



An answer to Dr. Hare's letter on certain theoretical opinions

M. Faraday

To cite this article: M. Faraday (1840) An answer to Dr. Hare's letter on certain theoretical opinions , Philosophical Magazine Series 3, 17:107, 54-65, DOI: [10.1080/14786444008650107](https://doi.org/10.1080/14786444008650107)

To link to this article: <http://dx.doi.org/10.1080/14786444008650107>



Published online: 01 Jun 2009.



Submit your article to this journal [↗](#)



Article views: 7



View related articles [↗](#)



Citing articles: 1 View citing articles [↗](#)

any hypothesis which I could imagine. How are we to explain the insensibility of a gold-leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

38. Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles, or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analogous to that which determines the capacity for heat, and the different influence of dielectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,

ROBERT HARE.

An Answer to Dr. Hare's Letter on certain Theoretical Opinions. By M. FARADAY.

MY DEAR SIR,

i. **Y**OUR kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgement. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

ii. My theory of induction (as set forth in Series xi. xii. and xiii.) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject

(1667). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phenomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

iii. Bodies whether conductors or non-conductors can be *charged*. The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or which the prime conductor of a machine acquires when the latter is in action; at other times it means the state of a Leyden jar or similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

iv. Let three insulated metallic spheres A, B and C be placed in a line, and not in contact; let A be electrified positively, and then C uninsulated; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory: A will be charged positively; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it; and C will be charged negatively.

v. The ball B will be in what is often called a polarized condition, i. e. opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (iii.), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (iv.) is concerned.

vi. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every

part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly; B assumes a polar condition; and C is charged inducteously (1483.), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

vii. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (v.)) does (1672.). The word charge as applied to a Leyden jar, or to the *whole* of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

viii. As another expression of my theory, I will put the following case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (v.) as regards its inner and outer surfaces.

ix. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

x. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

xi. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external

sphere C as before, and positive force equal to 100 will appear on its outer surface.

xii. Such a state of the space between the inductric and inductive surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

xiii. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative electricities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

xiv. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductive influence of A would be conveyed outwards to the inner surface of the sphere C, though that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

xv. The actions of the two portions of space, as they have just been supposed to be constituted (xiv.), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

xvi. In my papers I speak of all induction as being dependent on the action of contiguous particles, i. e. I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductric body acts upon conductors at a distance, it does so by polarizing (1298. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used

the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it: first by saying at par. 1615, "the next existing particle being considered as the contiguous one;" then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my *Researches*, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

xvii. Finally, my reasons for adopting the molecular theory of induction were the phenomena of electrolytic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.), of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

xviii. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develope principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide which is in the right.

xix. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page*. In referring to my own mat-

* We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.

ter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

xx. At paragraph 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that should be half an inch off. With the meaning which I have carefully attached to the word *contiguous* (xvi.) I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

xxi. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shown that the principle of communication is not the same in both.

xxii. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

xxiii. Paragraph 4 is answered by the above remarks and by viii. ix. x.

xxiv. Paragraph 5 is answered according to my theory by viii. ix. x. xi. xii. and xiii.

xxv. Paragraph 6 is answered, except in the matter of opinion (xviii.), according to my theory by xvi. The conduction of heat referred to in the paragraph itself will, as it appears to me, bear no comparison with the phænomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the

first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (viii. ix. x. xi. xii. xiii. xiv.) exactly equal to the exciting or moving force, which is still at the centre.

xxvi. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavour to set myself free. You say I “correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177). But if all this be true, how can there be a *positively excited particle*? (see par. 1616). Must not every particle be excited negatively if it be excited positively?—Must it not have a negative as well as a positive pole?” Now I have not said exactly what you attribute to me; my words are, “it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177.)” I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

xxvii. Using the word *charge* in its simplest meaning (iii. iv.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of iv., v., vi., vii., fully illustrate my views (1672.).

xxviii. Paragraph 20 includes the question, “is this consistent?” implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding para-

graphs. I believe that it is. I have charged small Leyden jars made of thin flint glass tube with electricity, taken out the charging wires, sealed them up hermetically, and after two and three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges*. I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my Researches; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by what I call convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating*; and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

xxix. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inductive surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the "some other directions" spoken of (1682. 1683.).

xxx. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (iv., v.,) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (x.) or the myriads of polarized conducting particles (xi.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

xxxi. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged

* *Bibliotheca Italiana*, 1837, lxxxv., p. 417,

state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (xxx.), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass of the jar, or the polarized globe B (v.), to their first state. My view of the relation of insulators and conductors, as bodies of one class, is given at 1320. 1675. &c. of the Researches; but I do not think the particles of the good conductors acquire an intensity of polarization anything like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy: thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

xxxii. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), "what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opakeness and translucency? and so forth.

xxxiii. I admit that these questions are very properly put;

not that I go to the full extent of them all, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phænomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in the gravitation of different bodies being considered due to differences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phænomena in question.

xxxiv. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phænomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class,) and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

xxxv. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in *degree* or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance: and it is certain whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

xxxvi. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that hot and cold are phænomena of the same kind. As it is of consequence to show that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to show that one body insulates and another conducts only in consequence of a difference in *degree* of one common property

which they both possess; and that in both cases the effects are consistent with my theory of induction.

xxxvii. I now come to what may be considered as queries in your letter which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act on each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (xvii.); and next, that in the arrangement given iv., ix., or x., A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

xxxviii. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687. 1688. 1699. 1700. 1701. 1702. 1703. 1704. in the *Researches*, would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (v., xxvi.), with the case given (iv., v.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of a conductor under induction (v.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

xxxix. Paragraph 11 involves a mistake of my views. I do not consider bodies which are charged by friction or otherwise, as polarized, or as having their particles polarized (iii., iv., xxvii.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (xxxviii.).

xl. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon the reconsideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the

contrast of facts to hold my judgement as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be, with the highest esteem,

My dear Sir,

Your obliged and faithful Servant,

Royal Institution, April 18, 1840.

M. FARADAY.

X. *Notices respecting New Books.*

Report on the Progress of Vegetable Physiology during the Year 1837.

By F. J. F. MEYEN, M.D., *Professor of Botany in the University of Berlin.* Translated from the German, by WILLIAM FRANCIS, A.L.S. London, 1839. 8vo, pp. 158.

OUR readers will doubtless remember the valuable Report on the Progress of Vegetable Physiology for the year 1836, which appeared in our pages about two years since*. The high position occupied by Professor Meyen in this department of science, and the vast increase which is constantly being made in the amount of our knowledge of it, by the labours of the industrious physiologists of Germany, combine to give these reports a peculiar value. A great part of the information contained in them would not have found its way to this country in any other shape; and it is much more agreeable to obtain it in the condensed form it assumes after being submitted to the Professor's *compressorium*,—which squeezes away the lighter fluid with which it is diluted, and retains the solid matter,—than in its original state. The Report at present before us is equally full of valuable information with the former one; and, when its much greater extent is considered, its importance as a contribution to scientific literature will be apparent. The rapid advance of discovery in this most interesting science will give, we are assured, a progressively increasing value to these reports: and when the utility of a well-executed translation is considered, especially from a language which needs long study and familiarity to give a certainty of the author's meaning being understood, we

* Lond. and Edinb. Phil. Mag., vol. ix. p. 381, *et seq.*