Jordantype, otherwise called 'electrotype : its early history, being a vindication of the claims of C.J. Jordan as the inventor of electrometallurgy / by Henry Dircks.

JORDANTYPE,

OTHERWISE CALLED 'ELECTROTYPE:'

ITS EARLY HISTORY,

BEING

A VINDICATION OF THE CLAIMS

OF

C. J. JORDAN,

AS THE INVENTOR OF ELECTRO-METALLURGY.

BY HENRY DIRCKS, C.E.

PATENT AGENT.

LIFE MEMBER OF THE BRITISH ASSOCIATION, CORRESPONDING MEMBER OF THE LIVERPOOL POLYTECHNIC SOCIETY, ETC.

London:

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With the Authors comps

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PREFACE.

Although the present discussion may not be one of general public interest, still it concerns science to be correctly informed of the origin and progress of its several applications to the arts. It is well when such inquiries depend on easily ascertained facts, under investigation at a remote period. But should disputes arise at the very birth of a new art, it is certainly desirable to put on record, as early as possible, such data as may enable posterity, at least, to settle such disputed claims. With this last view, I re-publish the present Papers, with some additional remarks,—affording altogether a candid and impartial statement of the subject of Electro-metallurgic History, while all the parties are still living, and all the facts prominently before the republic of letters and science.

In adopting the title of *Jordantype*, I have been guided by the claim to the invention now resting on unequivocal documentary evidence, repeatedly promised in 1844, but very unworthily witheld until Dec. 1851,—a circumstance alone sufficient to frustrate any earlier decided course in bestowing this well-merited crowning honour.

London, Feb. 1852. H. D.

CHAP. I.

PRELIMINARY STATEMENT OF THE CASE.

My paper recently published in the *Expositor* of Jan. 24, 1852, affords those preliminary observations requisite to form a correct judgment of the matter under consideration. I therefore insert it here.

MR. C. J. JORDAN

THE INVENTOR OF THE ELECTROTYPE.

Sir,—Until the commencement of the year 1844, I had been, in common with others, of opinion that the invention of the electrotype process originated with Mr. T. Spencer, a carver and gilder, of Liverpool. It is now pretty generally known, particularly among chemists and electricians, that I am the author of a series of contributions towards a history of this interesting, and also very important, subject, commenced in the 'Mechanics' Magazine,' February 3, 1844. I there prove, by irrefragable evidence, that this beautiful and extensively useful branch of art was invented by Mr. C. J. Jordan, of London, and the decision therein pronounced has been recognized and adopted by Dr. Ure, in his 'Dictionary of Arts,' Mr. Shaw, and other scientific writers on Electro-metallurgy, as well as by the Editors of numerous journals and publications.

Mr. Spencer, after a silence of nearly 8 years, has resuscitated his original claim, at a recent public dinner in Liverpool, reported in the 'Liverpool Mercury,' Dec. 23, 1851. He there endeavours to establish a claim dating from the 9th of May, 1839, a fortnight before Mr. Jordan. This simplifies the whole matter in dispute, as to the priority of invention, between the respective candidates, and may be thus stated:

MR. SPENCER'S CASE.

He addressed the following letter to the then secretary of the Liverpool Polytechnic Society:

(Copy). May 9, 1839.

"Dear Sir,—I wish it to be recorded upon the books of the society that for the last nine months I have been making occasional experiments on a method of executing any species of metallic device to any given pattern; and by the same agency have also been making expe-

riments on a method of engraving in relief, both of which have been comparatively successful, and will, in my opinion, ultimately prove useful in the arts.

"I shall take an early opportunity of laying my process before the society, with specimens produced by it I should not have brought this subject before the society thus early but for the following passage, that appeared in last Saturday's Athenæum,—'Prof. Jacobi, at St. Petersburgh, has made a discovery which promises to be of importance to the arts: he has found a method (if we understand our informant rightly) of converting any line, however fine, into relief by a galvanic process, the emperor of Russia having placed funds at his disposal, to enable him to perfect his discovery.'

"That I have been engaged in this process the time above stated is known to several gentlemen, who are now members of this society.

"I am yours respectfully,

"Rev. T. Dwyer." "T. Spencer.

Nothing further appears on the journals of the society before the annexed entry, viz:

"Thursday, Sept. 12, 1839.

"Mr. Spencer then proceeded to read a paper giving an account of some experiments made to ascertain how far voltaic electricity might be usefully applied to the purposes of working in metal.

"Resolved, that Mr. Spencer's paper be printed at the expense of the society, under the superintendence of the council of the society."

MR. JORDAN'S CASE.

On the 22nd of May, 1839, Mr. C. J. Jordan addressed a Letter on "Engraving by Galvanism," to the 'Mechanics' Magazine,' which appeared in the number of that periodical for June 8, 1839.

In that letter Mr. Jordan describes the electrotypic apparatus,—notices the usual deposits obtained by means of the earlier kinds of galvanic batteries,*—also by the use of Daniell's sustaining battery, and Dr. Bird's modification of it (constituting, indeed, the single-cell process)—states on what depends the tenacity of electrotype deposit,—specifies type-matrices, coins, dies, engravings, tubes, vessels, and other products,—and, in short, fully, clearly, succinctly developes the entire electro-metallurgic process.

[This Letter will be found in the next chapter, p. 7.]

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* It may be mentioned here, that this mode—known as the 'battery process,'—has been found to be, after all, the most economical, and, in manufacturing operations, is that almost universally resorted to.

REMARKS.

It will be observed that Mr. Spencer's letter of May 9 is so loosely worded as to be unintelligible, until we read the very last portion. During the discussion of 1844, this letter was never produced, has never yet been made public by Mr. Spencer, and is now published by myself for the first time. It will be acknowledged by those who read it, to be just such an undescriptive notice as any one—even ignorant of the subject itself—could have written; and yet, upon this precious 'document' he now attempts to establish a claim of priority in publication. It is no publication whatever, there being nothing in the shape of information about it. Mr. Spencer keeps his secret from May 9 to Sept. 12, 1839,—during which time it might have been lost to the public (as far as he is concerned) by his death or other casualty. And it is remarkable that in the course of his speech at the beforenamed dinner, we find him admitting, in reference to his subsequent pamphlet, "it is to this that the public are indebted for their FIRST practical acquaintance with the electrotype,"—ignoring, of course, the fact, that the public were made acquainted with the same, by Mr. Jordan, three months before!

In drawing up a parallel of Mr. Jordan's letter and Mr. Spencer's pamphlet (see page 16), I brought to light an extraordinary instance of literary and scientific larceny, showing the entire pamphlet to be nothing more than Mr. Jordan's Letter extended, elaborated, amplified, without an additional fact, and omitting one ingenious application through the typographical error of printing the word 'tubs' for 'tubes,'—which latter appears to have baffled his research, notwithstanding the somewhat broad hint conveyed in the word 'wire.'

To give the title to priority of invention, there must be a first publishing of some kind,—a sealed descriptive packet, open sale, printing, or any means that places the fact in the public possession. Such was *not* Mr. Spencer's case; and, however desirable for himself, he cannot disprove the fact, that Mr. C. J. Jordan is the true originator of electro-metallurgy.

I am, your obedient servant,

H. DIRCKS.

CHAP. II.

EXAMINATION OF THE MERITS OF MR. JORDAN'S CASE.

THE following Paper, addressed to the *Mechanics' Magazine* of Feb. 3, 1844, was written after going through 39 volumes of that work, — when, while reading Mr. Jordan's communication, of May 22, 1839, a *second* time, I called to mind my having told Mr. Spencer about it, soon after the appearance of his pamphlet, and being assured by him that it did not affect him! I immediately thought I detected a strong similarity, of which a reperusal of his pamphlet convinced me; and thereupon I wrote my opinion, particularly as I found that those interested in the journal, as well as all engaged in the electrotype, and even Mr. Jordan himself, appeared either indifferent about the true state of the case, or strangely inactive in a subject of such growing interest.

CONTRIBUTIONS
TOWARDS
A HISTORY OF ELECTRO-METALLURGY.

IN looking over the 'Mechanics' Magazine' for several years past, my attention was rather particularly attracted to vol. xxxvi. for 1842, in which appears a paper, entitled 'Books on Electro-Metallurgy,"—a review on the works of Mr. G. Shaw and Mr. A. Smee on that subject. The writer says, "On a former occasion (vol. xxxiii. p. 20) we stated our reasons for assigning to Mr. T. Spencer, of Liverpool, in preference to every other claimant, the merit of this exceedingly valuable addition to our manufacturing processes; and nothing has since transpired to induce us to modify in the slightest degree the opinion that we then expressed on this head." It is added, "we are sorry to see that there still exists in certain scientific circles the same dogged reluctance, on which we

before animadverted, to do justice to the humble 'carver and gilder of Liverpool,'—for no better reason that we can discover but the sin of being humble." And further on, says, "Be it so, since the fact is so, that it was by 'ACCIDENT' Mr. Spencer made the discovery, still it was an 'accident' of that happy sort which happens to but one or two men in an age, and which, by the universal consent of mankind, entitles him to whose lot it falls, to be looked upon with all the respect, honour, and gratitude due to the chosen instrument of any great revelation by Nature to her children." How far Mr. Spencer has been indebted to *accident* the sequel will show.

The earliest, but private, application of galvanic action to ornamental purpose, that I am acquainted with, was practised by Mr. Henry Bessemer, of Baxter-house, Camden Town, who, above ten years ago, employed the galvanic apparatus to deposit a coating of copper on small lead castings of antique heads in relief.

Mr. Warren de la Rue also notices, in the Philosophical Magazine for 1836, the superficial correspondence of reduced copper with the plate on which it is thrown down.

The earliest published account of the manipulation requisite for obtaining casts by galvanic action is contained in the letter of a Mr. C. J. Jordan, dated 22nd of May, 1839, and published in the 'Mechanics' Magazine' for June 8, 1839. This letter is so interesting in connexion with the history of electro-metallurgy, that I repeat it here: in particular, I would direct attention to the fact of the main incidents named by Mr. Jordan, published in June 8, 1839, agreeing* with those published by Mr. Spencer, September 12, 1839; and, curious enough, being called forth by the same vague announcement of Prof. Jacobi's experiments, which was then making the round of our periodicals. Both parties describe Dr. Golding Bird's small galvanic apparatus. The letter is as follows:—

ENGRAVING BY GALVANISM.

Sir, — Observing in the last page of a recent number of your Magazine a notice extracted from the *Athenæum* relative to a discovery of Professor Jacobi, its perusal occasioned the recollection of some experiments performed about the commencement of last summer, with the view of obtaining impressions from engraved copper plates, by the aid of galvanism, which led me to infer some analogy in principle with

* The very questionable character of this resemblance will be observed on referring to page 16.

those of the Russian Professor, and may probably give me the right to claim priority in its discovery and application. These experiments were abandoned from the want of that most important element in pursuits of this nature,—time, the writer's share of the said element being occupied in a manner more imperative than pleasing. I regret, however, not having made it the subject of an earlier communication, as this would have placed my pretensions beyond doubt; but, inasmuch as the notice alluded to is given from memory, and is undescriptive, while I may be enabled to exhibit the *modus operandi*, my assertion may be at least partially substantiated.

It is well known to experimentalists on the chemical action of voltaic electricity, that solutions of several metallic salts are decomposed by its agency, and the metal procured in a free state. Such results are very conspicuous with copper salts, which metal may be obtained from its sulphate (blue vitriol) by simply immersing the poles of a galvanic battery in its solution, the positive wire becoming gradually coated with copper. This phenomenon of metallic reduction is an essential feature in the action of sustaining batteries, the effect, in this case, taking place on more extended surfaces. But the form of voltaic apparatus which exhibits this result in the most interesting manner, and relates more immediately to the subject of the present communication, may be thus described: It consists of a glass tube, closed at one extremity with a plug of plaster of Paris, and nearly filled with a solution of sulphate of copper;

this tube and its contents are immersed in a solution of common salt. A plate of copper is placed in the first solution, and is connected, by means of a wire and solder, with a zinc plate which dips into the latter. A slow electric action is thus established through the pores of the plaster, which it is not necessary to mention here; the result of which is the precipitation of minutely crystallized copper on the plate of that metal, in a state of greater or less malleability according to the slowness or rapidity with which it is deposited.

In some experiments of this nature, on removing the copper thus formed, I remarked that the surface in contact with the plate equalled the latter in smoothness and polish, and mentioned this fact to some individuals of my acquaintance. It occurred to me, therefore, that if the surface of the plate were engraved, an impression might be obtained. This was found to be the case; for on detaching the precipitated metal, the most delicate and superficial markings, from the fine particles of powder used in polishing, to the deeper touches of a needle or graver, exhibited their correspondent impressions in relief with great fidelity. It is therefore evident that this principle will admit of improvement, and that casts and moulds may be obtained from any form of copper.

This rendered it probable that impressions might be obtained from those other metals having an electro-negative relation to the zinc plate of the battery. With this view, a common printing type was substituted for the copper plate, and treated in the same manner. This

also was successful: the reduced copper coated that portion of the type immersed in the solution. This, when removed, was found to be a perfect matrix, and might be employed for the purposes of casting.

It appears, therefore, that this discovery may be turned to some practical account. It may be taken advantage of in procuring casts from various metals, as above alluded to. For instance, a copper die may be formed from a cast of a coin in silver, type-metal, or lead, &c., which may be employed in striking impressions in soft metals. Casts may probably be obtained from a plaster surface surrounding a plate of copper; tubes [not 'tubs'], or any small vessel, may also be made by precipitating the metal around a wire, or any kind of surface, to form the interior, which may be removed mechanically, by the aid of an acid solvent, or by heat.

C. J. JORDAN.

May 22, 1839.

I was aware of Mr. Jordan's letter at the time of its publication, and have frequently been surprised since that his name has not transpired in any discussion I have read on the subject. Nothing certainly can be clearer than his reasoning, the details of his experiments, and his several concluding suggestions.

It was particularly in September and October, 1837, that several parties attached to scientific pursuits, in Liverpool, were engaged in repeating the experiments of Dr. Golding Bird, published in the Phil. Trans. for 1837, and of which he gave an account before the Chemical Section of the British Association, at its Meeting in Liverpool. The apparatus used on these occasions by myself and others, was that employed by Dr. Bird.

I was at that time a good deal in communication with Mr. John Dancer, philosophical instrument maker, of Liverpool, now of the firm of Messrs. Abraham and Dancer, of Manchester, and these experiments were a frequent subject of conversation. It was about October, of the year following (1838) that Dr. Brett was giving lectures upon electricity and galvanism at the Medical Institution, Liverpool, and being in communication with Mr. Dancer respecting the apparatus for those lectures, I was arranging for a supply

of galvanic troughs, when Mr. Dancer showed me a ribbon of copper, thin but very firm, granular on one side, while it was bright and smooth, all but some raised lines, on the other. Seeing my curiosity a good deal excited, he said at once, it was the result of galvanic action, and explained the whole process. He particularly noticed that generally the deposit was more crystalline, granular, and brittle. The difference of the specimen I examined, which possessed all the tenacity of rolled copper, he attributed to his having gone to the potteries to look for jars suitable for sustaining batteries, and having fixed on a lot which he was told would not answer, as they were not glazed. The idea occurred to him that such unglazed jars might be substituted for brown paper, bladder, plaster of Paris, and other porous substances, previously employed. He therefore obtained a sample for experiment, and the result was a more compact deposit of copper, — though that was not what he was seeking, his object being durability and equable action. Mr. Dancer was not negligent of this accidental discovery, and made no secret of it.

These facts were named by Mr. Dancer's friends to various persons, and in my capacity of Honorary Secretary, at the time, of a Literary and Scientific Institution, the experiments were a frequent subject of conversation.

Subsequently, Mr. Spencer laid claim to the discovery of the means for obtaining metallic casts by galvanic agency; and having, with others, expressed doubts of Mr. Spencer's claim to priority, I was surprised when verbally informed by Mr. Spencer that I was quite in error in repeating such statements, and that, so far from its being the fact, Mr Dancer had "made the amende honorable!" This statement, which appeared to me a very extraordinary one, induced me to write a note of inquiry to Mr. Dancer, and the following are extracted from his reply. Speaking of the experiment already mentioned, he says,—

"I met Mr. Spencer one morning in Berry Street, and I happened to have one of these precipitated copper plates with me, which I showed to him. When I told him how it had been formed, he would scarcely believe it, until I pointed out the impressions in relief of all the minute scratches that were on the plate against which it had been deposited. The surprise that Mr. Spencer expressed very naturally led me to suppose that it was the *first* compact piece of precipitated copper he had seen... ...

"All this happened many months before I was aware that Mr. Spencer had been engaged in anything of the kind, excepting that he had Dr. Bird's experiment in action. Some time after this, Mr. Spencer applied to me for one of my porous jars, and one evening at his house he told me the purpose for which he wanted it,—he stated that he intended to form ornaments by gilding patterns of plaster, and depositing copper upon them; he stated also that it would be of great use in his business. The same evening, I told him of my having obtained impressions by stamping my name on a copper cylinder.

"You will recollect that when Mr. Spencer brought the subject before the Polytechnic Society, that you and others also, who knew I had been experimenting in the precipitation of copper, expressed surprise that I did not make mention of it. The reason I gave you at the time was, that being acquainted with Mr. Spencer, I thought it would look like envy, or a wish to detract from the merits of his experiments, and share the honour without having brought it to any practical use: but, as he has stated to you and others that I made the 'amende honorable,' which insinuates that, in the letter I sent to him, I had been retracting, or making some apology, you will be able to judge, from this statement of undeniable facts, whether I have been misrepresented."

Mr. Thomas Spencer read his paper 'on Voltaic Electricity applied to the purpose of working in metal,' before the Liverpool Polytechnic Society, on the 12th September, 1839. He purposed making this matter the subject of a paper to have been read at the Meeting of the British Association, in August, at Birmingham. He had announced to the Polytechnic Society, on the 9th of May, (see p. 3) that he had a communication of this nature to make, and he did so then, and in that manner, in consequence of a paragraph in the public prints announcing that Professor Jacobi was engaged on experiments in engraving by galvanic action. The same year the Society printed Mr. Spencer's paper for gratuitous circulation. In this paper he claims to have been engaged on the subject of it for two years, dating, in short, from September, 1837,—the very time when Dr. Bird read his paper at the Liverpool Meeting of the British Assoc. He says, without the least acknowledgment of the identity of his apparatus with that of Dr. Bird, "In September, 1837, I was induced to try some experiments in electro-chemistry with a single pair of plates, consisting of a small piece of zinc and an equal sized piece of copper." It is unfortunate for the reputation of Mr. Spencer, that while the Society was at the expense of publishing his paper, to uphold his claims, he should make that very publication the vehicle for underrating and undervaluing not only the labours of others, but actually the labours of experimentalists to whom he was indebted for the germ of his own experiments in electro-metallurgy. The 'humble carver and gilder of Liverpool' designates the learned lecturer of Guy's Hospital "a clever young demonstrator (Dr. Bird, of London)," and speaking of Dr. Bird's experiments before alluded to, this same 'humble' individual very complacently observes, in reference to obtaining crystals of copper within the diaphragm of the apparatus, "I doubted this at the time, as it was opposed to all former experience." Not a word that Dr. Faraday had spoken at considerable length on this very point—not denouncing it as a decided error, but suggesting, with that delicacy of remark and philosophical acuteness of observation for which he is remarkable, various reasons for further enquiry and consideration. Mr. Spencer at one dash takes to himself the merit of singular shrewdness of penetration in this matter. Mr. Dancer had produced an indent, in

the shape of a D, on the deposited copper, where a letter, cut out of paper, had been attached by varnish to the the negative plate. Mr. Spencer details, as a discovery of his own, indentions on the deposited metal, where sealing-wax had dropped on the plate.

It shows a singular want of observation and ingenuity on the part of Mr. Spencer, a carver, and claiming acquaintance with friends "some of whom are connected with the public press," that he should not have seen, or did not try to obtain, a wood engraving before attempting the rude and clumsy production of a copper-plate for printing, with lines in relief "one-eighth of an inch in thickness!" when a mere hair line would have sufficed.

Mr. Spencer's paper is divided into two parts. The first refers to executing engravings, the second to forming moulds of medals, &c. and are said to have been all made simultaneously, that is, between Sept. 1837 and May, 1839. Whether any improvements between May and Sept. 1839 are introduced, is not stated, neither do any letters nor affidavits accompany the communication. Specimens were exhibited to the Society, Sept. 12, 1839, of alleged earlier date, but of a very brittle and friable character, and utterly useless for any purpose in the arts. And here it was that Mr. Spencer 'annexed' a most valuable hint from Mr. Dancer, whose results were not only freely communicated to him, but his very apparatus obtained from him by Mr. Spencer,—yet without the slightest acknowledgment, then or since, of his having acted otherwise than by his own unaided judgment. Yet we see that the first impulse was given at the Liverpool Meeting of the British Association, by Dr. Bird's voltaic apparatus,—that the scientific journals were rife with discussion on applications of galvanism to the arts,—that Mr. Jordan, a correspondent to the 'Mechanics' Magazine,' in the plainest and most intelligible manner, explains what Mr. Spencer subequently has a little—and but very little—elaborated. Lastly, therefore, that through the Mechanics' Magazine, (which Mr. Spencer was regularly taking in) — the experimental results of Mr. Dancer, and the reports, in April and May, 1839, in the public journals, of Jacobi's experiments,—all bring such broad hints and abundant assistance to aid Mr. Spencer, that he is rather to be looked upon as adopting what was already known than as a 'discoverer,'—much less the "father of electro-metallurgy!" having "a preference to every other claimant." He commences his paper by saying, "I do not profess to have brought forward a perfect invention:" he should rather have said, "I do not profess to have brought forward a perfect improvement of what has been already discovered and made known by others."

It is a remarkable fact that Mr. Spencer has made no useful or profitable application of electro-metallurgy; nor does it say much for his originality of genius and scientific acumen (which a perusal of his tract on voltaic electricity would persuade us he considers belongs to him) so long to have remained an absolute cypher in an art entirely new, and capable of modifications yet untried.

The facts now set forth have slumbered nearly seven years, and

are now given lest they should be entirely lost. I have all possible respect for what Mr. Spencer HAS done; but common candour obliges me to own that I consider that he has only followed in the footsteps of others, repeating their experiments with very little improvement,—but certainly neither at the first originating a new art, nor afterwards doing much to add to its resources or promote its progress.

London, Jan. 1844.

The justice of the statements made in this communication, in reference to the prior claims of Mr. Jordan, was immediately admitted by the Editor of the 'Mechanics' Magazine,' who appended thereto the following remarks:

"Mr. Dircks has proved beyond all doubt that we have made a great mistake in advocating so strenuously the claims of Mr. Spencer to the invention of electrography. No one, however, can suppose that we would intentionally exalt any one at the expense of our own journal, which we are now pleased to find was the honored medium of the first distinct revelation of this important art to the public, by an old and esteemed correspondent of ours, Mr. Jordan. Whatever Mr. Bessemer, Mr. Dancer, or Mr. Spencer, or others, may have previously said or done, it was in private—made no secret of, perhaps, but still not communicated to the public at large —not recorded in any printed work for general benefit. For anything previously done by any of them, the art might have still remained in the profoundest obscurity. No published description of an earlier date than Mr. Jordan's can, we believe, be produced; and when we look upon that description, it is really surprising to see with what fulness and precision the writer predicated of the art, nearly all that has been since accomplished. In supporting, as we did, the claims of Mr. Spencer to be considered as the first discoverer, we had lost all recollection of Mr. Jordan's communication. We had no personal acquaintance with either of the gentlemen, and could have no motive for favouring one more than the other. If it should seem to the reader surprising that Mr. Jordan's paper escaped the recollection of the Editor, his surprise will not be lessened when he observes how it has also escaped the notice of every one else down to the present time. To us the most surprising thing of all is that neither Mr. Jordan, nor his friends, should before now have thought it worth while to vindicate his claims to the promulgation of an art which justly entitles him to take a high place among the benefactors of his age and country."

CHAP. III.

PARALLEL OF MR. JORDAN'S AND MR. SPENCER'S PUBLICATIONS.

On reading the reports in London of the dinner given to Mr. Spencer, in Liverpool, I threw the substance of my strictures on his pamphlet in the Mech. Mag. of Feb. 17, 1839, into a parallel with Mr. Jordan's Letter, to show — 1, that if he had never read Mr. Jordan's Letter, he had written a pamphlet strikingly like it. 2, that, so far from containing "nothing practical," it contains all and something more (and *practical* too) than was *collected* in his pamphlet. And — 3, that the only inferences to be drawn were, that he had read it, well studied it, and endeavoured by *verbal* amplification to make all its details his own. This may not prove that he was not an independent inventor, but it does go far to prove that his invention was *not so early* complete as Mr. Jordan's, and that Mr. Jordan's communication was used by Mr. Spencer as the text-book for his pamphlet. This parallel of the two publications appeared in the *Liverpool Courier*, Dec. 31, 1851.

THE ORIGIN OF ELECTRO-METALLURGY.

Sir,—I have read in the 'Morning Advertiser,' of the 24th instant an account of a dinner given to Mr. Thomas Spencer, in Liverpool, representing him as the originator of the electrotype process; and he is reported to have made a speech animadverting upon certain statements made in the 'Mechanics' Magazine' (1844) as tending to deprive him of that honour. Claims to original invention, whether in literature or science, can only be maintained and settled by a true and plain statement of facts; and as I was the first to establish the priority of Mr. C. J. Jordan's claim, I beg briefly to recapitulate those facts, to show how unjustifiable it was in Mr. Spencer to state

that nothing 'practical' had been published before his pamphlet. I shall now give a parallel of the two:

Engraving by Galvanism. By C. J. Jordan. 'Mechanics' Magazine,' June 8, 1839.	Voltaic Electricity applied to working in metal. By T. Spencer. Read Sept. 12, 1839.
Observing ..... a notice, extracted from the 'Athenæum,' relative to a discovery of Professor Jacobi, its perusal occasioned the recollection of some experiments performed about the commencement of last summer—	About four months ago a paragraph appeared in the 'Athenæum,' stating that Professor Jacobi—7. It is two years since I began to experimentalise on this subject—7 In September, 1837, I was induced to try some experiments in electro-chymistry—12.
—with the view of obtaining impressions from engraved copper plates, by the aid of galvanism, which led me to infer some analogy in principle with those of the Russian Professor.	I accordingly concluded that he was engaged in experiments analogous to my own—7.
—This phenomenon of metallic reduction is an essential feature in the action of sustaining batteries.	I had long been aware of what every one knows who uses a sustaining galvanic battery, that the copper plates acquire a coating of copper—13.
But the form of voltaic apparatus which exhibits this result in the most interesting manner—it consists of a glass tube—a plate of copper is placed in the solution, and is connected with a zinc plate—the result is precipitation of minutely crystallized copper, on the plate of that metal, in a state of greater or less malleabilty according to the slowness or rapidity with which it is deposited.	A single pair of plates—a small piece of zinc, and an equal-sized piece of copper—12. I discovered that the solidity of the metallic deposition depended entirely on the weakness or intensity of the electro-chymical action—15.
I remarked that the surface in contact with the plate equalled the latter in smoothness and polish, and mentioned this fact to some individuals of my acquaintance.	I then made mention of it to a few friends—7.
—on detaching the precipitated	On examining with a magnify-

Mr. Jordan.	Mr. Spencer.
metal. the most delicate and superficial markings..... exhibited their correspondent impressions.	ing glass, I found every line was as perfect as the coin from which it was taken—19.
It is evident that casts and moulds may be obtained from any form of copper.	I was desirous of executing metallic ornaments, in either cameo or intaglio, First experiment, a very prominent medal—19.
This rendered it probable that impressions might be obtained from other metals having an electro-negative relation to the zinc of the battery.	Nor (I need hardly observe) is its application confined to copper only—8.
—a common printing type was substituted — the reduced copper —when removed, was found to be a perfect matrix.	—in the manufacture of printing types—the matrix or mould—advantages appear to present themselves—9.
It appears, therefore, that this discovery may be turned to some practical account.	I feel convinced that it exhibits many promising indications of utility—9.
—a copper die may be formed from a cast of a coin or medal in silver, type-metal, or lead, &c.	—a means of procuring a cast or a die in hard metal—8 The applicability of this process in procuring facsimiles of coins or medals—9.
—which may be employed for striking impressions in soft metals.	I took a silver coin, and put it between two pieces of clean sheet lead, and placed them under a screw press. From the softness of the lead, I had a sharp mould–20.
Casts may probably be obtained from a plaster surface surrounding a plate of copper;	—to deposit a metallic surface on a model of clay or other non-metallic body—21.
—tubs or any small vessels may also be made by precipitating the metal around a wire, or any kind of surface, to form the interior—	[For 'tubs' read 'tubes,'—a typographical error in the original letter. Mr. Spencer did not attempt *tubs*. He considered it impracticable to make copper 'vessels,'—ships!]
which may be removed mechanically, by the aid of an acid solvent, or by heat.	—easily separate ... by the application of heat. — When the heat of a spirit lamp was applied—the impression fell easily off—20.

I leave it to the impartial reader to say whether he considers these numerous coincidences accidental, or as the result of an intimate ac-

quaintance with Mr. Jordan's Letter, animated by a petty desire to say something more and better than he had done.

I informed Mr. Spencer of Mr. Jordan's discovery very shortly after its appearance, but he spoke of it as of no account. A reference to my letters and those of Mr. Spencer, which appeared in the 'Mechanics' Magazine,' of 1844, commencing on the 3rd of February (see chapter II.) and continuing to April, will best exhibit to my readers the details of the very extraordinary claim he has set up, and which for some time was maintained without opposition,—except in the journal in question,—where, notwithstanding all his letters, after much empty profession of what he could do, he has left Mr. Jordan, so far as published evidence goes, an indisputable right to the eulogium passed upon him by the celebrated Dr. Ure, in his 'Dictionary of Arts, &c.' immediately after the publication of my papers on the history of electro-metallurgy, when he says:—

"To this gentleman, therefore, the world is indebted for the first discovery of this new and important application of science to the uses of life."

Whatever may be due to Mr. Spencer, he must not snatch those laurels from Mr. Jordan, to which he has no just or equitable claim, —on the authority of established custom, the pre-eminence of printed published evidence over all oral or other communications in matters establishing the right of priority of invention.*

The papers we have extracted from the Mechanics' Magazine contain all that is requisite to form a decision on the subject in dispute; although subsequent to them a correspondence was maintained by the parties concerned, Mr. Spencer did everything but reply to the real question at issue, endeavouring to evade it by sneering personalities and vague allusions to the possession of documentary evidence, which now turns out to be nothing more than the vacuous notice given in our first page, which he himself has cautiously abstained from publishing in his own behalf.

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* Mr. Spencer has been the victim of several most vexatious coincidences: in 1844, I pointed out five distinct cases of what, in his late speech, he terms "duplicate discovery," one of which is recorded in the report of the Liverpool Polytech. Soc. 1838-40, and refers to his pretensions to the discovery of the autogenous soldering process, long after the French and two years after the English patent of M. de Richemont. (See Mech. Mag. 1840.)

We have not yet heard of any similar claims by Mr. Spencer upon the science of 'electro'-biology, into which his versatile genius has carried him, and which he has enriched with at least one very significant though somewhat inelegant experiment upon a 'clairvoyante,' alluded to in Mech. Mag. March, 1844, where he says,—"I put my thumb to the tip of the nose, and extended the fingers outwards, in the most approved manner, and as we frequently see enacted by the clown in the ring."

CHAP. IV.

STRICTURES ON MR. SPENCER'S DINNER SPEECH AT LIVERPOOL.

"Rude am I in speech."

Mr. Spencer, when writing to the 'Mechanics Magazine,' in Feb. 1844, made great profession of possessing evidence which he could adduce in support of his disputed claim; but, though promised, it was never produced. This blank naturally left it doubtful whether there might not be some fact in his favour which had been overlooked, or even interestedly suppressed. Mr. Spencer, however, has made the occasion of a recent "public dinner," given to him by his private friends in Liverpool (see the 'Liverpool Mercury,' Dec. 23, 1851) the medium of conveying to the public, in a long and carefully written speech, read on that occasion, his own version of that so-called documentary and other evidence which he has thus pertinaciously witheld for nearly eight years. For this extraordinary proceeding, with so many chances of time causing those losses in evidence, which fortunately have not occurred, even Mr. Spencer's own apology can scarcely be taken as a sufficient excuse for breaking his promise recorded, as above stated, in 1844, thus laying himself open to the suspicion of wishing to strengthen his case through defective evidence against him.

However, we shall wade through his desultory oration for those "facts and dates" we are there promised. But first for his apology, when he says,—"Let me not be accused of egotism in thus distinctly stating the particulars and dates of this discovery. I have been so apprehensive of such a construction, that I have hitherto forborne to give public denials to those statements to which I have referred until this evening." Egotism! Is it any egotism to defend just claims? Is it any egotism to fulfil promises written, printed, and published? Surely it is much more egotistical to promulgate hollow pretensions. Mr. Spencer does not appear to have been animated by any such delicately scrupulous aversion to egotism on many other occasions.

The statements which he here alludes to are charges affecting his claim, occurring, first at Glasgow; secondly, at Liverpool, in 1841; and thirdly, in the 'Mechanics' Magazine,' 1844,—this last being, in his own words, "to the effect that I had copied my experiments from a Letter written by a Mr. Jordan, and published, I think, in the June number of that print for 1839. Nothing could be more absurd than this last charge; in fact, I had never seen the letter until it was thus pointed out; and, if I had, I COULD NOT HAVE OBTAINED ANYTHING PRACTICAL FROM IT. It referred to the experiments of Prof. Jacobi; and the author stated, that in making galvanic experiments similar appearances occurred to himself, which I dare say was very true. Even the editor of the Magazine, though leaning to his correspondent,

had the candour to admit that even he had not seen the letter, though occurring in his own publication."

Nothing can be fairer than to let Mr. Spencer speak for himself. Now to answer him. 1. My charge was not that he had copied his 'experiments' from a letter by Mr. Jordan (however true it may be), but that he had made that letter the basis, the skeleton of his own pamphlet, and had said nothing beyond Mr. Jordan.

2. Mr. Spencer says, "he had never seen the letter," till 1844. This assertion is new, but false. I have before stated, and now again seriously, considerately, and deliberately repeat, that when I mentioned to him having seen the letter on its publication (see page 18), and named some of Mr. Jordan's results, he then spoke of it familiarly, as perfectly acquainted with the letter; and which I could not doubt to be the case, as he was a reader of the 'Mech. Magazine,' and had the reputation of great familiarity with scientific periodicals generally. He said the letter did not affect him, and I took him at his word at the time. Perhaps the parallel given in Chapter III. will throw additional light on this matter, and may surprise all who find two inventors following each other so closely, particularly when one writes four months after the other. IF Mr. Spencer was ignorant of Mr. Jordan's letter, in spite of our conversation at one of the societies in Liverpool, his own pamphlet must henceforth be considered as one of the most astonishing instances on record of two individuals, 200 miles apart, experimenting, thinking, and writing distinct productions, in an almost uniform coincident strain! Truly does he say—"duplicate discovery is but seldom the result of mere chance alone."

3. As to whether he could "have obtained anything practical" from the letter of Mr. Jordan, the unprejudiced and scientific reader can best judge by referring to it again, at page 7. Mr. Spencer is either very willing to be suspected of an exceedingly dull comprehension, or must suppose the scientific world are to be blinded by any statements, made with sufficient assurance and boldness, to dare thus to depreciate and stigmatize Mr. Jordan's efforts.

4. His outline of Mr. Jordan's truly lucid exposition of the science of electro-metallurgy, in its very commencement, and before the much vaunted enlightenment afforded to the world by his own pamphlet in Sept. following, is meagre, unjust, and entirely false; but it is an excellent comment on his own empty notice of May 9, 1839 (p. 3). This constant gross depreciation whenever Mr. Spencer alludes to Mr. Jordan, marks his every statement,—a common indication of a weak and indefensible cause.

5. The editor's absence from town, and his functions being temporarily in other hands, was the real cause of the oversight which Mr. Spencer has the effrontery to turn into an insinuation expressive of the utter insignificance of Mr. Jordan's ingenious communication, as if even the editor himself "had the candour to admit" &c. Certainly, "nothing can be more absurd than this!"

Fortunately, Mr. Jordan's position receives neither accession nor diminution from Mr Spencer's pretended ignorance or knowledge of his letter. It is sustained by its intrinsic merits. Mr. Jordan had

published when Mr. Spencer had NOT published, —a fact decisive on the question. But we will listen to Mr. Spencer, who says,—"My claims have been usually admitted by those who have written on the subject, as dating from May 9th, 1839, because at a meeting of the Polytechnic Society held on that day a letter was read from me to the secretary, bearing the date of May 6. This letter, which is entered on the books of the Society, mentioned some of the results of the discovery, and also that I had been engaged in perfecting (!) the process for a considerable period." Mr. Spencer should by all means have read this letter at the dinner,—a letter so important, and containing, too, "some of the results of the discovery," so far back as May 6, 1839, affording evidence (as the unwary might suppose) of a prior claim! But no; although this letter is, as it now appears, his mainstay, yet neither in the controversy of 1844 nor at the dinner of 1851, where he professes to give "those facts and dates" which have connected his name with the early history of this art, was this 'fact' ever before produced! This, then, is the letter (p. 3), the mention of which has duped scientific writers from 1839 to 1844, and by which it is even now attempted to deprive Mr. Jordan of the distinguished honour of being the inventor of the electrotype. (See the letter, p. 3.)

It will be here noticed, how Mr. Spencer colours and endeavours to render important this forestalling intimation, suggested by Prof. Jacobi's previous notice, of which it is a mere counterpart. It is not a 'communication,' a sealed packet containing a descriptive account of his entire process up to that period. It is—nothing, and all such notices go for nothing, in all scientific matters, in deciding the rights of independent inventors,—he alone ranking as the first and true inventor, who, by a 'communication,' first places the public directly, or in trust, in actual possession of the invention claimed. Mr. Spencer did nothing of this kind till Sept. 12, 1839. Had he died previous to that date, *his* secret would have been lost. Mr. Jordan wrote May 22, and his letter was not merely a *notice*, but literally a "communication,"—complete, explicit, and unreserved. And this conclusive fact will be admitted by the universal assent of an impartial and discerning public.

Mr. Spencer adopts, in a large portion of his speech, a system of accumulative evidence, as if confidently hoping that proofs of early engagements in electrical pursuits would strengthen the claim he has set up in connexion with electro-metallurgy. Thus, he read a paper Feb. 12, 1839, on 'the theory of the formation of metalliferous veins, by galvanic agency, in the interior of the earth,' when he "succeeded in obtaining specimens of pure crystalline copper" Then, again, his friends see his specimens, and so he proceeds, with an amusingly tortuous ingenuity, to assert a right to date his "discovery" anterior to May 9, 1839, — observing, "Now, the date of this paper carries my claims to priority very much further back than I have hitherto deemed it at all necessary to go!" This kind of reasoning, among partial personal friends, met for conviviality, may be received by them with considerable approval; but I will not insult the scientific understanding or the common sense of my readers, by the refutation

of such special pleading. Certainly, not even the tyro in chemistry, much less any practician in electro-metallurgy, however deficient in scientific history, will treat otherwise than with contempt such vain bombastic claims as these to an invention which he carefully, cautiously, and with a prospective object, declares, on the 9th of May, 1839, was even then only "partially successful!" What has the mere obtaining of "pure crystalline copper," by Dr. Bird's process, (see p. 10) to do with the quality and distinctly purposed application constituting the peculiar indispensable characteristics of Jordantype?

I contend, therefore, that this latter course of proceeding is quite untenable, opening, as it does, a wide field for dispute, both between the present parties as well as others who might become claimants on similar grounds. It looks, too, like the last resort in a dying cause. Besides, Mr. Spencer, while pleading his own case, with singular fatuity leaves his audience to take for granted that Mr Jordan could not prove as much,—conveniently forgetting, likewise, that Mr. Jordan himself, in his very Letter, refers to a period "early in the summer." But I deny the right of producing such doubtful evidence against accredited evidence. Admitting, on his own showing, Mr. Spencer's originality and independence as an inventor, just as I have proved Mr. Jordan to be original and independent,—how stands the case? We have two inventors, operating under similar circumstances, stimulated to print by the same cause, one writing a communication May 22, 1839, the other writing a *notice*, merely paraphrasing the vague rumour about Jacobi's experiments, dated May 9, 1839, and subsequently reading a communication to a Society Sept. 12,—nearly four months after Mr. Jordan. Dates so wide apart, from parties, too, so wide of collusion, render the settlement of this long protracted dispute so simple, that I fearlessly predict a verdict in favor of Mr. Jordan and consequent assent to the title of JORDANTYPE.

Since the foregoing chapters were written, I have received the following letter from Mr. Jordan.

Sir,—Mr. Spencer having reasserted his pretensions to priority in electrometallurgic discovery, after his long period of silence, I am reluctantly obliged to answer them. In doing so, it is not worth while to notice the depreciatory tone of his remarks towards myself, nor can I allude further to his protestations concerning "higher principles than personal vanity," (to which, by the way, he exhibits in himself a flat contradiction)—"honesty of purpose involved in a claim which he never suspected would be questioned," "clogging to science," &c. —than to insist upon their application with greater truth to my own case. The clearest proof of this is afforded by the fact, that, having discovered and published the electro-metallurgic process, I left my own claim (securely recorded) to rest on its own merits alone, without any accompaniment of personal considerations whatever.

The real question at issue between myself and this Mr. Spencer resolves itself into a very small compass: it is simply a question of dates. There exists in the pages of the 'Mechanics' Magazine' a

most unquestionable proof amply sufficient for impartial judgment to decide my claims of independent discovery and PRIOR PUBLICATION of the process in question, and entirely ignoring any secondary assumption on my part, whatever may have been its effects on himself. It appears, however, failing to overcome this stubborn fact of anterior publicity, Mr. Spencer now urges a reference to his "experiments" of a period antecedent to publication, as made known to his acquaintances, and as recorded on the books of a Liverpool Society. Surely no evidence of this nature can have the absolute force of unalterable proof which is possessed by a well-known PRINTED periodical, of which many thousand copies are circulated, conveying clearly to the public a descriptive detail of a process which others confine to the small and private circle of a provincial institution,— PROOF which is, or ought to be, quite sufficient to set the dispute at rest. This power cannot be accorded to any oral or written testimony, even if descriptive, and certainly not if unpossessed of this character.* A thousand such notices as that given by Mr. Spencer would still have left the public totally in the dark as to the process itself, which he did not describe until after I had done so. Between the period of appearance of this empty notice and his subsequent pamphlet, my own Letter was published, which at once cut asunder his chain of evidence, and throws doubt upon the originality of his subsequent proceedings—a suspicion which amounts to a conviction on a perusal of your able 'parallel' (p. 16).

It is evident, therefore, that the question is determined by ascertaining—Who actually published the first description of the electro-metallurgic process? since he alone is entitled to be acknowledged as the originator of a new art who gives the first public and decisive proofs that he is so. A reference to the 'Mechanics' Magazine' will show beyond doubt that I have completely satisfied these conditions. To quote in illustration the admission of a distinguished physicist, in reference to some disputed photographic process,—"If it was really first published by ——, he ought certainly to have the credit of it, according to the usual rule." By this rule I abide.

Mr. Spencer seems to calculate upon the credulity of his readers, in his equivocal representations of dates. Thus, in his first notice, of May 9, 1839 (see p. 3)—now May 6 (?)—he states that he had been making 'occasional experiments' for the last 9 months; whereas, in his pamphlet of September 12, these 9 months—like Falstaff's men in buckram—are expanded to '2 years'! Now, again, it appears, this is not deemed sufficient; for, in his speech, he endeavours to 'carry back' his claims very much farther than he had deemed it necessary! upon the strength of some experiments, made with a different object,

* *As far as the public is concerned, the proceedings of any society are as strictly private as those of a single individual, if not sent through the legitimate and accredited channels. The press alone, in these days, publishes; and both individuals and societies also, by availing themselves of its functions, acknowledge the fact: without this engine we might remain ignorant even of the existence of any society, scientific or secret; to the press these are indebted for their very publicity. Do inventions become publicly known by being 'booked' in the pages of a society? These observations apply to an erroneous opinion given in a public periodical, and not to Mr. S.—his barren notice not being, however issued, a publication, valid evidence, or anything of substance.*

which he attempts to strain into a convenient shape. When he speaks of the labours of others, a very different course is adopted. My letter was written in May, but Mr. Spencer can only acknowledge June—the date of its actual appearance—such deference does he pay to real evidence, when it suits his own purpose.

It is not easy to see for what reason Mr. Spencer appears to take for granted that a knowledge of the electrotype was not possessed by myself before the date of my letter; or that I cannot urge my claim in a similar private form. I have never hitherto done this, for the reasons assigned, although I possessed a knowledge and had planned experiments therein many years before its appearance,—deriving my ideas neither from Bird's arrangement, nor the original battery of Prof. Daniell, in support of which I can urge personal evidence. I do not attach any importance to such allegations in these cases,—my claim resting on the all-sufficient fact of bonâ fide publication prior to any one else.

Although the fact of his having availed himself of it is too palpable to be doubted, yet Mr. Spencer has the gross effrontery to deny his knowledge of the existence of my Letter previous to his pamphlet, following up this statement with the sneering remark, that even if he had, "he could not have obtained anything practical from it." That the first assertion is false, you have proved; while an inspection of your 'parallel' exposes the wholesale borrowing and 'annexations' of this "high principled" repudiator. The applications of electro-metallurgy in my letter are each and all perfectly practical—some economically so. I stated this in my letter, I repeat it here, and shall take a future opportunity of recurring to the subject. For the present, I most unhesitatingly assert them to be not only entirely practical, but strictly the electro-metallurgic processes applicable to useful purposes, a feature which does not belong to the production of half-casts of coins and medallions, for the amusement of amateurs too often ignorant of the rationale of the process they employ. Mr. S. himself has sufficiently contradicted the stigma he now attempts to cast upon them, by adopting them in his own pamphlet, but without acknowledging the source.

Mr. Spencer talks absurdly about "perfecting the process," and takes credit for the apparatus sold in the shops being that which he adopted for electro-coining. It is nothing more than the apparatus of Daniell, and has received no more perfection from his "occasional experiments" than the process to which it is applied.

To conclude, what would have been the value of Mr. Spencer's adverse pretensions if the form of my communication on the electrotype had been that of a patent?—for the *non est* of which I have been frequently blamed, and can only now regret. Recognition would then have attended fortune, as it always does, and I should have defied the vexatious rivalry of a scientific 'Bottom,' which has acquired strength simply by my early silence,—affording a sickening verification of a statement I have somewhere met with, that "a lie, current and uncontradicted for a week, is as good, for all public purposes, as the best truth ever promulgated."

Confident, however, of the impregnability of my position, I am, &c,

Feb. 1852. C. J. JORDAN.