practically identical with what is now known as Sir William
Thomson's galvanometer, having been re-invented by him.

The greater part of Weber's work, however, was in close re-
lation with the working out of the electro-magnetic measurement
of electric currents, as in the case of his invention of the
electro-dynamometer, now so extensively used in the numerous
forms which it has assumed to meet the various requirements
of the electrical engineer.

Weber's attempt to explain the phenomena of electric current
induction by means of an electrostatic repulsion between
moving particles carrying electric charges formed a portion of
his researches on the electro-magnetic measurement of electric
currents, published in 1864 and subsequent years under the
title of "Elektrodynamische Bestimmungen," in the
"Abhandlungen der Math. Phys. Classe der königlichen
Sächsischen Gesellschaft der Wissenschaften." Helmholtz
showed that Weber's formula led, in certain cases, to im-
possible results, but Prof. Hindmann, in 1884, showed that
certain assumptions as to the nature of the molecules of matter
led to Weber's formula, so long as the relative velocity of the
particles does not exceed the velocity of light, but when this
critical value is exceeded the law no longer holds, and in this
form it is not open to Helmholtz's objection.

Weber's well-known theory of magnetic induction was
another contribution to electrical science of which it would be
difficult to exaggerate the importance, for, although in the
light of our present knowledge it is in many respects inad-
quate, it undoubtedly formed the basis upon which a theory of
magnetic induction is being gradually built up.

---

THE HISTORICAL DEVELOPMENT OF THE INDUCTION
COIL AND TRANSFORMER.

BY DR. J. A. FLEMING, M.A.

(Continued from page 213.)

In Stilliman's Journal of Science, Page made some very excellent
remarks upon the action of "closed circuits" in preventing or
reducing the inductive action of the primary current upon the
secondary circuit. He states that if a closed circuit, such as
a metallic sheath or tube, is interposed between the primary or
secondary circuits, it more or less annuls the inductive action
of the former on the latter. He hence notes that wrapping,
what he calls a "compound magnet," i.e., an induction coil with
two separate circuits, in a metallic sheath, greatly reduces the
inductive action of the primary on the secondary, although it
does not prevent the action of the primary circuit in permanently
magnetising the core. The same action he points out proves
prejudicial if bobbins, made of metal, or having metal cheeks,
are used upon which to wind the circuits of the coil. Hence,
bobbins for induction coils should be made entirely of non-
conducting material.

We also find that in 1840, J. H. Abbot, of Boston, U.S.A.,
constructed a large induction coil (see Stilliman's American
Journal of Science, Vol. XL, April, 1841, p. 107), which was
capable of charging a Leyden jar, and which gave small
secondary sparks. The break was a hand-worked break (see
Fig. 16a). The secondary terminals were described as being
luminous in the dark when the coil was in action. This coil
was modelled on Page's.

It is evident from the foregoing that, even in the year 1838,
C. G. Page had brought the induction coil to a high degree of
perfection by his researches made at Salem and at Washington,
and in particular had shown that the secondary circuit could
exhibit effects of "tension" and produce electrostatic change
in conductors having capacity connected with them when
induction coils were used in which the secondary currents con-
sisted of great lengths of fine wire. He had obtained sparks
in air from the secondary terminals of coils, and shown the
conditions under which these "electrostatic" effects could be
exulted. We must also credit him with being an independent
inventor of the self-acting hammer electro-magnetic contact
breaker.

Continuing his researches, Page made many coils between
1838 and 1850, having highly insulated secondary circuits and
vibrating automatic contact breakers. He found, as above
observed, that he could charge Leyden jars, divide the gold
leaves of electroscopes, and produce many of the effects hitherto
only obtained with electrostatic machines by means of these
induction coils. With one of his coils he found he could obtain
sparks half an inch long in air from the secondary terminals.
He noted, also, the effect of rarefying the air round these
terminals on the length of discharge. With one coil, which
gave a secondary spark of 1/12th of an inch in air, he obtained a

FIG. 16a.—Abbot's Induction Coil (1840).

FIG. 17.—Wagner's Automatic Contact Breaker.

---

* The greater part of Dr. C. G. Page's valuable work is recorded in his
own Papers, published in Stilliman's American Journal of Science between
1834 and 1850. He published in 1867 a brochure entitled the History of
Induction, which is alluded to by Du Moncel in his work (Vol. II.) Recherches
sur les Applications de l'Electricité, but this pamphlet of Page's is not to be
found either in the British Museum Library or in the English Patent
Office Library.

---

seconds, according to the position of the iron core. Page
also noted a phenomenon, afterwards recorded by Belye in
1855, viz., that when the primary circuit was broken just
between the poles of a powerful magnet the break-spark was
extinguished with a loud explosion, like that of a pistol when
fired.

§ 10. Wagner and Nees' Automatic Contact Breaker.—
Continental writers generally attribute the invention of the
automatically vibrating electro-magnetic contact breaker to J. P.
Wagner* and to Neef.† These inventors improved upon Page's mercurial break by constructing the vibrating armature with platinum contacts in the form in which it is now used in every trembling electric bell. The apparatus is too well known to need description. A form of Wagner's hammer, as it is generally called, is shown in Fig. 17, intended as an interrupter to the primary circuit of a coil. Du Monceau, however, states that MacGauley, of Dublin, independently invented the form of hammer contact as now used. From this date onwards it has generally been the custom to interrupt the primary circuit of an induction coil, if small, in the manner introduced by Page in 1838, viz., employing the intermittent magnetism of the soft iron core of the coil to work the vibrating hammer of the break, using, however, the platinum contacts of MacGauley or Wagner instead of Page's mercurial cup. On the other hand, when the coil is large, the break is usually made as a separate piece of apparatus with independent magnet, and in that case it is often found best to revert to the mercury cup break of Page, and cover, as he did, the surface of the mercury with oil or alcohol to prevent oxidation, as was done subsequently by Ruhmkorff and others. As is usually the case, useful improvements are invented several times over by inventors who are not familiar with what has already been done. We find in Sturgeon's *Annals of Electricity,* Vol. V., p. 30, a description of a coil by Thomas Wright, of Knutsford, dated 1840, in which he gives a sketch of a neat form of vibrating contact breaker (see Figs. 18 and 19), which is practically the same as that used on all small coils at present. Wright followed Neef and Wagner in suggesting that the contact points should be tipped with platinum. He succeeded in making some "electrotomes," as they were called, in which the spring vibrated so fast as to give out a musical note. Wright also gives details of many coils made by him about this time to determine the best dimensions of the coil. It will be seen that by the year 1840 the induction coil had been practically completed in all essential parts, with the exception of the condenser. The separate primary and secondary circuits of thick and thin wire, the divided iron core and the vibrating contact breaker had been arrived at and perfected; chiefly by the investigations of Callan, C. G. Page, Sturgeon, Buckhoffer, Wagner, and Neef.

§ 11. The Researches of Masson and Breguet.—Between 1838 and 1842 the French physicist, MM. Masson and Breguet, prosecuted researches of a valuable character on the induction of electric currents. In the *Annales de Chimie et de Physique* (3rd Series, Vol. IV., 1842, page 129) will be found a long memoir, summing up the results of their work, which was communicated to the Academy of Sciences on August 23rd, 1841. This Paper contains an account of very careful experiments on the production of electrostatic effects by secondary induced currents. In their investigations, Masson and Breguet employed a toothed wheel interrupter, or rhotrope, consisting of a brass toothed wheel having the teeth interspaced filled up with wood or ivory; one or more such wheels could be revolved on the same axis, and, by means of springs pressing the periphery of the wheels, the primary current could be broken, and the secondary circuit closed at instants corresponding to the closing or opening of the primary circuit. By the help of this apparatus they could separate out the two induced currents, and by means of a condenser and electroscope they examined the electrostatic potential at various points on the secondary circuit. Not being aware of the previous researches of Page, these investigators were apparently under the impression that they were the first to show that a condenser could be charged from the ends of the secondary circuit. Transforming, as they called it, induced currents into static electricity, they produced luminous discharges in vacuo by means of induced currents, and showed that these discharges had all the characters of discharges produced by electrical machines or Leyden jars. These results surprised European physicists, who, as Du Monceau observes, were apparently not aware that similar experiments had already been conducted in America. Amongst the chief results of this investigation was the experimental confirmation of the fact that the two secondary induced currents, the one produced by the commencement of the primary current, and the other produced by its cessation, consisted of equal quantities of electricity set flowing in opposite directions. Also the other important fact that the maximum value of the electromotive force of the secondary current at break of the primary is much greater than the maximum value of the secondary current at the starting of the primary, was stated by them. Hence it became clear that the direct or break-induced secondary current could traverse air spaces or overcome resistance which was impossible in the case of the inverse or make-induced current. The researches of Masson and Breguet established on a quantitative basis more firmly than before the facts of the induction of electric currents, but they did not achieve more than had previously been done in exciting the spark-producing power of secondary coils.

§ 12. Henley's Coil.—In the years between 1842 and 1851 very little progress appears to have been made in improving induction coils. Some modifications of arrangement either of the coil or the contact breaker were introduced. To this period, probably, belong two such varieties which are mentioned and described by Noad (Manual of Electricity, Vol. II.). One of these is represented in Fig. 20. The primary coil consisted of about 35ft. of insulated copper wire (No. 21), and the secondary of 1,400ft. of silk-covered copper wire. The battery contact is renewed and broken by the rotation of the soft iron bar, which, mounted between two brass supports, is situated immediately over the axis of the coil, in which is placed a bundle of iron wires. The current from the battery passes through the support and the axis carrying the iron bar;
and the contact is interrupted by the small steel point, dipping as the bar revolves into and out of the mercury contained in the brass cup mounted on the brass pillar, through which the circuit is complete. This apparatus formed a self-acting shocking coil, but could produce no sparks from the secondary. Noad describes also an interesting form of coil which bears some relation to Page’s electro-magnetic multiplier, and which, Noad says, was given to him by Mr. W. T. Henley. It is represented in Fig 21. A series of U-shaped bars of soft iron bolted down to a base board was wound with four coils of No. 14 covered copper wire, to within an inch of either extremity. Over this was wound 1,000 yards of No. 34 silk-covered wire in one continuous length. A revolving iron armature rotates on a vertical spindle, so that in passing over the magnetic poles it nearly completes the magnetic circuit. The contact breaker consists of a bent lever, one end of which dips in a mercury cup and is so pivoted that the motion of the other end against the undulating surface of a cam-wheel attached to the spindle serves to dip the first-named end in and out of the mercury. The break of the primary circuit is so adjusted as to take place just after the soft iron armature has completed the magnetic circuit. Actuated with a battery of 10 cells this coil gave sparks one-eighth of an inch long in air, and remarkable for their quantity. It is easily seen that the appliance is, at the same time, an induction coil and also an electro-magnetic motor, and that a suitable arrangement of the time of making and breaking the primary circuit will cause the soft iron armature to be kept continuously in motion. It was noted as a curious fact that when the ends of the secondary circuit were metallically joined the spark at the primary break was much reduced. This is obviously due to the fact that the closing of the secondary circuit reduces the self-induction of the primary coil. These pieces of apparatus formed no real advance on what had been done before.

(To be continued.)

ELECTRIC AND MAGNETIC THEORIES.

BY SILVANUS F. THOMPSON, F.R.S.

§ 1. As Mr. Sprague has called in question the method of my argument, which I took the pains to explain beforehand in § 9, so that no one should have any ground to accuse me for laying a pitfall, I begin my rejoinder by quoting a few lines from Mr. Sprague’s reply, p. 188, § 2.

“The real point in debate is how energy is transferred by an electric current. I ask him, has he any evidence of a single case of lateral transfer during the existence of true current? It is notorious that there is no scrap of evidence.”

“He has dealt (in the way of evidence) solely with the phenomena of the variable periods.”

“Therefore he avoids this difficulty by a logical dodge. He says, practically, if I can show that there are some cases in which we must agree that energy is transferred through the medium, I am entitled to assert that it is so in all cases. Bad logic and worse science.”

Now, without admitting for a moment that I resorted to any “logical dodge,” or argued with bad logic and worse science, I will at once say that I entirely agree with the first four sentences that I have quoted. There is no evidence (and I said as much on p. 44) during the steady flow of the current that the energy is transferred laterally across the medium. Neither is there any evidence during the steady flow that the energy is transferred along the wire longitudinally. The simple fact is that the current in the case of unvarying flow gives no evidence at all of the mode of propagation of the energy, and, therefore, in the present state of science affords no basis for an argument either way, in favour of lateral propagation or in favour of longitudinal.

Under these circumstances what can we do, in default of any crucial proof, to discover how the energy is propagated? The only course is that which has been the practice of every branch of science, and which has proved the key to discovery, namely, argue forward from the known to the unknown. Find a case in which direct evidence exists, and argue on from this to those cases in which we have not yet been able to find direct evidence. If this procedure is to be stigmatised as a “logical dodge” then I fear that most of the advances of science during the last century must be set down as the result of logical dodges also. To argue from the known to the unknown, and then to test the result by probing to the bottom the necessary consequences of the step, is the one true scientific course in such a dilemma.

What did I say at the outset, on page 44? “Since in those cases in which the hypothesis of lateral transfer cannot in the present science be directly demonstrated, there is no evidence to show that the transfer takes place in any other way, then in the absence of such evidence one is logically driven to the conclusion that in all cases the mode of transfer is the same as in those cases in which a direct demonstration is possible.” Is not that simply a statement that I proposed to argue from the known and demonstrable to the unknown and yet undemonstrated? A “dodge” it is not; it is a legitimate proceeding, both logical and scientific.

§ 2. Mr. Sprague denies the legitimacy of arguing from the cases of variable currents to that of steady currents. He virtually says that, when looking for evidence of the invisible mechanism by which a current is sustained, we have no business to regard any evidence derived from altering the strength of the current. Why does he lay down any such short-sighted limitation? If I want to find out whether a leak of gas comes from a certain pipe, must I refuse to make such