Here, then, to me, as it would have been to any other chemist, was a very suggestive fact, and a means offered whereby the singular phenomenon that Faraday discovered might be hopefully investigated. Now, the part of the gold-leaf that is immersed in the cyanide-solution may be viewed as merely an extension of the under-surface of the gold-leaf that lies on the surface of this solution, and so I was led to suppose that the phenomenon had, as it were, an electrical side—was, in fact, if not produced electrically, at least contemporaneous, and connected with a current of electricity; so, acting on this idea, I connected a square of gold-leaf that laid on the cyanide solution with a square of the leaf submerged therein through a galvanometer, when an electric current manifested its presence, the direction of which would have been a surprise to Faraday himself, for it indicated that the current proceeded from the submerged gold. Thus the idea of the rapid dissolution of gold lying on a cyanide-solution being due to electromotive force generated at the surface, as Faraday supposes, receives a very rude shock.

Following up this new line of investigation it was soon found that gold in strong solutions of the cyanide is always positive to gold in weak solutions of this salt, and that the current thus generated is sufficiently strong to give electrolytic effects—*e.g.*, the deposition of copper and gold from their sulphate and chloride respectively. These results, while showing clearly and undeniably that the rapidity with which gold dissolves when lying on a cyanide-solution is not due to the generation of any electrical current at all, shows just as clearly the fact of the potency of cyanide-solutions of different strengths in contact with the gold for accelerating the cyaniding of the metal; and the inference I would make from this is, that the rapidity with which this surface gold dissolves is in greater part, if not wholly, due to the action of cyanide of different strengths—in fact, to the action of the stronger solution on the under-surface of the leaf, its upper surface forming in the weaker cyanide the negative pole.

Now, that the solutions in the vicinity of the gold-leaf as there situated are, or soon get to be, of different strength, appears certain for the following reasons:—Gold-leaf is, as Faraday long ago pointed out, very porous, and it is easily conceivable that the easily-decomposable cyanide-solution, rising through the capillary pores of the metal, and subjected therein to oxygen condensed on their sides, would undergo a partial decomposition. There is this, too, to be taken cognisance of, and that helps to perpetuate this difference in cyanide strength: The solution next the air cannot so well be recuperated from the normal solution as can the solution underneath, the former being almost cut off from the underlying solution.

In this connection I would state that my experiments show a very small negative pole will serve for quite a large positive pole, reckoning by superficies. Therefore, a local weakening of the solution at any point would give us a general dissolution of gold over the parts of the metal that have still contact with the normal solution. If any one should reply to this theory for the dissolution of gold that by the same rule a solid body—say, glass—placed over the gold-leaf should also favour the production of the heterogeneity in the solution which my theory requires, and *which it does not*, I would answer that solid substances thus placed prevent the escape of air out of the capillary cavities or pores of the gold-leaf, and so keep the cyanide-solution from that contact with the surface-gold necessary to complete the interpolar connection between its upper and lower surfaces.

Now, if differentiation of the strength of the cyanide-solution does explain the rapidity with which gold, as placed on a cyanide-solution, is dissolved, differentiation in strength must be a factor in the dissolution of gold in the cyanide process generally. I conceive it, indeed, to be the key, the mainstay, of the process as worked at the mines. The initiation of this is easy, and easy to understand: the percolation of the cyanide-solution through the charge—this "drip, drip, drip,"—and in a damp atmosphere, is all very favourable for its production, also for its maintenance. Thus the cyaniding of gold appears to be eminently an electric process: not that the gold is dissolved therein by means of electric currents (not even in the case of gold on the cyanide), but that, on the other hand, it generates these currents—gives us, in fact, electro-motive power; and if the mechanical fixings are not present which are necessary for the setting-up of these voltaic currents, no dissolution occurs. In reality, the dissolution of gold in the cyanide is analogous to the dissolution of zinc in the sulphuric acid of a Smee's cell. If means are not present for the production of the voltaic circle no pronounced, no regular, action takes place—it is merely a difference in the construction and the arrangement of the cell. Thus, in the Smee's cell it is one solution and two metals, but in the cyanide process one metal and one solution at the start, which solution, from the causes I have named, and probably others in conjunction, becomes of various strength, and so fulfils the condition necessary to insure the existence of the voltaic circle.

The Theories of Dr. Keith and Dr. Hood.

In this connection it is proper that I note, and shortly discuss, two theories that I just learn from the second edition of Rose's "Metallurgy of Gold" have been constructed to show how the chemical actions obtaining in the cyanide process are initiated and brought about. Dr. Keith suggests, in "Engineering" for 1895, page 379, "that the action of oxygen (in this process) is due to its strongly electric negative relation to gold in the cyanide-solution." Making use of Dr. Gore's table of the electro-motive power of certain metals in these solutions as compared among themselves,* he selected carbon for experiment to support the theory, and, as he states, found that it greatly accelerated the dissolution of gold—a fact that twenty years before I had stated would occur.

In regard to this supposed capability of oxygen to act the part of a negative pole—to substitute a metal—say, mercury—in the voltaic cell—I must, however, strongly dissent from him, and would

* My own table for this purpose was published two years before Dr. Gore's table was. The tables pretty well agree as far as they go; but, as Dr. Gore's was published in the Transactions of the Royal Society, it takes precedence over mine in the editorial mind.