same piece of metal, cast different times over, and almost all cast metals become heavier by hammering; and it is likely that the same thing may obtain in quicksilver, which is only a metal which melts with a much less degree of heat than the rest. I do not know, indeed, how much this variation can amount to; but, on casting the same piece of tin three times over, I found its density to vary from 7,252 to 7,294, though I have great reason to think that no hollows were left in it, and that only a small part of this difference could proceed from the error of the experiment. This variation of density is as much as is produced in quicksilver by an alteration of 66° of heat; and it is not unlikely, that the descent of a thermometer, on account of the contraction of the quicksilver in its ball by freezing,

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may vary as much in different trials, though the whole maſs of quickſilver is frozen and without any vacuities.

The thermometer marked C was intended for trying how much the contraction of quickſilver is; but the experiments made with it were not attended with ſucceſs, as in the firſt experiment it did not ſink ſo low as A had done, owing, moſt likely, to the great cold of the weather which froze the quickſilver in the tube; and in the ſecond experiment the ball broke.

On the cold of the freezing mixtures.

The cold produced by mixing ſpirit of nitre with ſnow is owing, as was before ſaid, to the melting of the ſnow. Now, in all probability, there is a certain degree of cold in which the ſpirit of nitre, ſo far from diſſolving ſnow, will yield out part of its own water, and ſuffer that to freeze, as is the caſe with ſolutions of common ſalt; ſo that if the cold of the materials before mixing is equal to this, no additional cold can be produced. If the cold of the materials is leſs, ſome increaſe of cold will be produced; but the total cold will be leſs than in the former caſe, ſince the additional cold cannot be generated without ſome of the ſnow being diſſolved, and thereby weakening the acid, and making it leſs able to diſſolve more ſnow; but yet the leſs the cold of the materials is, the greater will be the additional cold produced. This is conformable to Mr. HUTCHINS's experiments; for in the fifth experiment, in which the cold of the materials was $-40°$, the additional cold produced was only 5°. In the firſt experiment, in which the cold of the materials was only $-25°$, an addition of at leaſt 19° of cold was obtained; and by mixing ſome of the ſame ſpirit of nitre with ſnow in this climate, when the heat of the materials

materials was + 26°, I have ſunk the thermometer to − 29°; ſo that an addition of 55° of cold was produced.

It is remarkable, that in none of Mr. HUTCHINS's experiments the cold of the mixture was more than 6° of the ſpirit thermometer below the point of freezing quickſilver, which is ſo little that it might incline one to think, that the ſpirit of nitre uſed by him was weak. This, however, was not the caſe, as its ſpecific gravity at 58° of heat was 1,4923. It was able to diſſolve $\frac{1}{1,42}$ its weight of marble, and contained very little mixture of the vitriolic or marine acid: as well as I could judge from what experience I have of ſpirit of nitre, it was as little phlogiſticated as acid of that ſtrength uſually is.

But, however extraordinary it may at firſt appear, there is the utmoſt reaſon to think, that a rather greater degree of cold would have been obtained if the ſpirit of nitre had been weaker; for I found, by adding ſnow gradually to ſome of this acid, that the addition of a ſmall quantity produced heat inſtead of cold; and it was not until ſo much was added as to increaſe the heat from 28° to 51°, that the addition of more ſnow began to produce cold; the quantity of ſnow required for this purpoſe being pretty exactly one-quarter of the weight of the ſpirit of nitre, and the heat of the ſnow and air of the room, as well as of the acid, being 28°. The reaſon of this is, that a great deal of heat is produced by mixing water with ſpirit of nitre, and the ſtronger the ſpirit is, the greater is the heat produced. Now it appears from this experiment, that before the acid was diluted, the heat produced by its union with the water formed from the melted ſnow was greater than the cold produced by the melting of the ſnow; and it was not till it was diluted by the addition of one-quarter of its weight of

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that fubftance, that the cold generated by the latter caufe began to exceed the heat generated by the former. From what has been faid it is evident, that the cold of a freezing mixture, made with the undiluted acid, cannot be quite fo great as that of one made with the fame acid, diluted with a quarter of its weight of water, fuppofing the acid and fnow to be both at 28° of heat, and there is no reafon to think, that the event will be different if they are colder; for the undiluted acid will not begin to generate cold until fo much fnow is diffolved as to increafe its heat from 28° to 51°, fo that no greater cold will be produced than would be obtained by mixing the diluted acid heated to 51° with fnow of the heat of 28°. This method of adding fnow gradually to an acid is much the beft way I know of finding what ftrength it ought to be of, in order to produce the greateft effect poffible.

By means of this acid, diluted in the above-mentioned proportion, I froze the quickfilver in the thermometer called G by Mr. HUTCHINS, on the 26th of laft February. I did not, indeed, break the thermometer to examine the ftate of the quickfilver therein; for as it funk to −110° it muft certainly have been in part frozen; but immediately took it out, and put the fpirit thermometer in its room, in order to find the cold of the mixture. It funk only to −30°; but, by making allowance for the fpirit in the tube being not fo cold as that in the ball, it appears, that if it had not been for this caufe it would have funk to −35°*, which is 5° below the point of freezing, and is

* As the furface of the freezing mixture anfwered to −185° on the tube, there were 155° of fpirit in the tube which could hardly be cooled much below the temper of the air, and which muft, therefore, be warmer than that in the ball by about 55° of this thermometer, as the heat of the fpirit in the ball was before faid

is as great a degree of cold, within 1°, as was produced in any of Mr. HUTCHINS's experiments.

In this experiment the thermometer G ſunk very rapidly, and, as far as I could perceive, without ſtopping at any intermediate point, till it came to the above-mentioned degree of − 110°, where it ſtuck. The materials uſed in making the mixture were previouſly cooled, by means of ſalt and ſnow, to near nothing; the temper of the air was between 20° and 25°; the quantity of acid uſed was $4\frac{1}{4}$ oz.; and the glaſs in which the mixture was made was ſurrounded with wool, and placed in a wooden box, to prevent its loſing its cold ſo faſt as it would otherwiſe have done.

Some weeks before this, I made a freezing mixture with ſome ſpirit of nitre, much ſtronger than that uſed in the foregoing experiment, though not quite ſo ſtrong as the undiluted acid, in which the cold was leſs intenſe by $4°\frac{1}{2}$, as the thermometer G ſunk to $-40°\frac{1}{2}$. It is true, that the temper of the air was much leſs cold, namely, 35°; but the ſpirit of nitre was at leaſt as cold, and the ſnow not much leſs ſo. The experiment was tried in the ſame veſſel and with the ſame precautions as the former.

The cold produced by mixing oil of vitriol, properly diluted with ſnow, is not ſo great as that procured by ſpirit of nitre, though it ſeems not to differ from it by ſo much as 8°; for a freezing mixture, prepared with diluted oil of vitriol, whoſe

ſaid to be − 35°, and the temper of the air above +20. Therefore, the correction muſt be equal to the expanſion of a column of ſpirits 155° long, by an alteration of heat equal to 55° on this thermometer, which, if 1° on the ſcale anſwers to $\frac{1}{1700}$th of the bulk of the ſpirit, is equal to $\frac{55 \times 155}{1700}$ or 5°.

ſpecific gravity, at 60° of heat, was 1,5642, ſunk the thermometer G to − 37°, the experiment being tried at the ſame time, and with the ſame precautions, as the foregoing. It was previouſly found, by adding ſnow gradually to ſome of this acid, as was done by the ſpirit of nitre, that it was a little, but not much ſtronger than it ought to be, in order to produce the greateſt effect.