possible to land and winter. Continuing their course to the west, they intend running along by the "Southern Continent," where the existence of land is certain, and endeavour to penetrate through the ice, as did D'Urville, Wilkes, and Ross. The hope is that canals in the ice might be found through which they might attain a remote latitude, or, running along them when massed into a continent, arrive at Kemp or Endermet, where they could pass the second winter.

A JAPANESE paper states that the Swedish skipper Johannesen, who has already done a good deal of exploration in the Spitzbergen seas, is to set out this month from Yokohama in the steamer *Nordenskjöld* to make the North-East Passage in an opposite direction to that taken by the *Vega*, viz., from Behring Straits to Europe.

PROF. NORDENSKJÖLD reached Copenhagen at the end of last week, in the *Vega*, and was received on landing by the acclamations of 20,000 persons. Every one has united to do him and his companions honour, from the Royal Family downwards.

THE death is announced, on the 16th inst., of Mr. Robert Fortune, well known as a botanical collector in China and Japan, and author of several volumes describing his travels in those countries in search of new plants. It was he who introduced the tea-plant from China into the North-West Provinces of India. He was born in 1812.

ON THE EMPLOYMENT OF THE PENDULUM FOR DETERMINING THE FIGURE OF THE EARTH

MY object in writing this paper is principally to draw attention to the course which the employment of pendulums has taken, from the time when Richer's first experiment at Cayenne, in 1672, attracted the attention of Newton; and to show in what respect the present aspect of the question is different from that which successive observers, as well as writers upon the subject, have at various times taken. It is no part of my object to discuss the observations themselves, or to discriminate between them, still less to enter upon any investigation of the figure of the earth, except incidentally in alluding to the conclusions which different writers have accepted. But as it is nearly impossible—perhaps not altogether desirable—to hold no independent opinions, I may add that I hope to be able to influence the future course of such operations in a certain direction which will be recognisable as we proceed.

The literature of the subject is very extensive,—not so much in respect of the pendulum itself, or of the use which has been made of it, as on account of the intimate relation which the laws which govern its motion have to larger questions. It is the discussion of these that experiments with the pendulum have influenced, and in general it is only with reference to such influence that the experiments have been instituted, described, and considered; and that in close connection with other operations of wholly different character. It is thus nearly impossible to have a thorough knowledge of the history of pendulum operations without at least a general acquaintance with the history of geodesy, and of that part of astronomical and mathematical literature which deals with the probable forms and constitutions of cosmical bodies. This would be less the case than it is if some of the many writers on the figure of the earth had written less exclusively from their own point of view, and (at any rate in writing of pendulum operations) had dealt more fully with the historical aspect of this particular branch of the general subject: I mean in the modern sense of the word; describing not only the sequence of experiments, but also the development of the comprehension of the questions in issue. I have felt the want of this myself so strongly that now that a somewhat protracted study has partly supplied that want I am fain to attempt this review, in aid of those who may have to prosecute the work.

It is of course impossible to present the course of pendulum operations without continually referring to their intention. At the same time one learns at last that, with one or two exceptions, the intention itself was not well grasped by those who conducted the experiments. Indeed one may almost say that even on the part of those who directed them the intention is not very clear; or more correctly, that it was more confined than we now might wish had been the case. Laplace was not perhaps the first to give utterance to the opinion that the anomalies noticeable in pendulum results were probably due rather to inequalities of figure than to errors of observation. Nevertheless it is with something of surprise, considering that the importance of his opinion lay latent so long as a practically unrecognised consideration, that we find him saying as follows:—"We shall here remark that the same anomalies. . . arising without doubt from the irregularity of the parts of the earth, are also perceived in the observed lengths of pendulums." That such irregularities existed was doubtless always a suspicion, but the fact was very slow of being recognised, and to this day it does not govern the observations.

In reviewing the course of pendulum operations then we must be prepared to put this aside as a fact which has not entered into account. It may be strange, but such is the case. It follows that a very considerable portion of the discussions and calculations, based on results which I am very far from wishing to impeach, must also be set aside as almost entirely without present value other than as evidence that the breadth of the question had not been measured.

If the absence of a true appreciation of the influence of local irregularity is apparent in the narrowness of the discussion of individual observations, or of small groups of results; it is also noticeable in the rejection of many, on the sole ground that the methods of observation were inferior; without any proof being adduced that the probable error was greater than the probable effect of local irregularity. This may be taken as indicating that there was also on the part of those who set themselves to review the produce of experiment a reluctance to accept as facts the irregularities which now we recognise as necessary concomitants.

Here again it follows that we must be ready to turn aside from conclusions which are seen to rest on the exclusion of an important consideration. But it by no means follows that, in thus finding reason on all hands to go back to the original sources, and to discard more or less summarily much which has been at one time or another accepted as legitimate deduction, there is any occasion to slight those deductions. Mere trials as they have often been, they have served many purposes which we cannot disdain, and (in ways which it is vain now to examine) have placed us in the more advanced position. There is one thing however which they must have no power to effect, and that is to obstruct us in further advance.

At the same time I confess that, for my own part, I cannot turn over the innumerable pages of vain calculations without profound regret that they represent so much labour—not thrown away—but without further use. I would give instances, but perhaps it is better to refrain. If anything could excuse it, it would be the hope of saving some other learner from spending time over them, and that object can perhaps be otherwise secured.

From another point of view I have also been led to perceive a want of distinctness in the plan of operations, which accounts for an otherwise inexplicable diversity in the individual contributions. In studying the history of these operations we are reminded of an edifice which presents different styles; and parts which make up a whole, not so much after any known design as casually. The two principal styles, to continue the metaphor, have indeed a common element which is a key to the whole construction; but though we can perceive that it is there in every part, and was present to the mind of every worker, it scarcely ever amounts to an expressed design by which future work is to be regulated. I allude to the absolute and differential methods. As we cannot properly appreciate the value of the work which has been done without understanding the relation in which these stand to each other, it is necessary to preface the merely historical account by a description of those methods in their relation both to the general purpose and to each other.

The conception of the earth as an oblate spheroid probably preceded the first use of the pendulum as an instrument by which its oblateness could be proved and measured. But the uncertainty which characterised geodetic measures—an uncertainty so great that it was, at a later date, actually the subject of vehement controversy whether the elipticity was not prolate—was such that Richer's discovery (in 1672) that the length of a pendulum beating seconds at Cayenne was notably less than that of a pendulum beating seconds at Paris, was from its very simplicity and conciseness, a revelation which promised inestimable consequences.

This was not the first time that a measurement of the seconds pendulum figures in the annals of geodesy. Picard had two years before adopted the idea—subsequently, towards the close of the next century, so nearly being given final effect to—of making the seconds pendulum the unit of length. But although he did actually determine the length at Paris, and thus unwittingly laid the first stone of the "absolute" method, it was not until the length had also been ascertained at another place and in another latitude that the firstfruits of the method could be gathered.

It is conceivable (though unlikely) that the pendulums used in the two places were the same. But this is of no consequence, since their lengths were different—the time of oscillation being constant.

Precisely the same result, in respect of variation of attractive force, would have been deduced had the same pendulum, of unchanged length, been used; the fact of observation in this case being a diminution of rate.

In the one case we have the absolute method; in the other we should have had the differential method. But in the former we have not only an indication of the variation of the force, but also a measure of its magnitude in terms of the measured lengths. Hence the designations.

It is well to remark here that unless the measurements at the two places refer directly, or can, by intermediary scales, be referred ultimately, to one and the same standard, there is no exact comparison, and no certain result. Further, that if, from any cause, the standard of reference is of unknown length, or, technically speaking, lost, the result is differential only. Thus we see that observations which were, in intention, of the absolute class, may fall into the differential class for want of reliable connection with existing standards of length.

Measurement of the absolute force of gravity at the earth's surface by means of a pendulum was doubtless contemplated by astronomers anterior to Richer's discovery of its variation. It is therefore scarcely permissible to recognise in the prosecution of this discovery the determination of the absolute force as a principal object. The object was distinctly to ascertain the variation at different parts of the surface. Such being the case, we must admit that the absolute method was not the simplest for the purpose. In due time this was recognised. It is doubtful to whom is due the credit of earliest perceiving that the measurement of the pendulum might be dispensed with, provided means were supplied of securing a constant length. Graham and Campbell (in 1732), and Bouguer (in 1735), were the earliest in the field;[1] and Bradley (in 1736) in describing Campbell's use of Graham's pendulum at Jamaica, very decidedly recommends the use of a pendulum of constant length. Bouguer and Godin at the same period were also using pendulums which were measured. From this time forward to the present day both methods have been practised.

I confess myself unable to frame arguments in favour of the measured pendulum, as an instrument for its purpose, which can at all account for its prolonged use. I mean of course if we are to presume an intimate acquaintance, on the part of the observer, with the meaning of his labours. Considering how fragmentary and scattered, and often unimportant, not to say mistaken, are the early writings on the use of the pendulum, it is scarcely invidious to say that such a presumption is sometimes gratuitous. The men who made scientific voyages in those days doubtless had something else to do than to study. Moreover, study was not very feasible when books were scarce and libraries few. Nevertheless one cannot help a curiosity as to the charm which protected the absolute method. Was it precedent?

There is something seductive, it must be admitted, in the conciseness and completeness which attends an absolute determination, as contrasted with the dependence of a differential one. To adjust a pendulum to such a length that it will beat seconds, and then to measure its length against a portable standard whose length has been ascertained—this is conclusive. When done, the worst that can happen to the instruments used is no worse than prevention of further use. This is true; and if the observer is full of confidence in the accuracy of his measurements, unconscious of the errors that lurk in reductions, and innocent of the insidious nature of instrumental mischances, why should he surrender the security of present gain? Nevertheless these flaws exist, and ultimately they are recognised and found irremediable. The more honour to those who foresee and provide against them, in preparing instructions, in conducting experiments to elucidate difficulties, or in multiplying observations by which the work of others may be consolidated.

[1] Mairan, in the same year, suggested it independently, and Lacondamine advocated and used one frequently.

Much of the doubtfulness which attaches to the earlier work is due to want of that knowledge which experience brings. Among the most important causes of error must be reckoned the imperfect comprehension which formerly existed as to the retarding influence of the air on the swinging pendulum. It was of course known that a body being lighter when suspended in a fluid than when in air, the oscillations of a pendulum when swung in air would be slower than when swung in a vacuum. No account was taken of this at first, but the time came when the effect was calculated by determining the diminution of weight due to flotation. Ultimately it was shown that there was also retardation due to the disturbance of the surrounding air, and it was surmised that this depended, not only on the bulk of the air displaced by the pendulum, but on the form of its surface as well. Of course it was immediately apparent that the old results must undergo some correction depending on the forms of the pendulums used; and equally, of course, the want of precise descriptions was then felt in a way the original observers never contemplated.

The difficulty was perhaps less real than apparent. As I have already pointed out, the magnitude of the force was not the object of the experiments, except as a means of inter-comparison. Hence any shortcoming which invalidated the determination of the absolute magnitude without rendering impossible that of the relative force, was of no real consequence. It was only necessary to abandon the idea of retaining a set of results in the absolute class, and to consider them as differential only; the *sine quâ non* being that such set were taken with one pendulum or with pendulums of the same size and construction. But I cannot remember a single instance in which the difficulty has been met in this way.

I have pointed out that the idea of measuring the force at any place absolutely arose anterior to that of measuring it relatively, and was afterwards retained as a means of securing the relative measure. Whatever interest attached to the determination of the force of gravity for itself, it is pretty certain that it went for little in the experiments which succeeded Richer's. It is therefore remarkable that even after the simpler differential method had been inaugurated it should still have held its ground. It is probable that Picard's idea[1] of a base or standard of length dominated to some extent the subsequent line of investigation. At any rate it is to his experiments that we must look for the rudiments of the instrument. I am[2] unfortunately unable to refer to the original memoirs in which these experiments are recorded, but I gather from other accounts that all the experimenters aimed at as near an approach to a "simple" pendulum as possible, and that even the celebrated form which Borda adopted in 1792 scarcely differed at all from the earlier ones. The following notice of Borda's experiments by Lalande, in his "Histoire Abregé de l'Astronomie," is noteworthy as sustaining the view which I am led to take of the vitality of the absolute method:—

"Le décret de l'Assemblée nationale qui, le 8 mai, ordonna la réforme des mesures en France, en indiquant le pendule à secondes pour mesure primitive, exigeait que la longueur du pendule fût déterminée avec une nouvelle précision. En 1735 Mairan avait fait ses observations avec bien du soin; mais alors on ne pouvait guère l'assurer d'un quinzieme de ligne. Borda espéra obtenir une précision bien plus grande par des moyens nouveaux; il l'entreprit donc cette année à l'Observatoire, avec des instruments faits d'après ses idées par C^en Lenoir, et il en résulta enfin une détermination du pendule de 36p. 8l. 60 réduite[3] à la température de 10°; et dans le vide; ce résultat, qui est à un cinquantieme de ligne, et mieux encore, a été obtenu avec un pendule de douze pieds de long."

The old form of pendulum consisted of a weight, of simple geometrical form, suspended by a fine wire or fibre. In Borda's the weight was spherical and attached by adhesion to a cup to which the wire was fastened. The object of this was to vary the position of the ball without detaching the wire. This attachment seems to be the *only* part of consequence which was without precedent.

In all these forms it is particularly to be noticed that the ball was made heavy and the suspension light and fine. Although the obvious intention of this was to attain to the

[1] Priority, in point of time, is due to Wren in this matter; but Mouton, from whom Picard got it (and perhaps also Huygens), no doubt evolved it independently, though some years later.

[2] This article was written in Calcutta, and no library there possesses the early volumes of the Paris Acad. *Memoirs*.

[3] Please to remark the absence of intelligible meaning in this. What is it that is reduced? and why?

nearest practical model of a simple pendulum, I cannot help seeing in it also an unconscious recognition that there was *resistance* to be met in the air as well as buoyancy. It is not credible that the resistance of the air to a body moving through it was not thought of, though it is intelligible that the effect of such resistance was so underrated or misunderstood as to be supposed insignificant.

Having mentioned Borda's pendulum in this connection, I will add that I do not find grounds for assenting unreservedly to the practice of assigning to him so large a share in the merit of inventing the measurable pendulum. He improved somewhat on an already existing form, and experimented with greater accuracy, if not with greater care, than his predecessors; but we must, I think, in justice associate Picard's name, and still more Graham's, with that form.[1]

About the year 1786 Whitehurst, carrying out an abortive idea of Hatton's, presented to the Royal Society, and afterwards withdrew and published independently, a paper describing experiments with a pendulum of the ball-and-wire construction, the use of which depended on a change in the length of the wire. I have not had access to the original paper, but if we may trust Saigey's account the experiments were singularly correct in their result. In all experiments with very long pendulums—indeed whatever be the length, but especially with long ones—the ultimate precision turns on the measurement of the distance between the upper and lower planes. The liability to error in such measurements is much better understood now than it was in Whitehurst's time, and it is somewhat doubtful if the correctness of his result may be accepted as an argument in favour of his method. But even if we had not ground for anticipating advantages in a method which secures, to some extent, the elimination of certain errors, the fact that Bessel adopted the same method in preference to employing either Borda's or Kater's pendulum, some forty years later, would go far to require that full recognition should be accorded to Whitehurst.

The result of the use of the single ball-and-wire pendulum, in whatever form, depends ultimately, as I have said, on the accuracy with which the distance can be measured between the point of support and the lower contact plane. The measurement, and perhaps also the distance itself, will vary with the temperature. The length of the pendulum also, and therefore its rate, will vary either with the temperature (if the suspension is by a wire) or with the dampness of the air (if by a fibre). If the temperature varies much during the time occupied by the experiment, the effects will be so complex—owing to the difference of the masses of the wire, the scale, and the support—that great precision can scarcely be expected. To some extent the uncertainties are eliminated when the experiment takes the form of a comparison between the rates of two pendulums of different lengths, but otherwise identical. In any case, however, there is an element of uncertainty peculiar to the quest of *length* distinct from that which is peculiar to the quest of *rate*.

It is not, I think, possible to form a correct conception of the progress of the research which was prosecuted by the help of the pendulum—still less to understand its present aspect—without grasping firmly the idea that the use of the absolute pendulum contemplated two distinct objects which had no essential connection, viz., the force of gravity and the figure of the earth; while the use of the differential pendulum contemplated one only of these. Of course I do not mean to imply that this distinction was not perceived; but I do suggest that no small portion of the difficulties which have attended the research are traceable to the frequent absence of a sufficient perception of the independence of the two quests. From the time of Graham, Bradley, and La Condamine, to the present day, while the ostensible main purpose, has been one, the methods have been two; and one of these was encumbered with a hardly acknowledged second purpose whose presence created a set of difficulties from which the single-minded purpose and method were free. This is so obvious, so well known, that it seems almost an impertinence to bring it forward. It is not so, however. Fully recognised and admitted as it must be allowed to be, the fact remains that notwithstanding the comparative failure of the absolute method and the acknowledged success of the differential, the tendency at this day is still to have recourse to the former, although the second purpose is now scarcely thought of as a real desideratum, the whole interest centring in variation, and variation only.

Let us consider the two objects of the absolute method separately; or rather, let us consider that one especially which is its peculiar object—the *length* of the equatorial seconds pendulum.

What is the equatorial seconds pendulum? It is a simple pendulum beating seconds under the force of gravity at, or near, the equator. We are obliged to add the qualifying words, because it is certain that the rate varies along the actual equator. It is necessary, therefore, either to specify some spot, or to define in some other way the force to be designated. How is this to be done?

As I remarked at the beginning of this paper, I do not propose to approach the question of the figure of the earth more nearly than is necessary. But it is perfectly clear that as soon as it is admitted that the form which we are to study by means of a pendulum actuated by gravity is to be expressed as a more or less complicated mathematical equation, the force of gravity enters as a principal variable. The limits and law of variation it is not necessary here to attempt to define. All that is necessary is to perceive that there will be one or more maxima at or near the equator, one or more minima at or near the pole, and as many means as we choose to invent functions expressing what may be called means.

The idea of a seconds pendulum as a definite length rests on the idea of gravity as a definite force. The idea of an equatorial seconds pendulum as a determinable length rests on the idea of equatorial gravity as a determinable force. We are therefore driven to consider in what sense, and by what means, it was, and perhaps is, supposed determinable.

Had France been an equatorial country there need be no doubt that, not Paris, but some equatorial village, would have been chosen as the site of experiment to be repeated again and again as time went on. But as this was not the case French philosophers went some thousand miles and sojourned for years in an equatorial country, in search of this stone. And in after time, when confirmation was wanted, voyagers experimenting in foreign latitudes made a point of getting as near to the equator as possible.

Gradually the idea of determining gravity *by experiment at the equator* gave place to the idea of determining it by experiment elsewhere. The summit lost its immediate attraction in the interest of perfecting a road to it. By degrees it became apparent that there was no summit, or at any rate that the summit could only be designated by a careful study of all the approaches; and lastly, that it was in fact only an idea. Only an idea, and that an undefined one.

The history of physical research is full of instances of this kind of baffled inquiry. From a distance the goal is clear, distinct, definite, precise; we can in thought put a finger on it. We approach, and the aspect changes, foreshortened distances extend, and small things become great; forms are changed, and though we penetrate into the very midst of what we ran for, we recognise it no longer. Had we run open-eyed we should have been prepared for the transformation and have realised better the success.

So it is with equatorial gravity. What was seen at a distance *was* that very idea, which, close at hand, we cannot readily define.

Some such difficulty appears to be the explanation of the vigour with which the more concrete idea of the actual force at a definite spot was grasped; and perhaps we may recognise in the almost extravagant pretensions of the London and Paris seconds pendulums a sense of retreat from, and abandonment of, the hopeless equatorial representative.

But in falling back from the equator upon Paris and London there was no abandonment of the length of the seconds pendulum as a linear standard. This came somewhat later, when the difficulties of precise determination even at one and the same spot were more apparent. Meanwhile the local lengths were retained as provisional units.

This appears to me to be the key of the position. It was anticipated that the exact relation of gravity at Paris and at

[1] On another point also, namely that of the general association of Borda's name with the invention of the method of coincidences, I am glad to find myself not alone in demurring. Legentil distinctly says that he followed Mairan in this; and Meyer alludes (1865) to the same misconception when he says: "Die von Mairan erfundene Methode der Coincidenzen, die gewöhnlich de Borda zugeschrieben wird" (*Pogg. Ann.*, cxxv. p. 182, note). The merit due to Borda in this connection would seem to be limited to his employment of a cross mark on the clock-pendulum as an object with which the thread of the free pendulum was to be in concert—just as Kater afterwards used a white disk and an opaque slip; out of which in after years arose a somewhat complicated and very differently understood question of precision.

London to gravity at the equator would eventually be known, and meanwhile a base of connection was wanted. It is perfectly true, as I have already said, that an absolute determinination is eminently satisfactory, and (theoretically) can stand by itself; but practically they rarely did so. It is perfectly true that if the length of a pendulum is actually measured and its rate observed, an independent determination is made; but practically the determination was almost always relative. The pendulum was generally not so much measured as to its actual length, whatever that might be, as adjusted to a certain length such as (very commonly) had been previously done at Paris or London. The distinction is very clear in some cases, less so in others. But, generally speaking, the determination has as good a right to be classed among the differential ones as among the absolute.

Consider the case of Graham's pendulum as used by Campbell at Jamaica. It was purposely designed to be adjusted *to the same length*. Or, again, consider Legentil's. He was constantly testing and adjusting the length by means of a *règle en fer*, and the only kind of measurement which took place was that of examining the equality from time to time of the length of his *pite* fibre.

It appears to me to be entirely beside the mark to insist that his *règle* or gauge had been compared or measured. It was used as a gauge and not as a measuring scale.

The same applies in nearly all cases. A gauge is always found to have been used, and some constant addition or subtraction made for the calculated position of the centre of oscillation.

That which gives to all the older determinations their apparently absolute character is that the result is *expressed* in linear measure. Considering the exceedingly doubtful character of the linear element so introduced, it is practically certain that the only chance of utilising any of these is to get back to the *observed rate* if possible, and to treat them all as merely differential.

Let it not be supposed that we shall lose anything by this. As things now stand, observations which were essentially differential and often good of their kind are under the cloud of doubtful reduction, caused by the endeavour to kill two birds with one stone. Experience has shown that this is barely possible even now, with vastly better means. Common sense suggests that it was vain before.

I have hitherto been speaking of the last century. The aspect changes somewhat as we enter the present one. Scarcely a trace remains of the absolute force of gravity as a real object. The idea of a linear standard is still active, but evidently doomed. What will be left as the *motive* for absolute determination, in preference to differential? I confess that I can give no answer. Anxious as I have been, and am, to learn and to understand the whole of this subject; careful as I may be to catch at every indication of an unexpressed idea latent in the mind; it is in vain that I try to find a *raison d'être* for absolute pendulum operations at the present day. It would be impossible to say this and not imply dissent from the views of those who advocate their prosecution, and I am well aware that such views *are* advocated by a section of the Continental geodesists. But I seem to be unable otherwise to find a solution. A year has elapsed since this paper was written—all but these two sentences—and I have learnt nothing to change my opinion. J. HERSCHEL

NOTE ON SOME EFFECTS PRODUCED BY THE IMMERSION OF STEEL AND IRON WIRES IN ACIDULATED WATER [1]

DURING a discussion upon a very interesting paper by our president, "On the Durability of some Iron Wire," I mentioned a fact which I had lately observed, viz., that steel or iron wires immersed for a few minutes in acidulated water containing one tenth sulphuric acid became excessively brittle. Our president has since kindly asked me to make a few more experiments on this subject, and to embody them in the form of the present note.

Upon repetition of these experiments I have found that this brittleness is no mere accidental result, due to some flaw in the steel or iron wires, but that the resulting brittleness is invariable in all kinds of steel as well as iron. Nor is the effect due to any specific proportions of sulphuric acid to the water; nor, in fact, as we shall see later, to any particular acid. The effects, however, seem confined to steel and iron; as by similar treatment I have as yet obtained no perceptible effect on copper or brass. At first I was inclined to believe that the effects were due primarily to a change in the molecular structure; but a more extended series of experiments has led me to adopt entirely the view taken by my friend Mr. W. Chandler Roberts, who predicted that the effects were most probably due to the absorption of hydrogen.

I have tested these wires in my induction balance, but can find no change whatever in its magnetic conductivity, nor any change which would be the equivalent of those produced by heat, strain, torsion, or tempering; but there are very evident results produced: if the conditions of the experiments are such as to favour the absorption of hydrogen. For instance, if we reduce the proportion of sulphuric acid to one-twentieth, we find that it requires some thirty minutes' immersion to produce the full effect, a few minutes' immersion producing no perceptible result. If now we place an amalgamated zinc plate in the same liquid, and join the two extremities, we have an ordinary battery, where hydrogen is given off on the steel wire. Now as the hydrogen produced by the decomposition of the water is much more rapid than before, we find that a few minutes' immersion produces a far more brittle wire than could be obtained by hours of simple immersion, and we have the result free from any doubt as to its being a mere surface action; for if we immerse the wire alone, surface corrosion rapidly takes place, but by simply connecting it with the zinc the steel is perfectly protected, retaining its original bright surface, for any time, as long as it is so protected.

It is not absolutely necessary that we should join the zinc in the same cell, for if we pass a current from a few cells of an external battery through two steel wires as electrodes in sulphuric acid and water we find that both wires have become brittle, though in a very different degree, the wire connected with the zinc or negative pole remaining bright, although excessively brittle, whilst the one connected with the positive pole is much corroded, and but feebly brittle, with this arrangement. I find that sulphuric acid is no longer required, but that all acids, neutral salts, and ordinary water produce an active effect, the time required being simply as the conductivity of the liquids employed. When water or most neutral salts are used, we find the negative pole quite bright, but brittle, the positive pole much corroded, but not at all changed as regards its flexibility.

I believe that these effects are due to the absorption of hydrogen when the hydrogen is in the "nascent" state, for I have obtained no results by continued immersion in carburetted hydrogen gas (ordinary lighting gas), but when plunged into a medium containing the hydrogen just freed from its combination, its effects are most remarkable: for if we immerse a wire into sulphuric acid and water, say one-twentieth, the effects are slow, requiring at least thirty minutes; but if we let fall into this water some scraps of zinc hydrogen is rapidly given out, and by now immersing the steel wire in this gaseous liquid, taking care not to touch the zinc, we find that the steel becomes rapidly brittle, whilst its surface is free from corrosion, due no doubt to the protecting surface of surrounding hydrogen.

Hydrogen seems to permeate through the entire mass, for iron rods a quarter of an inch thick were equally affected, requiring more time, or in other words, a supply of nascent hydrogen sufficient for the larger mass; and once the wire has become hydrogenised (if we may be allowed the expression), it retains it under all circumstances of time and change of surrounding atmosphere: heat alone, of all the means I have tried, has any effect; and if we heat a wire to cherry red in a spirit lamp we find that it is completely restored to its primitive flexibility in a few seconds. This same wire, however, on being immersed in the accidulated water, rapidly becomes again brittle; we may thus at will render the same wire flexible by previously heating it, or render it exceedingly brittle by favouring its absorption of hydrogen.

I have remarked that a wire immersed in sulphuric acid and water of any proportion, say one-sixteenth, becomes more electronegative than at the first instant of plunging. If we take amalgamated zinc as the positive element, and a steel or iron rod or wire for negative, we find that there is such a remarkable similarity of electromotive force between all kinds of steel and iron that we are forced to the conclusion that we are simply testing the electro-negative qualities of hydrogenised iron; the force being with amalgamated zinc ·56.

I noted here a remarkable fact, and which does not agree with the results of many authorities. I found that as soon as the

[1] Read before the Society of Telegraph Engineers, April 14, by Prof. D. E. Hughes.