

## **Confirming Idealized Theories and Scientific Realism**

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### **I. The problem**

In the traditional debate between realism and antirealism, or more broadly speaking, in any traditional systematic account of aspects of science, such as scientific explanation and confirmation, scientific theories or their constituent statements are assumed to be either true or false. The notion of idealization or approximation was rarely invoked. More recent participants of this debate seem to have noticed this neglect, and phrases such as ‘approximately true’ appeared more frequently in the literature; but the change is merely cosmetic: the phrases are added merely to extend the applicability of claims obtained by arguments that take no notice of approximation or idealization. What I can gather in reading philosophers such as Richard Boyd, who uses ‘true or approximately true’ rather than simply ‘true’ consistently in his writings is that a theory or a law statement in question is either not yet true but very close to being true or not yet true but well on the way to being true. Similarly, philosophers who acknowledge the fact that most, if not all, scientific theories or statements of law are idealized usually take such theories or law statements to differ from the true ones in only negligible degrees.

Similar situations occur in approaches of confirmation (or theory testing): while I do not off hand have any examples in the literature that discuss the confirmation of

idealized theories or theories that are only approximately true (except of course the case I will devote the present paper to discuss), it is obvious that most claims in the major approaches of confirmation can be easily extended to apply to approximately true theories or law statements. For example, if an account of confirmation works for the testing of a 'true' theory like Einstein's theory of gravity in general relativity, then it also works for the approximately true Newtonian theory of gravity in the appropriate context.<sup>1</sup>

As I have argued elsewhere, such nonchalant attitudes towards idealization and approximation are not justified, nor are they entirely harmless. Based on this assessment, I would like to find out in this paper what we can say about such questions as, what does it mean to confirm or disconfirm an idealized theory? Could an answer to such a question provide an account that is amenable to scientific realism? What I have here today is really work-in-progress and preliminary. I have by no means exhausted all the plausible answers to the first question, namely, I have not considered all the viable accounts for the notion of confirming or disconfirming idealized theories, nor have I any definitive arguments for or against scientific realism in the context that is independent of whichever account we adopt. What I did here is to discuss a particular account that provides so to speak a triangulation of the three things: approximation/idealization, confirmation, and scientific realism, and see where the discussion points to in terms of improving on our understanding of all three concepts.

If we assume for the moment, while reasons for this will be provided later, that idealization is not merely neglecting the negligible, and whatever is applicable to an idealized theory or law-statement is not automatically also applicable when the

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<sup>1</sup> For some reason, the situation with the literature on scientific explanation is very different. Not only models and the semantic view of scientific theories are widely discussed there, idealization and approximation are longstanding topics that receive serious attention.

idealization is removed, the first problem for idealized theories and realism, on the one hand, and idealized theories and their confirmation, on the other, is essential the following. If idealized law statements are false, how can there be an independent reality whose goings-on are said to be governed by such laws? And if idealized law statements are false, what sense does it have to try to test and confirm them? A further, presumably more serious problem threatens the legitimacy of the widespread use of idealization in science. If scientists spend their lifetime looking for laws, how can such laws, being necessarily untrue because of being idealized, deserve their efforts? To contemplate these problems, one must remember that idealization is indispensable in theory construction and the practice of science abounds in cases in which everyone involved knows that the idealized theory/model is not true.

At least two types of idealization coexist in the practice of science. Weary of the danger of formalization, I shall be content with illustrative examples in marking clear the differences of these two types of idealization. When idealization is used to draw up the model of ideal gas, from which Boyle's law is obtained, everybody knows that the model is false of real gases and Boyle's law is no real law of physics. It is rather a guide to the law of real gas that points to the right direction but provides no true description. But when idealization is used to obtain the notion of inertial motion, from which the first law of Newtonian mechanics is obtained, everybody knows that the model of motion on a frictionless plane is true and real. I am of course exaggerating when I say 'everybody knows,' but I assume the exaggeration is by no means confusing or misleading. This example is not the most ideal because of complications with the Mach principle and general relativity. A better example is perhaps the law of motion of a charge body, which

is under the electromagnetic influence alone, or some such cases. The point here is of course the truth, not just the idealized truth, of linear compositionality of causal influences. Because of the enormous complexity of actual phenomena, idealization is needed to 'decompose' causal factors, each of which may be governed by a unique set of laws alone (cf. Liu 2004).

The first observation I want to make is that neither is the ideal gas model the result of a trivial act that merely neglected the negligible, and yet it represents a type of idealization that is widely used in science, nor are completely isolated systems and the laws they instantiate regarded as false just because they are results of idealization. Therefore, even though the last sentence in the paragraph before the last is true, there are idealized models or theories in science which we have no reasons to regard as false *ab initio*.

Moreover, three philosophical attitudes seem to be possible regarding these two types of idealizations and the theories that result from them. One may think and argue that all idealizations are essentially of the first type, and if so, one may be more inclined towards antirealism regarding scientific theories; or, one may think and argue that all idealizations are essentially of the second type, and if so, one may be more inclined to realism regarding scientific theories; or, one may think and argue that there are two types of idealizations and they are fundamentally different, and if so, one's inclination towards realism or antirealism may have nothing to do with this.

I must admit that the notion of scientific realism in this problematic is a rather circumscribed one. A complaint can certainly be lodged against this discussion as being beside the point: how could the use of idealized theories be relevant to the question of

whether there exists a world out there whose goings-on are independent of how and whether they can be known to us? Hence, to avoid confusion, let me state as clearly as I can what I take to be the topic at issue regarding realism. I take it that both realists and antirealists agree that objects such as the sun and the moon exist independently of how they are known; and also they agree that ideal gas do not exist in the same sense. The dispute is rather about the existence of unobservables (which may or may not include idealized objects) and statements about them. Do they exist in the same sense as the sun or the moon exists? And are the truth-makers of statements about them facts that obtain independently of our conception of them?

## **II. Laymon's account**

I choose Laymon's account to begin our discussion of the confirmation of idealized scientific theories and let it serve as the central focus of my later critical assessment of the problematic not only because it is explicitly constructed to provide a confirmation theory that lends support to scientific realism under the kind of threats from idealization that I articulated above -- in other words, it provides a triangulation I mentioned above, nor only because it provides a connection between idealization and approximation on different elements in a theory testing model, but also because it, aside from being a bit outdated, still is the best account. I will in due time point out some insurmountable obstacles for it and therefore declare it untenable, but since I haven't seen any other account that can get around my objections, Laymon's may still be the most approximate account (no pun intended) for a confirmation theory that naturally supports scientific realism.

Laymon's project was a pretty ambitious one -- it was first announced in 1980, a time that such ambitious scheme was still possible -- and here is how he announces it.

My theses in stark unqualified form are these. First, explanations...consist of two parts: (1) an idealized or simplified deductive-nomological sketch...and (2) an auxiliary argument or set of arguments made showing that if the idealizations of the sketch are improved, i.e., made more exact or realistic, then the prediction of the idealized sketch will be correspondingly more exact and realistic. I shall refer to this second component as the *modal auxiliary*, since the argument purports to show that an improved idealized sketch is *possible*. My second thesis is that confirmation and disconfirmation occur in the realm of the modal auxiliaries. That is, a theory is confirmed if it can be shown that it is possible to improve its idealized sketches (Laymon 1980, p. 338).

On the realism connection of his account of confirmation, Laymon says,

I shall now consider the consequences of the above sort of *converging counterfactual* theory of confirmation for the issue of scientific realism. Realist and antirealist perhaps can agree on this methodological point: proceeds *as if* one were developing ever more accurate descriptions of an existing reality. Given this agreement, an argument *for* realism is that cases of successful convergence to better experimental fit are *miraculous coincidences* for the antirealist (Laymon, 1984, p. 118)

Laymon has had several later occasions to revise and enhance his account on the confirmation of idealized theories and its contribution to the support of scientific realism, and my summary below comes from this body of his work.

The general form of a scientific theory is: if a system were I, it would be T, where I is a set of ideal conditions and T the theory. For example, if a block moving on a plane were not subject to friction, it would move forever in a constant speed; or if a tank of gas were ideal gas, its thermal states, states described by values of the gas's pressure, volume, and temperature, would be determined by Boyle's law (Laymon 1984, 1989; cf. also Weston 1992). Ideal conditions that circumscribe the exact extent to which the theory can hold are not initial/boundary conditions, although the i/b conditions may also be idealized. Although Laymon sometime speaks about idealizing i/b conditions, I think it is better to keep them apart. By stipulation in the current discussion, if a theory is not idealized then it gives realistic predictions under physically possible i/b conditions; unrealistic predictions only come from idealized theories, or from the ideal conditions of such theories. It seems right to say that i/b conditions can also be idealized; but they are certainly not ideal conditions that produce idealized theories. It might be correct to say that the distinction between ideal conditions and (non-idealized) i/b conditions is that the latter are always physical possible while the former are not. This is the bit about the nature of idealization I would like to leave out of this talk.

As to the question of whether the theory, T, is simply false because of the ideal conditions, Laymon's answer, in an article in response to Cartwright's idea that laws of physics lie in the sense that they can only be true in the models that are made for them

and the models being idealized never exist in reality, is that the laws are true, but the models are false, and the two together do not make predictions, even when given true i/b conditions, that match the ‘experimental fit’ that is meant to confirm the theory (more on this later).

To the question of how an idealized theory is to be tested -- confirmed or disconfirmed -- the account proceeds as follows. Suppose that our theory,  $T$ , consists of law-statements which when supplied with initial and boundary conditions yield empirically testable predictions; and suppose again that  $T$  is idealized, which means that there are some set of ideal conditions (statements)  $I_d$  such that  $T$  is true only if provided with the ideal conditions. In other words, given the appropriate i/b conditions,  $T$  and  $I_d$  implies  $P$ , a prediction that can be tested in an experiment.

Suppose again there is a set of theories,  $\mathbf{T} = \{T^j \mid j = 1, 2, \dots, N\}$  where  $T \in \mathbf{T}$ ; and correspondingly, there is a set of ideal conditions  $\mathbf{I}_d = \{I_d^j \mid j = 1, 2, \dots, N\}$ , such that given the same set of i/b conditions,  $T^i$  and  $I_d^i$  yields  $P^i$ , where  $P^i \in \mathbf{P} = \{P^j \mid j = 1, 2, \dots, N\}$  is a testable result in the same experiment. This prediction-making scheme does not have to be understood as a simple H-D scheme, although it could be whenever that scheme is applicable in the case in question; otherwise, it represents any scheme of theory testing that goes from to-be-tested theory together with all the auxiliaries to the experimentally testable result. This is roughly what the deductive-nomological sketch, which appears above in the first quote of Laymon, amounts to. Next, let  $P^E$  be the observed datum that is obtained in the experiment that is designed to test  $T$ .  $P^E$  is almost certainly not the result of a single trial, but rather it is the result of some standard



statistical treatment of a set of trials that is well justified in the discipline in question; and it may or may not be a member of  $\mathbf{P}$ .

The upshot of Laymon's account for confirming idealized theories is to take confirmation as the improvability of approximation under idealization. In other words, an idealized theory is found to be confirmed if it is shown that relaxing its idealness (making it more realistic) improves upon the approximation of its predictions (towards  $P^E$ ). One way of making this more precise (as in what he calls 'having monotonicity' (Laymon 1985, pp. 213-4)) can be formulated as follows.

[L]  $T^k$ , for any  $1 \leq k \leq N$ , is confirmed if for any  $T^i$  and  $T^j$  in  $\mathbf{T}$ , where  $k < i < j \leq N$ , we have  $|P^j - P^E| < |P^i - P^E|$  (i.e.  $P^j$  is closer to  $P^E$  than  $P^i$  whenever  $T^i$  is less idealized than  $T^j$ ); otherwise,  $T^k$  is disconfirmed.

This version essentially says that  $\mathbf{P}$  must form a strictly ordered set that matches the same strict (descending) order in  $\mathbf{T}$ . And this is precisely what is meant for an idealized theory to *approximate approximation monotonically*. This version obviously seems a bit too strong; the requirement of monotonicity does not seem necessary. What one needs is something weaker, but as a consequence it becomes more difficult to formulate with complete rigor. One simple modification of [L] is to say that for  $T^k$  to be confirmed is for *some*  $T^i$  and  $T^j$  in  $\mathbf{T}$ , where  $k < i < j \leq N$ , we have  $|P^j - P^E| < |P^i - P^E|$ . But will this be too weak? It will depend on how  $\mathbf{Id}$  is constructed. Suppose we look at the ideal gas case. Two kinds of idealization are essential to the model: idealization on the size of the gas molecules and that of interaction forces among them. Suppose it turns out, Boyle's

law approbate approximation when the stipulation on molecule sizes is relaxed but it does nothing when interaction forces are brought back into the picture. Would we still regard Boyle's law as confirmed since it obviously satisfied the weaker version of [L]? Or perhaps we can simply use the notion of a limit to characterize it, namely, for any positive number,  $\epsilon$ , there is an  $N$  after which the difference between the  $N+1$  term of the prediction and  $P^E$  is equal or smaller than  $\epsilon$ . This does not require monotonicity, but it does assume that the improvement process forms a dense sequence.

There are problems with this account which I shall discuss shortly, but if we dismiss it as unrealistic because there may be actual cases of theory confirmation that do not fit it perfectly, we are doing philosophy of science a disservice. It has often happened in the history of philosophy of science, that certain formalized accounts of a practice in science, such as Hempel's D-N model of explanation, are rejected wholesale, namely together with the intuitions that ground the accounts in question, just because there are actual cases that do not fit into their formal details. I regard this as an unfortunate situation. In this aspect, scientists are much wiser than philosophers, although it sounds a very odd thing to say. If one compares many philosophers' treatment of Hempel's D-N model and physicists treatment of such laws as Boyle's ideal gas law, one may understand what I mean by what I just said.

From the second quote above we can glean that Laymon meant to use his account of confirmation to mount an inference to the best explanation argument for realism. [L] supports scientific realism because otherwise, it would be hard to understand why confirmation of an idealized theory should approbate approximation. If it is not a fact in a world whose goings-on are independent of us that an object moves in a straight line

with constant speed if isolated from all external influence, then how can we explain the fact, the observable fact, that by reducing the magnitude of influence the object will move closer and closer to a straight line and with constant speed? But wait, is this not the opposite of relaxing the ideal conditions? As I shall explain in a moment, this is not really a problem.

In connection to the realism issue, Laymon also raised two important points. First is the point that the realism/antirealism debate is really about theoretical entities. It is difficult to take the view seriously if it says that ever more accurate mechanical account of the earth movement in the solar system is not really about an existing earth but a theoretical construct. But if we replace 'the earth' with 'an electron' in the above, then the view cannot be so easily dismissed. And the second is an easily anticipated objection raised by Laymon himself. What if '[t]here were,...., increasingly more realistic theories of phlogiston' (Laymon 1984, p. 119)? I shall return to this point later.

### **III. Observations on Laymon's account**

I will not have space in this short paper to do a thorough critique of Laymon's account and articulate an alternative approach concerning idealization and approximation, on the latter of which I have done some work elsewhere. What I can do in the remaining space is to point out some immediate and small, albeit interesting enough, points concerning Laymon's account. But first perhaps some friendly amendments.

One thing that I want to say immediately after seeing this account of confirmation is that it must be just a part of an account; it cannot be the whole thing. A theory or law statement, whether idealized or not, has to be confirmed by a supply of actual pieces of

evidence. It can't just be about a set of modal auxiliaries that specifies counterfactually how the theory would behave when the idealization is relaxed. Therefore, what is sketched above is the extra portion of a confirmation theory that deals exclusively with idealized theories or law statements. It is an indispensable part because we have good reason to believe that idealization, in some form or shape and ranging from nearly trivial to highly controversial, is necessary to any theory construction. But when this part is settled, or apart from settling this part of confirming an idealized theory, a method of confirming a chosen theory has to be adopted and applied, for instance, in a lab so that the scientists know when to accept or reject the theory.

Further, this part does not have to be conceived as entirely comprising modal auxiliaries, although Laymon is right to insist that it is sufficient to have the possibility of approximation approbation established. An idealized theory or law statement is 'confirmed' in this part of confirmation if the relaxation of the ideal conditions is conceivable and the approximation approbation is demonstrable under that conception. However, the opposite direction often serves as the objective for theory testing. Although they may in principle be unrealizable in a lab, ideal conditions in many cases can certainly be approximated and they often are with successful results. Perhaps it is already implied in Laymon's account, for it hardly needs any originality to see it, but we can certainly imagine situations in which experimental scientists pursue a strategy of inching ever closer to the ideal conditions and see if results approach the ideal prediction monotonically or asymptotically.

It might not be unfruitful to borrow the term, 'direction of fit,' to illustrate this point. Laymon's account only looks at one direction of fit in the business of confirmation,

namely, the desire to fit one's belief to what is already experienced of the external world; while there is the possibility of pursuing the opposite direction, namely, the desire to fit the world to one's belief by way of arranging one's environment so that one experiences results that monotonically or asymptotically approach what one firmly believes. This latter point lends support, as we will see in a moment, to the idea, to which Laymon has partially signed on, that idealized theories may be true. More on this later.

Laymon's point that an idealized molecule or electron may, but an idealized earth does not, threaten realism is significant but incomplete. A host of questions arise from this point. I said in the first section above that there are at least two types of idealized objects that are separated as much by the attitudes their users regard them as by their own features. One type results in theories or laws, such as Boyle's law, that no one would regard as true of nature, but the other type results in theories or laws, such as the first law of Newtonian mechanics, that some scientists at least regard as true of nature. Is this connected to Laymon's point? I am tempted to assert that those 'theories' or 'law-statements' that everybody knows to be false are results of physically impossible ideal conditions. Molecules can't be without size and (actual) electrons can't be all identical, where 'can not' means physically impossible. But completely isolated objects or systems may not be actual but are nonetheless physically possible. Secondly, a mechanical theory of an idealized earth does not seem to be so threatening to realism as a mechanical theory of electrons (presumably necessarily idealized) does, but Laymon did not explain why this is so. Here I can only offer a simple remark. What the realist can claim in the case of a mechanical theory of an idealized earth is not that the idealized earth of which the theory is exactly true is real; no, it is just as unreal as the idealized electron or anything

else; but the earth we live on, the actual earth of which the theory is only approximately true is real. The need for idealizing the earth, or anything else that we can directly interact, is that otherwise we can only either say something general and vague about it and have a chance to say something true or say something exact about it and have no chance of saying anything true. This does not seem to be the case at all for electrons or other theoretical objects. We may wonder with some reason whether electrons under our current theory are an idealization. Do we know what the ideal conditions are for electrons? Because of this, the realist may be hard put to answer the question: what exactly is the thing that is supposed to be real, or that is supposed to exist independently of our conception of it? Given how underdetermined from a philosophical point of view our models of the unobservables are, it is difficult indeed to argue that the creatures that are responsible for all the observable results attributable to electrons are really electrons in any particular model. But we are now walking over well treaded ground in the war between realism and antirealism; therefore I don't expect to be able to say anything illuminating that is not already said many times over in the literature.

The distinction between observables and unobservables has been a controversial issue in philosophy of science. Some argue that the distinction, though useful on many occasions, has no philosophical grounds, but some argue the opposite. Even among those who believe in the distinction, they do not know how, or think it's impossible, to argue for it. Van Fraassen has an argument for the distinction in terms of possible direct observations. But it seems here is another argument for the distinction. Observables are idealized objects whose originals we have confidence to identify. We have a good idea of why and how the idealized observables in our theory got there. This is not true at all

with the unobservables. We can't even be sure which features of the unobservables are the results of idealization and which are not. For instance, we have identical elementary particles in our theory. Is it an idealization or is it a real feature of the particles?

Let me explain an unexpected benefit of Laymon's account before we turn to its problems. If we think of idealization as an act that neglect the negligible in theory construction, Laymon's modal account matches well with an approach in the philosophical literature on approximate truth. Risto Hilpinen (1976) characterizes an approximately true proposition as one that is true in some possible (different from the actual) world that is *close* to the actual world, where the sense of closeness is essentially a Lewisian notion. One can certainly imagine a metric and a cutoff point in some formal sense that supply a criterion that separates approximate propositions from not even approximate ones. Complications aside, one can in general view good idealized law-statements as expressing propositions that are (at least) approximately true and bad ones propositions that are not even approximately true. I assume that the reverse is not true for obvious reasons.

There is a problem for Hilpinen's conception of approximate truth, namely, when a proposition is about some theoretical objects, how is it possible to even in principle come up with a measure of closeness between the world in which the approximate proposition is true and the actual world. Since it is by stipulation not possible to know what molecules or electrons are *actually* like, how do we know whether the idealized ones are close to the actual ones or not? I think Laymon's account at least goes in some ways towards answering this question. Of course we don't know and will never directly know what electrons are like. But if we have a model for them and this model approximates

approximations in the observable predictions a theory of the model yields, we say that the possible worlds in which electrons are like the ones in our model is closer to the actual world than the ones in which electrons are different. Again, the radical underdetermination thesis threatens this realist solution, since it assumes that what we do in coming up with an ideal model of electrons is essentially the same as what we do in coming up with an ideal model of the moon.

Now, some problems for Laymon's account. Again, if idealized theories or law statements are false, what is the sense in which it can be said to be confirmed? Doesn't the notion of confirming something simply means confirming that it is true? If like most people who, when confronted with this question, say that confirming something really means confirming that it is true or approximate true, where the latter means 'true enough,' why do we need Laymon's account then? This sensible but too naïve an answer to the question won't do for all idealized theories. In fact, if the theories or law statements are obviously true enough in the sense that the deviation is negligible, then the idealization is necessarily so trivial that it is often not even mentioned. In ordinary circumstances, the difference between non-relativistic mechanics and relativistic mechanics is so small, whatever confirms the one confirms the other. In other words, the confirming evidence in such circumstances is not likely to be sensitive enough to distinguish the two. In this case, we don't even consider the non-relativistic mechanics as an idealized theory. But frictionless planes or regions without gravity are not just neglecting the negligible, and such assumptions will certainly create discrepancies in the testing results. In such cases, what does it mean to confirm idealized theories that are false? But are statements about frictionless objects or fields in regions without gravity necessarily false statements?



Cartwright (1983) thinks they are indeed such, but Laymon thinks that some of them are not, and I agree with Laymon.

What about law statements such as Boyle's law, which we should all agree are not true in any sense of being true. The best way to regard statements of this sort is to think of them not as law-statement but as 'law-maps,' if the phrase is understood properly. It is certainly not false, as a good map of a city is not false, but it is not true either, as the lines on it with street names attached to them are not true of the streets in the city that bear those names. A good map contains true statements about the streets and buildings in the city it represents; it is their spatial relations and orientations, etc. that must be stated correctly for a map to contain such statements. And the notion of confirming a map, or one map being better confirmed than another, is not hard to define either. Some philosophers want to argue that all scientific theories are like maps. I think the idea is not so much wrong as uninteresting or non-illuminating. One ends up, in arguing for such a sweeping claim, struggling to find a sense in which the analogy fits every case, but by then a map in that sense is so different from any maps we know that the analogy loses its grip on our imagination. Law-statements such as Boyle's law are like maps in the most straightforward sense.

Therefore, harking back to the insight of the two opposite directions of fit I mentioned earlier, one can be a realist and maintain that a clear distinction between the two types of idealization can be found, and then claim that theories involving one kind can be true in the correspondence sense of being true and theories involving the other kind can never be true; and such theories are not real theories but rather theoretical maps that hopefully lead to real theories (which only involve idealization of the previous kind).

Let us call the theories or lawlike statements of the first kind ‘theories’ or ‘lawlike statements’, and theories or lawlike statements of the second kind ‘theomaps’ or ‘lawmaps’. When confirmation is considered regarding these two types of things, opposite attitudes should be adopted. For theories and lawlike statements, the experimental effort should primarily be given to approximate the conditions under which a theory or lawlike statement in question is literally true. In our case for the law of inertia, approximation may simply mean getting rid of the friction as much as possible. For theomaps or lawmaps, we do the opposite: we relax as much as possible the ideal stipulations in them so that new theories, new idealized theories that have a chance to be true and produce predications that can be made to approximate results in possible experimental conditions. Confirmation in a qualitative sense is achieved in either case when a qualitative convergence is detectable with either approximation process.

Keeping this distinction in mind, let us now examine Laymon's IBE argument for realism. The argument won't work if idealization is no more than what Laymon wanted, namely, just to approximate approximation in that when the ideal conditions are reduced or made successively closer to the actual situation, the predictions will monotonically or asymptotically approach the observed result. It appears to work well for such cases as an idealized earth or any other planets in Newtonian mechanics. In fact, in any cases where what is idealized is an object that direct or semi-direct observations give us its 'true' self. In such cases, what Laymon's argument concludes is actually this: the object of which we, for the sake of theoretical expediency, makes an idealization must be a real object, must be something that exists independent of us, of our theory. So the earth can't be said not to really exist just because its idealized copy in Newtonian mechanics happen to have unreal

aspects. On the contrary, if the real earth does not exist, it wouldn't make much sense to call the earth in Newtonian mechanics an idealization, would it? This bit of Laymon's reasoning seems harmless enough. But scientific realism claims much more than that. If the earth really exists, then there is no reason to think that molecules or electrons do not, for the same reason; and there is also no reason to think that frictionless objects are not real, and there is no reason to think that the weak and the strong interactional forces do not actually operate among elementary particles. Can our notion of an approximation approximating idealization be compatible with realism understood in this sense?

To repeat what I said above. The earth really exists, but what exists is the earth, not the Newtonian earth, which is nothing but an idealized copy, a fantasy, an idea, no more. But when we say that a frictionless object exists, what do we mean? Suppose we replicate Galileo's incline experiment, in which a block is sliding down an incline and then continues its motion on a level plane. We reduce the friction among all parts and every time we reduce a bit more the block moves more smoothly while moving and stops further away from the incline. When we conclude with an idealization that if the friction is *par impossibili* completely removed, the block will continue to move with a constant speed forever, what do we claim to be real? It looks like we have to be saying that the frictionless block is real. In other words, the idealized block rather than the actual one is real. But that is not what the above extension of the earth/Newtonian earth case is telling us. If a frictionless object can be real, why can't a dimensionless molecule be real? If so, we arrive at the rather bizarre argument: because the van de Waals modification of the ideal gas law is empirically successful, the ideal gas is real and Boyle's law is a true law-statement. Obvious this is not right. The way out is to put such idealized objects as

dimensionless molecules with the Newtonian earth rather than with frictionless blocks. So, what is real are molecules in real gases of which the dimensionless molecules in an ideal gas are but mental constructs. But then how about electrons and other elementary particles?

Here are several things regarding the realism-antirealism debate that we can already clarify. The contention between scientific realism and antirealism is not about whether objects we can plainly see exist independent of us. No antirealists in this debate I hope would be such an idealist as to say that the earth we live on are but an idea or such an empiricist as to say that the solar system didn't exist before experience can be had about it or the question of whether it existed before it is experienced is a meaningless question. The question is really whether the objects that are talked about in scientific theories do exist in nature. Since the objects are idealized in our theories, we need to be sure which objects we mean when we try to figure out whether to believe in realism or antirealism. Are we talking about the idealized objects or are we talking about the objects from which an idealization is extracted? Two types of idealization need to be distinguished: one type ignores what objects could not exist or be real without and the other ignores what objects could exist or be real without but do in fact possess. This is another, or more general, way of characterizing the same two types of idealization I discussed above. For the former type, the realists and the antirealists should agree that the idealized objects cannot be real while the objects from which idealization is made do exist; but for the latter type, the realists argue that what is real this time are the idealized objects, while those objects from which idealization is made are also real but beside the point. The antirealists disagree and argue that the ontological status of the idealized

objects should be the same as in the former type, while those objects from which idealization is made are, just as the realists say, real but beside the point.

The antirealists now have two arguments against realism in this context. First, and this is the one Laymon himself anticipated, the antirealists could ask: what could the realists say about those theoretical objects that were tossed out to the wayside in the history of science, objects such as phlogistons (Laymon 1984, p. 119)? There could well be a theory of phlogiston that approbate approximation, and yet there aren't any such objects. Second, from the above it seems that all the realists can and want to argue for the reality of objects is that the objects from which idealizations are made and to which terms in our theories refer exist or are real. But if so, it is not even clear what the realists can say are real in the case of theoretical/unobservable objects. Do they mean to say that they don't believe that electrons -- the stuff that are talked about in quantum electrodynamics - - are real because they are idealized objects, rather they believe as real whatever out there of which electrons are an idealization? But the latter is not substantive enough for someone who claims it to be a realist, the same way Kant's granting the existence of the noumena doesn't make him a realist. The realists need to say, for instance, what it is of which electrons in QED are idealized copies.

Laymon's response to the first problem on behalf of realism (one of the several he thinks are available to the realists) is simple and in my opinion quite effective. The approximation approbation scheme only works within a paradigm or regime; whenever there is paradigm or regime change and, for instance, the phlogiston theory of combustion is replaced by the oxygen theory, the latter is able to explain why the former is approximately true and capable of approbate approximation in a limited and out-of-

date scope. The antirealists would have a hard time explain why such an explanation is always available for the later theory that replaced an earlier one. But of course the realists do not have the last word on this, since there is necessary to the claim that later theory of a certain phenomenon always explains why the earlier theory works. Can a later mechanical theory of lightening and thunder explain why an earlier anthropomorphic theory of them works? A possible response to the second problem available for Laymon is perhaps to bite the bullet and say that theories such as QED of electrons are true theories, or at least not made false because of idealizations it uses.<sup>2</sup> Hence, electrons are like totally isolated systems or electromagnetic fields, whose theories are true despite of a certain amount of idealization to get them out of the hopelessly tangled actual world.

Third, I think a real problem with Laymon's version of the IBE argument is this: which of many possible idealizations regarding a given target is the right one is in principle underdetermined by the convergence of approximation approbation, if the latter is understood in terms of factual closeness. To all practical purposes, there is no difference between the law of inertia given by Newton and an alternative law that says if all influences are removed, the object will move in a speed with a damping factor of  $e^{-\lambda t}$ <sup>30</sup> or one can make the difference between perpetually uniform motion and this damped motion as small as any finite number. But as I mentioned a couple of time already, such sweeping underdetermination argument against realism has been launched many times

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<sup>2</sup> Newton's first law is no longer true; but it is so not because of the ideal conditions it stipulates, namely, the completely isolated systems, but because of some other assumptions, such as assuming that there is no upper limit for the transmission of causal influences. So, QED may well be only approximately true because of some hidden or not so hidden assumptions of the scope of its applicability, not because of its idealizing the electrons or other charged quantum particles.

before this paper in many different guises. I may have some opinions of how to get around this, but I haven't yet got any good arguments.

#### **IV. Conclusion**

What I have argued in this paper, in stark unqualified form, is the following: (i) Laymon's account is good in general in that it obtains a stable triangulation of approximation/idealization, confirmation, and scientific realism that is at least in the ballpark. (ii) To use the account effectively, two different types of idealization must be clearly distinguished, namely, the idealization to get real but perhaps non-actual physical systems, and the idealization to get unreal systems. The distinction is as much a matter of attitude as is a matter of logic. The unreal systems that idealization creates are regarded as such by scientists and/or are physically impossible. (iii) With the first type, confirmation is achieved when ideal conditions are asymptotically approached by experiments, and with the second type, confirmation is achieved when a relaxation of the ideal conditions produces an asymptotic approximation of predictions. (iv) Laymon's IBE argument for realism is most problematic when it concerns unobservable entities, first because it is not clear how a relaxation of ideal conditions can be achieved and secondly because it is not clear which -- the ideal objects or whatever the ideal objects are models for -- are claimed to be real. Many of the points in this paper received indirectly hints from Sklar (2000, ch.3), whose discussion however is in a more general context.

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