Chapter 17
Letters to Melba Phillips, 1952

Letter 34. Folder C48, dated: Jan 15 [1952]

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Dear Melba

I've been waiting to answer your last letter until my ideas are more definite on certain subjects. First, the article that I sent you will be published in "Nature". Massey wrote me at last, saying that he had sent it in. Nevertheless, it might be worthwhile to publish something a little more popular and more extensive in Scientific American. Perhaps you can sound out the editors, using the articles I sent you as a base.

I have made much progress on plasma theory and on the causal interpretation of qu. mech. Interestingly enough, the two are now very closely related. I have become convinced that the time has come to reconsider the concept of an "ether" that fills all space. For in the causal interpretation of the quantum-mechanics, the negative energy electrons, protons, and neutrons are all supposed to be really there, forming a medium of incredible density. For this reason, the word "ether" is bad, as the medium is anything but "ethereal". A better word would be "substratum".

As a matter of fact, there never has been a proof of the non - existence of the "ether". The exp'ts leading to relativity merely showed that if the laws of relativity are strictly true, our velocity relative to the ether cannot be observed. But from this we cannot deduce that the ether does not exist. All we can deduce is that at a certain level, it should be possible to formulate the laws of physics abstractly without reference to the "ether". But since it is always conceivable that in some domain, the laws of relativity may fail, it is always possible that the ether not only exists, but is also observable, by methods not yet known to us (note the similarity to the problem of interpreting the quantum theory).

C. Talbot (ed.), *David Bohm: Causality and Chance, Letters to Three Women*,
DOI 10.1007/978-3-319-55492-1_17

The ideas I have been led to are as follows: Matter at our level is an inhomogeneity in the substratum (a stable and highly localized one). Light waves, etc., are other possible types of inhomogeneity. The "disappearance" of matter at our level is simply a transformation in the substratum, in which energy changes its form from localized to a spreading wave (and vice versa). Of course, the substratum particles are not indestructible either, but are structures in a sub-substratum, etc., and so on ad infinitum. But for the present, we can regard them as approximately permanent. When we look at space, it appears to be empty, because a uniform background does not scatter light, or electrons, protons, or planets, etc. A similar situation arises in the theory of metals, where a perfectly regular array of ions does not scatter an electron. Thus, a positivist might be tempted to conclude that a metal consists of empty space, if he had only evidence from the scattering of low energy electrons.

In studying plasma theory, I have found that each particle is surrounded by a cloud of charge of elliptical shape, which shortens in the direction of motion, as the particle approaches the velocity of waves in that medium. When the particle exceeds the speed of the waves, it throws off a "wake" (as in the Cerenkov effect) and loses energy rapidly, until it falls bellow the speed of the wave. Now, I can conceive of a substratum theory along these lines, in which a "particle" accelerated slowly (adiabatically) would shorten in the direction of motion according to the formula of Einstein, and would get heavier, because its associated "cloud" became more and more intense. Thus, in processes of this kind, the particle could not be accelerated faster than light, as it would get infinitely heavy. But if one of the substratum particles were given a violent acceleration that pulled it right out of its "cloud", it could go at theoretically unlimited speeds, except that, because of the "Cerenkov effect" it would throw off energy and slow down very rapidly until it fell below the speed of light. Let us imagine that processes at our level and at the atomic level are too slow for such phenomena to appear, but that at the level of "creation" of elementary particles, processes are fast enough for this to happen. We should then have a theory that is "relativistic" in every domain where the theory of relativity has been tested, but non-relativistic in those domains where the present theory is known to fail.

I should also add that it is possible to put a purely classical plasma theory into a form which is remarkably close to that of the quantum theory, as causally interpreted. I therefore think that it is not impossible that by means of suitable assumptions about the laws prevailing in the substratum, we might obtain both relativity and quantum-mechanics as approximations holding at atomic and higher levels. Also, there is some possibility of bringing in gravitation, but I prefer not to discuss that yet. Roughly speaking, the idea is that "matter" is (as in general relativity) a disturbance that is localized in a small region, but actually spreads out over all space, in a steadily weakening form (by the inverse square law). Thus, in a sense, everything that we call "matter" (i.e., the inhomogeneities in the substratum) are really in contact, and "interaction" of the central points is our approximate way of describing the effects of this contact. We might make a picture of a forest, where each tree seems to be separate, whereas in the soil underneath, the roots intertwine, so that the forest is in a sense a single thing. But the interconnections are (as in the substratum) at a level that is not "observable" by readily available methods. Thus, a naive observer could

see one tree affecting another, and he could postulate an "interaction", inversely proportional to some power of the distance, because as the roots of a given tree spread out, they grow less dense.

Well, as you can see, I am enjoying myself, scientifically speaking. It's nice to be off by oneself for a while, away from people who prevent the free use of one's imagination. Actually, I find that the pieces are slowly fitting together, and each day, some aspect of the problem becomes clearer.

Regards to all,

Dave

Letter 35. Folder C47, not dated.

[Early 1952].

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Dear Melba

I have just heard from Bernard that O. is going to be called before some committee again. Perhaps he'll be able to avoid it again this time - perhaps not. Let's hope for the best. But I am curious about the status of the immunity statutes. Have you heard anything about them yet, or other significant items of information? I am a little worried because no one in the States has written for over two weeks. You might try writing your next letter for a change to Brasilio Machado 380, Apto. 703, and see of this helps speed up the delivery. I am also curious to know how people react to my paper. (This should be published in Jan Phys. Rev.) I have a few hints so far. Von Neumann thinks my work correct, and even "elegant", but he expects difficulties in extending it to spin. The older Bohr didn't say much to Art Wightman but told him he thought it "very foolish". The younger Bohr thinks it logically consistent, but "something like an absolute frame in relativity", but thinks if "I could find some modification significant in domain of 10^{-13} cm, it would be wonderful". De Broglie is now fairly friendly to me, saying in a letter that I have carried the pilot wave theory much further than he did in 1927. You know of course, what Pauli's reactions are. Rosenfeld thinks the theory very "ingenious", but "basically wrong". However, he seems, according to my informant, to be remarkably vague on just what is wrong with it.

I must say that events worry me, not only with regard to my own special problems, but because it seems hard to see how war can be avoided. Of course, my information here is bad, because the papers are atrociously prejudiced, and I hope that my present very depressed feelings are unfounded. Sometimes I feel as if "the game is up" for everyone living in this generation, and this largely because of atomic power, which seemed such a bright dream when I was a child. Now I have dreams of new levels of reality more fundamental than the nuclear particles, which may help lead to concepts

and ways of thinking that will compensate somewhat for the damage done by the atomic bomb. But this is so far in the future that it no longer seems so significant to me, now that the threat of catastrophe is really here. I have a great confidence in the ultimate future, but no confidence whatever in the next 20 years. This will be a disheartening time, when the decay prepared during the last 50 years will finally manifest itself openly, in the form of everything going to pieces. Personal relations, careers, children, or whatever else people live for, will become meaningless in a disintegrating society. It seems hard now to hold on to the idea that a concept of things not yet known, such as I am working on, is worth pursuing, but I suppose that it is.

I hope I haven't bored you with all this depressing talk. I'll probably get out of this mood soon. By the way, did you ever give that talk to your students on causality in the quantum theory, and if so, how did it go? With regard to the talk on the crisis in physics, I now feel that it would be very difficult to present a good case, without a really exhaustive and rather frank analysis, connecting with all sorts of factors, political, social, etc. What I could do would be to present a much more limited case, in favour of a "realistic" point of view in science, as the most fruitful in the long run. But even this could be strongly attacked, because as Pauli said "I am cashing in on future dividends" when I maintain that this method is likely to be helpful in the domain of 10^{-13} cm. The whole thesis is right, but positivist confusion has spread so far in our science that it is remarkably difficult to carry the argument to that large majority, which without realizing it, has come to regard the positivist point of view inevitable. I can make an impression on a person after 3 or 4 hours of discussion, but a short paper would have little effect. There are an infinite number of twists to the positivist argument, and each person must be reached through the peculiar twist that he has absorbed.

I don't like to repeat it, but I wish I could shake off this awful feeling of foreboding and depression. I am hoping for some letters from old friends in the States, which are really more helpful than you would think, not only because it is good to hear from them, but also because it gives me an idea of what is going on behind the curtain of the "free press".

Incidentally, I have a job offer from Mott in England, and could probably obtain offers in other countries if I really wanted them. The passport situation doesn't look so good, however. I am looking into methods of getting the passport back, by being invited to England, but if they spread O. over the front pages, I'll be lucky if I am not called back myself. I am praying that my unimportance + inconspicuousness will come to my rescue. To feel unimportant & ineffective is an annoying thing, but it sometimes has its advantages (I hope, this is one of the times).

Coming back to the ubiquitous positivist point of view, I find that Brazil is full of positivists. Relativity is the culprit here, as no one here understands quantum theory

well enough to be confused by it.[1] People have the idea that by reducing everything to a study of the role of the observer, Einstein got the whole theory of relativity, and that this is the best way to proceed in science from now on. This also ties in with the disapproving attitude to manual labour characteristic of Latin countries; for people feel that math equations deduced by an analysis of the role of the observer, are the basis of physics. The flexibility of positivism is amazing, for among exp'tal physicists in U.S., there is a belief that physics flows solely from empirically observed data, or "operations", which is also combined with a belief that theorists take these numbers, and with the aid of a few geniuses like Dirac, produce equations that fit these numbers.

Give my regards to all

Love
Dave

Letter 36. Folder C46, not dated.

[April, 1952].

Tuesday

Dear Melba

Well - all hell is ready to break loose. The two dep't stinkers (Prof. Marcelo Damy de Souza Santos and Prof. Stammreich) have joined forces with the local Nazi group. I am dismissing my "assistant" whose father organized the Institute of Theoretical Physics. V. Wiseacre [Bohm's name for von Weizsäcker – CT] has discovered my troubles in the States, and knows I don't have a passport. Now, the skunks are trying to blackmail me, saying that if I don't take Schiller, but take two German boys instead, all will be well. Otherwise, they will ask for army intervention. Everyone here says it is a bluff, and we are going to call it. Meanwhile, a long and bitter fight is in the offing. It will be a fight to the finish, and if we lose, science in Brazil is done for. The rest of the dep't and many people in Rio realize it, and are getting ready to stake their careers on this fight. Also, the director of the Faculdade + a powerful faction are on my side, as they hate the 2 dep't stinkers and the Nazis. So we'll see what happens.

I would like to find some way to let the world know what a skunk von Weissacre is, or at least, let all physicists know. If you have ideas, let me know. We are only 90% sure of v. W's role in this, not 100%, but we'll soon be sure. Perhaps Phil Morrison has some ideas. In any case, you should let Phil know what is happening. I have already let Einstein know, by way of Shenstone. Who else should hear of it? It might make a good story "Nazis taking over Brazilian Physics".

[1] For example, Tiomno expressed surprise when I pointed out to him that q. theory as usually interpreted, implies a subjective point of view, since all is finally referred to the consciousness of an observer. However, he had often wondered how q. t. treated this problem and readily agreed that the ultimate implications of usual point of view are nonsensical.

Schiller's case comes up in the Congregação of the Faculdade Saturday. This is a crucial test, but we can probably win it. But this will be hardly the beginning. Our only solution is to discredit the 2 stinkers (Everybody knows that they are incompetent, and that at least one steals dep't money) and to discredit the Nazis. Either they will be smashed, or us. There is no choice. Meanwhile, they go around saying that "v. Weissacre is a genius" every time he says "$F = ma$", and they are clearly getting ready to say that the causal interpretation of the quantum theory is nonsense. I have asked Einstein for some statements on this, which we can publish, if necessary. I will have to refuse to hire the German boys on with a public statement that I don't think they are suitable. Then the 2 stinkers will bring in their great Nazi genius, v. W. (or Heisenberg), and ask how I can disagree with him. I will just have to say "He is a competent physicist, but no genius, and that is that". Well, here we go. Where we will end up, who can tell?

Please, let me know what is happening at your end. I hear that you are under attack too. I hope it is nothing serious.

Regards to all

Love
Dave

Letter 37. Folder C47, not dated.

[April, 1952].

Universidade de São Paulo
Faculdade de Filosofia, Ciências e Letras

Dear Melba

If I haven't been writing much to you lately, it's because I've been very busy. First, there is the chaotically complicated situation developing here. There are two jokers, Prof. Souza Santos, and Prof. Stammreich, who are throwing monkey wrenches into everything that they can. Two years ago, they nearly wrecked the physics dep't, and they were put in their places by the Director of the Faculdade, but now they are up to their old tricks again. Souza Santos is almost on the verge of paranoia, suspecting everyone of being against him, and as a result opposes almost every move that anyone makes, seeing in it only a move against him. Stammreich is a professional pessimist, believing that nothing can ever be done in Brasil. Nevertheless, there is some pretty good evidence that he made a good thing out of his job, for he arrived in Brasil penniless (excuse my tendency to Portuguese spelling) and is now quite rich. But nobody has ever been able to really prove anything against him. In any case dep't meetings with these two characters are a big waste of time, as nothing can be accomplished. I am in a tough spot, as the dep't has only 4 professors, and one is on leave, leaving behind an assistant, who is on my side.

A typical sort of trick engaged in by these characters is to get various functionaries to lose contracts and important documents, thus delaying things for many months.

The number of mislaid contracts is really impressive. That is why I'll be sure of Schiller's contract only when I see it.

But these difficulties pale into insignificance compared with those that seem to be looming on the horizon. Recently, an assistant of mine, whom I inherited, who is very wealthy and has Fascist leanings, succeeded in getting about $200,000 from the state gov't and $800,000 from private sources to finance an Institute for Theoretical Physics. This fellow's military connections seem to have been responsible for his success in raising money. To give you an idea of his history, when he was a student here, he was able to go to the gov't, and ask the army to take over the physics dep't here, because "it had so many communists in it". He created a serious threat that was stopped only by the action of the professors. Anyway, now they have gotten hold of von Weissacker, (who is here with a few German students right now) to head the Institute, alternating every 3 months with Heisenberg. The two will commute apparently from here to Germany or Chicago, or wherever it is that they now are. The physics dep't generally resents them, but they have so much money, political influence and big names that they will inevitably exert much power in physics here. And now I have to decide whether or not to fire my assistant, since he is not supposed to have two jobs at once. As yet, we are on overtly friendly terms, but the tension in our relations is quite obvious.

In Rio, meanwhile, your friend, the Admiral Alvaro Roberto has come back from the U.S. with plans to build a pile. This will put the military in control of a very large section of Brasilian physics research.

On the top of all this, I am waiting until one of these characters discovers my history in the States. Tiomno warned me against talking about it, as there are too many people around here who would seize on it to cause trouble. But if I get into the inevitable fight that is brewing on two fronts at once, it is quite possible that the whole situation will become really ugly.

Meanwhile, I am working on many things. The ether theory is quite a bit more developed, but I have set it aside for a while to write a paper showing that if, in the causal interpretation of quantum theory, you start with an arbitrary probability distribution, $P(X)$, you will ultimately get $P(X) = |\psi(X)|^2$ as a result of chaotic collisions with atoms, molecules, etc., undergoing random thermal motion. Thus, quantum probabilities have the same origin as those of classical statistical mechanics. This will nail down the last point in the theory.

I have received reports of a seminar given by Pauli + de Broglie, in which they make some rather childish objections to my theory, calling it "metaphysical". I hope they publish this nonsense, because if they do, I can really do a good job of tearing them to pieces, I can hardly wait!

I heard from Schiller that Phil Morrison agrees with my philosophy, but prefers the Bohr interpretation, "because it is simpler". This inconsistency, (which Schiller also remarked on) amazes me, because Phil ought to know that Bohr's interpretation is "simpler" only because it does not attempt to explain what is happening at the quantum level. In any case, it is not really "simpler" as I can vouch for after teaching it for several years. In practice, it is an exceptionally complicated and difficult point of view, compared with that of the causal interpretation. This type of inconsistency in

Phil disturbs me. He should be helping me, instead of raising irrelevant obstacles. Or at worst, he should do nothing. But to aid in the creation of confusion is something he ought not to do. Sometimes I have a feeling (knowing him only a little, however) that he does this sort of thing to create a greater feeling of friendliness with his colleagues. Of course, I realize that he is making serious enemies in the political field, and I admire his courage in this regard, but from my observations of his speech, mannerisms, etc., I often got the idea that he tries to compensate for this a little in the scientific field, by being very moderate in all of his opinions, and tending to agree with the majority. I have found that such impressions are often fairly accurate, because quite often after speaking with a person, I have a vague feeling of distrust - that something is not quite what it seems to be - and subsequent experience shows that it is justified. Some of your remarks about his having swallowed "Oppenheimeristic" ideas of a fellowship of scientists who would be responsible for the wise use of atomic power, would tend to confirm these impressions. In any case, I find it difficult to believe that a person of Phil's calibre can honestly be that confused.

I have received an encouraging number of favourable comments on my papers (about 15, at least) as well as 30 requests for reprints. However, since both Pauli + Phil Morrison make the argument that my interpretation is more "complicated" than it needs to be, I'd like to summarize my objections to their arguments.

The crux of the problem is in the statement of the usual interpretation that two systems having the same wave function are "physically identical". This follows because the w. f. is said to define all physically significant properties. Thus two uranium atoms in the same quantum state are said to be "physically identical" even though one of them may explode tomorrow, and the other in two billion years. But these atoms might be each connected to an atom bomb in such a way that the bomb would go off when the uranium atom disintegrated. The usual interpretation then asserts that the two atom bombs are "physically identical".

How does the usual interpretation then explain the obvious fact that they behave differently. Different physicists adopt one of 2 possible points of view:
(a) They can assume that the relation of past and future is arbitrary. Then two "physically identical" things need not behave in the same way. But in my point of view, the two uranium atoms are not "physically identical" because each of them has a particle in it in a position that will determine when it will disintegrate. Thus, in my point of view, I make a hypothesis, namely that there exists an as yet unobserved particle, but with this hypothesis, I can unite things that were previously arbitrary. But this is one of the criteria for a good hypothesis; viz., "Does it relate things that were previously unrelated?" The usual interpretation may be "simpler" in the sense that it makes less hypotheses, but it achieves "simplicity" only by leaving a large number of things arbitrary. We could always achieve simplicity in this trivial way, but only by giving up one of the most fundamental methods of making progress in science.

(b) One can take Bohr's point of view that if two systems with the same w.f.'s are "physically identical", then the obvious difference between them must be associated with something "unphysical" (i.e., not subject to physical investigation). This difference is what Bohr has called the "identity" of an event, a property not subject to

physical investigation, because in the usual interpretation, only statistical properties can be studied in physics. The property of "identity" is therefore intrinsically undefinable - it is connected with real physical events, but it is itself forever beyond the domain of physics.

To this point of view, one can raise the objection that it is certainly the purest possible form of metaphysics. A person who holds it cannot with consistency maintain that he objects to the causal interpretation because it is "metaphysical". For in the causal interpretation, the hidden variables are at least observable in principle, whereas Bohr's "individuality" is unobservable as a matter of principle.

I would like to suggest criteria for theories to replace the positivist criterion that only those elements that are already in principle observable can have physical significance. Instead I would suggest the following:

(1) In order for a hypothesis to be admissible, it is necessary that there be no inconsistency in supposing that every element appearing in it has been observed. However, the hypothesis itself does not have to include within it a method that permits the observation of every element. Thus, it is permissible to suppose that some of the elements may later be observed with the aid of interactions or processes that are not yet known. This is exactly what was done with the atomic hypothesis, and it is what is done in the causal interpretation of q. t. To refuse to admit such hypothesis is to cripple the imagination, when the time comes to investigate new fields, and to confine ourselves to the existing domain of concepts. (It is clear that Bohr's point of view does not satisfy this criterion, for Bohr is compelled to admit the existence of systems, having an "individuality" whose observation would be inconsistent.)

(2) Now, in order to prevent the multiplication of hypotheses which might satisfy (1), we also require that every hypothesis whose elements are not yet observable be required to unite facts or domains of facts which had hitherto been arbitrary. (This, as I have shown, the causal interpretation does.) Experience shows that such a hypothesis has a high probability of being correct, and therefore of being fruitful.

The application of these two criteria is almost instinctive for most people who have not been confused by a positivist philosophy, but the positivists have created so much confusion that all sorts of nonsense can get by. The principal job in science now is to remove this confusion.

Well, I guess that's all I have time for now. Regards to all.

Love
Dave

P.S. If you see Phil could you please discuss these points with him. As for the cosmic rays job, I got answers from a number of people, but the only one who wants to come now is John Tinlot of Rochester. Do you know anything about him?

Letter 38. Folder C47, dated: June 28, 1952.

Dear Melba,

I must have failed to get one of your letters (at least one, at any rate). But I was glad to hear that all is well in N.Y. Things here are still quiet. The Institute got started

at an official ceremony full of generals, colonels, etc. but it isn't certain that they can keep von Weissacker, while Heisenberg probably won't come. As for me, I am getting tireder & tireder of Brazil, but maybe things will brighten after Schiller gets here. I really have no one to talk to at all. The students are so young that they seem like children, while most of the older people just don't know anything. Smith is OK but limited. I was going to go on a vacation in the mountains next week with Andrea Wataghin (the son of Gleb Wataghin), but he is too busy to go. So I shall go instead with a German immigrant – a former member of the Wehrmacht. So you see how desperate I am!

As for the article, let's give it up as a hopeless case. I did write Rosenfeld but all he said was that there isn't any controversy because there is nothing to argue about. He is writing an article for a de Broglie commemorative journal in which he seems to argue that as long as my point of view leads to the same results as complementarity, the whole idea of mine is just silly. He seems to have missed the point completely that the similarity of the two points of view is only in a limited domain, and that my point of view may lead to something very different in the domain of 10^{-13} cm. When the atomic theory was first proposed, positivists like Mach argued that after all it led to the same results as the macroscopic theories (such as the perfect gas laws) so that the extra assumption of atoms was just a pretty little piece of imaginative fiction. Mach would have said that new results of the atomic theory, not yet observed, did not have any meaning, particularly since no one knew just what to predict at that early stage. I suspect that like most complementarians Rosenfeld is a confirmed positivist - so deeply confirmed that like M. Jourdain, who spoke prose without knowing it, they speak positivism without knowing it (and call it dialectics).

I have come to the conclusion that barring a lucky accidental discovery, it will take at least 20 years before theoretical physicists will alter their general point of view very much (This is the time needed to replace the present generation of physicists (especially the big shots) by a new generation). So what's the hurry? I may as well take it easy and try to enjoy life. But this cannot be done by me in Brazil. However, there is a small chance that I may get my passport back, in which case I'll take a trip to England in November. I have asked my lawyer to apply on the basis of the argument that Massey + Mott have invited me to give a few talks.

I have just finished a new article that removes one source of confusion. It proves that as a result of random collisions, an arbitrary probability density $P(X)$ approaches $|\psi(X)|^2$ with the passage of time. One of the boys (Keller), at NYU in Courant's group has just written an article for the Phys Rev (reviewed by me) in which he asserts that behind my theory is an implicit assumption that a "deeper" kind of probability is needed in my point of view than the usual kind (so that as a result, I didn't really succeed in establishing a new interpretation of qu. theory, he says). For if P must equal $|\psi|^2$, it cannot take on an arbitrary value. He said this, despite the fact that I pointed out several times in my articles that a reasonable conjecture was that with collisions, an arbitrary P approaches $|\psi|^2$ with time. I hope he will get the point at last. I sent him a copy of my paper. (I am trying to have the paper mimeographed, but with typical Brazilian confusion, this will take 2 months, plus a considerable amount of effort). But anyway, people seem to be able to find every possible source of confusion

in the theory, and even some impossible one's, like Phil Morrison agreeing with the philosophy, but saying that complementarity is "simpler" because you don't have to make so many hypotheses.

I can't help becoming discouraged, because if I suggest some new idea, it will surely be misunderstood, not only because people are so mixed up, but also because they want to protect themselves from understanding something that could require them to revamp their ideas. I know this "protective confusion" because I used to use it almost consciously to avoid embarrassing arguments (especially political arguments with reactionaries). At this distance, it's very difficult to do anything about it. By talking with people, I can clear it up. For example, Feynman said that he first thought my ideas were crazy, then when I explained them he saw they were at least consistent and later with this proof that $P \rightarrow |\psi|^2$, he even began to prefer this point of view. But I am afraid that Feynman's readiness to consider new ideas is very unusual. (Did you know that he just got married, after returning to the U.S.?)

I have some new ideas on the theory of probability now. I am trying to reduce the subjective element in classical statistics by means of developing the concept of chaotic fluctuations which follow from the causal laws. I am writing up an informal summary, and in the unlikely event that I live long enough to get the article past the endless series of hurdles needed to get it mimeographed, I'll send you a copy. These new ideas on probability fit in integrally into the further development of the causal interpretation of qu. th. & into the ether theory (which is now in the background for a while). If I can succeed in my general plan, physics can be put back on a basis much nearer to common sense than it has been for a long time, and therefore much easier for everyone to appreciate (at least in its over-all aspects). But the present generation of bright young physicists will resist such changes to the bitter end, so I have little confidence that such ideas will be taken seriously for a long time. It's a little hard, but I have to get used to working with the new time scale of 20 years or more, now that I have seen what life is really like in the dep't of initiating (or fomenting as we would say in Portuguese) new theories.

I hear that you have been honored by getting on to various lists of citizens of doubtful character. I hope that your job is still safe. I suppose that you could always retire to the farm and raise vegetables + wild flowers instead of physicists. Perhaps the results would be more satisfactory in the long run.

Give my regards to all the others. Perhaps some day we'll get together again.

Dave

Letter 39. Folder C48, not dated.

[July, 1952].

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Dear Melba

Thanks for your letter. It looks as if the arrangements for Schiller are complete, but you can never trust the people here to do what they say they are going to do. In any case, if all goes according to plan, Schiller should be here before the end of the month.

I had a pleasant vacation in the mountains, and after that we had a Congress in Physics that was a bit dull. 8 physicists from the States (including Wigner, Rabi, Herb [Anderson], [Donald] Kerst, and others), 10 from Mexico, Argentina, and Bolivia, and a few from Europe, were brought here by the Unesco and by the Brazilian National Research Council. Most of the meeting was in Rio, where it is even now quite hot. As usual when I visit Rio, I got sick, and this with the heat & the insane traffic made life quite unpleasant. I gave a talk on my hidden variables, but ran into much opposition, especially from Rabi. Most of it made no real sense, but it boiled down to this: "As yet, your theory is just based on hopes, so why bother us with it until it produces results. The hidden variables are at present analogous to 'angels' which people introduced in the Middle Ages to explain things". People have a peculiar double standard. When someone tries to make a meson theory or a non-local theory and says he hopes for results in the future, even though it is not even self - consistent today, everyone says - fine. But when I present a new point of view which has hopes of solving the problem people say "We are interested only in results".

I am still somewhat discouraged about getting anywhere with the hidden variables, since it is a difficult problem, and since there is little stimulation or help to be gotten from others. But things should improve somewhat when Schiller gets down here.

I am also discouraged about Brasilian physics. I think that any serious efforts made to help in this direction are just being poured down the drain. The set up here is just plain crazy. Part of the insanity stems from the fact that physics here has no solid base - it is being transplanted en-masse from other countries. Industry has not developed enough yet to need much physics. As a result, physics is at the mercy of various asses who have gotten influence and control of the money, like the admiral Alvaro Alberto, and like Professor Marcello Damy de Souza Santos of São Paulo. There is so little consciousness of the nature of physics, however, that people like these can easily fool everybody, pose as geniuses among non - physicists, (there are only about 6 or 7 physicists in the country) and have their way. The latest insanity of the admiral is his plan to import a 450 MeV synchro-cyclotron, plus the people to build it and run it. It is told to me that Lattes arranged this as a "compromise", since originally the admiral planned to build an "atomic city" like Oak Ridge or Los Alamos. (He still plans to do it.)

It is really impossible to describe the poor education and the confusion shown by most of the physics students here. Part of it is rooted in the generally bad situation in Brasilian education, part of it in the contempt endemic in the culture for manual labor, but a large part of it could be remedied in São Paulo if we could get rid of this paranoically insane Marcello Souza Santos, who controls the physics teaching in the first two years, and not only does not teach the students anything, but ruins them by giving them the impression that physics is done only by "geniuses" like him. Incidentally, I have never seen such a contemptible individual as this man. He

presents a good impression at first, but in a few months, one gradually becomes aware of what he really is.

The general picture of the organization of physics is very bad, both here and in Rio. I have little confidence that anything will come out of it, except disappointed and soured individuals who have tried to do some physics. In general, I feel that I ought to try to get out within a year or so, perhaps taking advantage of the time to do some work with Schiller.

As for the manuscript, it has been typed and mimeographed finally. I sent you a copy, and it will be interesting to see whether it arrives, because with printed matter, the post office employees have a habit of robbing the stamps and throwing the mail away.

Well, regards to all. Let me hear from you soon.

Love
Dave

Letter 40. Folder C46, not dated.

[Fall, 1952].

Universidade de São Paulo
Faculdade de Filosofia, Ciências e Letras
Caixa Postal: 8105
São Paulo

Dear Melba

I was very sorry to hear about your troubles - as yet no word of them has reached here in the press or otherwise. I hope that you can weather the storm and retain your job, but apparently you don't believe that this is likely. I am wondering what you will do if the "sword falls". Do you have any idea? I know that the situation must be very unpleasant for you, but I also know what kind of a person you are, so have complete confidence that you will come through O.K. in some way or other. My strongest hope is that at least they don't bother you with trials & things like that. It's one thing to lose your job, and another to face that sort of thing. Incidentally, could you let me have some idea of the source of the trouble? If you would rather not, then of course, just say nothing. In any case, let me welcome you to the fraternity. I shall always remember a very true statement of yours to the effect that the earlier the blow falls, the more fortunate you are, since the first blows are just the fore - warnings of the storm, while the later ones are really murderous.

Incidentally, if you ever need money, I have quite a bit. Of course, it's in cruzeiros, but it could be changed to dollars in a pinch. Perhaps you could come to Brazil. Have you ever considered that possibility? I don't know whether something could be arranged, but it could be looked into if you are interested. Whether it's worth while for you depends on your relative devotion to physics or to accomplishing other things in a place where as they say, you have "roots". In Brazil, little can be done outside of physics by a foreigner, especially by an American.

You once asked about snakes in São Paulo. There are quite a few of them here - so many that it's dangerous to walk in the woods (more accurately, the jungle). But I spent my vacation at Campos de Jardão, where there are relatively few snakes, because of the altitude of 6000 ft.

As for my work with Schiller, it looks promising. I wouldn't be surprised if something resembling Einstein's dream of deducing qu. mechs from general relativity could eventually be achieved. Einstein's general orientation appears to be correct, but his weak point was in not paying enough attention to the specific clues coming from a careful study of quantum theory, especially the Dirac equation. I have vowed to understand the Dirac equation, even if it kills me to do it. As I told Phil Smith, the day that we defeat the Dirac equation, we are going to have a special victory party, with a case of champagne.

In general, I admire Einstein more and more as time goes on for the general correctness of his line of thinking, and for his tenacity in sticking to it. The only place he goes wrong is in his lack of a dialectical orientation. But in general, his ideas are basically clear, simple, and well-founded. Eventually they will probably be superseded by the substratum point of view, but much progress can and must be made, before we are ready to seriously study the substratum.

I want to say that I still miss you very often, and hope that we will eventually come to see each other again. I also miss the house at 298 W 11th St. That is really quite a family that you've got there. I heard from the Schillers that Charley has fixed up a fancy new amplifier system. Please give my love to all of them. How is Paul doing?

As for this business of a trip to England, it looks unlikely, according to my lawyer, who is handling things in Washington. But it may come though. If so, it would be wonderful. I am very much looking forward to such a trip, even though I really don't expect to go. Incidentally, I hear that the causal interp. of qu. theory is being extensively discussed in England + France.

Once again, I send you my best wishes for a satisfactory finish to this encounter with the Senator.

Love
Dave

P.S. I would be interested at least in seeing clippings about your encounter.

Letter 41. Folder C46, not dated.

[December, 1952].

Universidade de São Paulo
Faculdade de Filosofia, Ciências e Letras
Caixa Postal: 8105
São Paulo

Dear Melba

I was glad to hear that all is still (relatively) well with you, and that you are still holding your job. Please let me know how things are going with you now. The Schiller's are also anxious to know.

The Eisenhower victory, with such a smashing majority, was quite a blow. It means trouble of a higher order of intensity. Everyone here was in favour of Stevenson, even the conservatives. Eisenhower's victory has provoked some anti - U.S. reaction, but in general, most people in Brasil do not realize what it really means. This is probably so in other countries too.

There has recently been much trouble in the Unesco. The Mexican and Brazilian delegates resigned, because the U.S. and Britain have been trying to prevent the Unesco from expanding and playing a real role. Instead, they want to keep it under their control as a pretty plaything which exhibits "Western" generosity to sub - developed countries, etc. In addition, the Mexican delegate has been under American attack because he is a "neutralist" and therefore does not actually support the "cold" war.

All of this has significance for us, as it is no longer certain whether contracts, such as we have in mind for a professor of Cosmic Rays, will continue to be renewed. Things are further complicated by the fact that Tinlot, who is officially indicated for this job by the Unesco at present, just told us that he can only get leave from Rochester for a year. This is not enough to get anything done. Therefore, we would like to change to Kurt Sitte who can stay longer. But now, in the present confusion, it isn't clear whether we can do this, as we have been working through the Brazilian delegate who just resigned. Also, we hesitate to ask Kurt to come down here for several years under conditions in which he gets a contract year by year, subject to continual renewal. In the past, such contracts have been renewed, but the future looks doubtful. So we don't know quite what to do.

I just went on a vacation in the mountains again for a week. It was very restful, as the mountains of Brasil are really peaceful (so unlike São Paulo). They give the impression of being timeless - i.e., nothing seems to have happened there for millions of years.

Progress on research in the quantum theory is at present a bit slow. In general, I tend to feel a bit discouraged these days, because there has been very little reaction to my papers. Many people have asked for reprints, but the few communications I received showed (except of course, for those from Peter and a few others) a general lack of understanding of the nature of the problem. Reports that I get from people who travel in Europe say that the reaction there is "So what". In U.S., we have a similar reaction.

The situation in Brazil doesn't look too good. The gov't faces a severe economic crisis due to lack of dollars, and is doing nothing about it, except to sell out to the U.S. Even the National Research Council here has done this, as there is now an employee of the U.S. Atomic Energy Commission who goes around inspecting all laboratories to which this Council has given money.

The situation in the University here is basically bad. There are a few potentially good students, but their basic education is very inadequate. The University runs by personal politics, and we'll never change that. One of the two jokers here, Marcello

Damy de Souza Santos, is thoroughly entrenched, because he grew up with the University, and has the reputation of being an internationally famous genius, even though he is really utterly incompetent. But to get such a reputation here, you need only be a Brazilian physicist. The other character, Stammreich, whom people generally characterize by the word "Nauseous", we may be able to hope to get rid of, as he may soon have to take the competitive examination for his chair, and he knows nothing but spectroscopy, so he might fail. But I don't like the situation here at all. It isn't just the physics dep't that is bad, but the same basically rotten system extends throughout all departments.

The Schillers are gradually getting used to things here. Bunny [Berenice] was very sick for a while, because of difficulties with food, but she is recovering and has begun to work on a thesis problem. Ralph and I are working on the qu. mechs., but things look difficult right now.

Otherwise I have nothing much to say. Please let me know how things are with [missing]. Regards to all (and have a good Christmas!).

Love
Dave