If Dr Small's views are correct we shall require to know every littl detail concerning the habits and life history of this fungus *Rhizoctonia bataticola* in order that its ravages may, to some extent, be controlled. For that reason it has been advocated that Dr. Small be afforded opportunities of continuing his researches on this subject. His researches will be watched with interest and his publications closely studied for some tangible proof of the correctness of his views.

Rhizoctonia bataticola & Root Disease

A Reply to Dr. C. H. Gadd's Criticism of my Views.

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THERE is no justification for the attempt by Dr. Gadd to show that the recently-disclosed presence of *Rhizoctonia bataticola* in cases of root disease of economic and other plants in Ceylon and the views held by me regarding the harmfulness of the fungus give reason for alarm in agricultural circles. He has said, for example, that it is "only a matter of time before its presence becomes more notably evident." He means, it is presumed, that the proved presence of *Rhizoctonia bataticola* and my views upon its responsibility for root disease imply that the immediate or near future will see an increase in the amount of root disease in the field and that *Rhizoctonia bataticola* will be responsible for that increase. There are no grounds for such a deduction. The finding of a new fungus cause of root disease does not imply that the fungus in question has begun only at the moment of its discovery to cause disease or that it is likely to increase its efforts in the future. On the contrary, it is held, not without reason, that *Rhizoctonia bataticola* has been present in Ceylon soils and has been in operation as a cause of root disease in the past. It follows that, as far as the *Rhizoctonia* is concerned, field conditions affecting the amount or incidence of root disease have not changed; in other words, *Rhizoctonia bataticola* has been and still is doing its worst. As far as Ceylon is concerned, there is nothing new about *Rhizoctonia bataticola* except its recent discovery and my interpretation of the meaning of its presence. I attempted to make this point clear when I wrote in the *Tropical Agriculturist* of last October (67, p.237) with reference to *Rhizoctonia bataticola* on tea: "It may be added that the discovery of a new root fungus on tea does not imply an increase in the gross amount of tea root disease in Ceylon, even if the fungus in question should be found alone, as it has been, on tea and even if it should be proved to be capable, as it may, of causing tea disease unaided. It means in this case that the *Rhizoctonia* has not been taken into account in the past and that its presence adds one more to the number of fungus agents causing or associated with root disease. The *Rhizoctonia* is thus to be regarded as a new agent, the full and real significance of which is not clear at the moment." The phrase "a new agent" may be improved to read "a newly-found agent," and I may add that further investigation of the conditions under which *Rhizoctonia bataticola* occurs has made clear its significance and has led, in fact, to the views which I hold now and which I enunciated at the Agricultural Conference in March (*Tropical Agriculturist* 68, p. 201, April, 1927). I may add that my views of the significance of the presence of the *Rhizoctonia* apply to all the plants attacked by the fungus and, further, that, despite the fact that "experienced mycologists" have not yet recorded the *Rhizoctonia* from other parts of the tropics, I see no reason for ceasing to expect the finding of it in regions other than those in which it has been found and studied, namely, Uganda and Ceylon. I refer particularly to its occurrences on woody hosts for, although

Rhizoctonia bataticola has been recorded as a parasite of herbaceous plants in India, Egypt and America in addition to Uganda and Ceylon, investigation of its woody hosts has been confined to the two last countries.*

The control of *Rhizoctonia bataticola* presents a difficult problem, but I believe it is a mistake to regard it as insoluble. Even if it proves to be insoluble, there will be no cause for alarm so long as the present field conditions which affect the fungus remain unchanged, and I see no reason why they should change in favour, so to speak, of *Rhizoctonia bataticola*. They are more likely to change in favour of the plant exposed to the *Rhizoctonia*. The introduction of the question of alarm is therefore unreasonable and unnecessary, and there are no grounds for the assumption that "the prospects of our main agricultural industries in Ceylon are anything but bright." From the point of view of *Rhizoctonia bataticola* the said prospects are unaffected. The introduction of the question of alarm tends merely to confuse the real issue. The latter is the question of the responsibility for root disease, a responsibility which, in my opinion, has been laid on the wrong shoulders and which should be transferred to *Rhizoctonia bataticola*.

Dr. Gadd proceeds to discuss the local treatment (trenching and stumping) of root disease which he attributes to *Fomes*, *Poria* and *Rosellinia* and the reasons for the adoption of the treatment. He asserts that it has been successful, and he concludes that "the success achieved by these methods has certainly justified their adoption." He also remarks that, "if the success of the treatment is admitted, the correctness of the principles on which it was founded is tacitly implied." He is understood to argue that the treatment has been so successful that the supposed diminution in the incidence of root disease which he mentions and which is accepted as a fact for the sake of argument has been due entirely to the treatment. He also implies that *Fomes*, *Poria* and *Rosellinia* must be causes of root disease on the ground that the treatment of their supposed outbreaks is successful in controlling their spread.

Dr. Gadd's argument and his conclusion regarding the parasitism of *Fomes*, *Poria* and *Rosellinia* hinge wholly upon his assertion that a certain treatment of root disease has been followed as a matter of course by a diminution of the amount of root desease, and his argument must fall to pieces and his conclusion be invalidated if it can be shown that his assertion regarding the success of the root disease treatment may be doubted. The first point against the said assertion is that it is founded on a basis of *post hoc ergo propter hoc* and that it possesses all the weaknesses of a *post hoc ergo propter hoc* proposition. When a certain act, say, trenching and stumping, appears to be succeeded by a certain result, say, diminution of root disease, it does not follow that the act and the apparent result are related in the sense that the former is, or must be, the cause of the latter or the latter the effect of the former. Between the cause and the apparent effect there is a gap which can be stopped effectively in only one way, namely, by proof of a connection between the cause and the apparent effect. Such proof is entirely lacking in the case under discussion, and the assertion of the success of the root disease treatment is nothing more than a mere statement on which legitimate doubt may be cast so long as the supposed connection between treatment and diminution of root disease is unproved. Apart from the fact that it is doubtful if all those who have practised the treatment would claim success for it, there will be no good reason for postulating a logical connection between the treatment and the diminution of root disease until control

* Information has been received recently from the my ologists of Burma and Southern Rhodesia that they have *Rhizoctonia bataticola* in their respective regions, but stress is not to be laid on the presence of the fungus in the countries mentioned until the identifications can be confirmed from specimens and isolations of the fungus.

experiments in trenching and non-trenching and in removal of stumps and debris and non-removal have demonstrated that trenching and stumping influence in any way the magnitude of a supposed outbreak of *Fomes*, *Poria* or *Rosellinia* disease; that is to say, when it is shown particularly that the lack of trenching and the non-removal of stumps and debris result in a greater loss than would be occasioned if trenching and stumping were performed. While there is no such proof of the efficacy of the treatment, it cannot be claimed that the treatment has led to a diminution of root disease, and the assertion that the methods of treatment are successful and that their success has justified their adoption are valueless. The deduction that *Fomes*, *Poria* and *Rosellinia* are causes of root disease because certain treatment has appeared to control the extent of their outbreak has nothing to commend it. Even if the treatment controls their spread, they may still be saprophytes or secondary fungi, but their parasitism is more doubtful than ever when it is shown that there need be no connection between their supposed successful treatment and the diminution of root disease. Again, Dr. Gadd has neglected to state that the diminution of root disease may be capable of explanation on grounds other than those he mentions, and it may be pointed out that the diminution is difficult of proof. It may also be doubted if all Ceylon agriculturists will agree that there has been a diminution of root disease. Another point is that the fungi supposed to be treated with success cannot be regarded as the basic causes of root disease until their parasitism has been proved. I am justified, therefore, in holding the opinion that the treatment only *appears* to be successful inasmuch as it may control the spread of secondary fungi like *Fomes*, *Poria* and *Rosellinia*, the habits of which, be it noted, enable them to kill off plants already attacked by *Rhizoctonia bataticola* more quickly than the slow-acting *Rhizoctonia* itself. The treatment has no effect upon the real cause of the disease, *Rhizoctonia bataticola*, and no good reason has yet been brought forward for departing from the hypothesis that the real cause of root disease was and is the hitherto unrecognised presence and parasitism of *Rhizoctonia bataticola* and that *Fomes*, *Poria* and *Rosellinia*, if present, were and are only secondary agents. I therefore continue to regard all the plants involved in an outbreak of root disease, whether large or small, as having been aitacked in the first place by *Rhizoctonia bataticola*, and I hold that trenching and stumping are of little, if any, value as treatment of the outbreak because they do not affect in any way the incidence or the spread of the *Rhizoctonia*, a fungus which does not move from plant to plant by contact, does not spread through the soil by creeping mycelium and is widely distributed in Ceylon soils.

The value of trenching is severely limited. It may be used to confine secondary fungi to the area in which they first appear and in that way it may ward off their attacks on plants which are outside the area, but it must be understood that, attacked as they are by *Rhizoctonia bataticola*, all the plants of a given outbreak of root disease of, say, tea are doomed whether they are protected from the rapid effects of secondary fungi by their being outside a trench which limits the spread of the secondary fungi or whether they are exposed to the secondary fungi by virtue of their position inside the trench. In other words, trenching does not influence the magnitude of an outbreak of root disease though it may succeed in prolonging the life of attacked plants by protecting a proportion of them from secondary fungi,—a doubtful advantage. It should be noted that the facts that the rate of progress of the *Rhizoctonia* in individual plants may vary and that all diseased plants are not necessarily infected at the same moment may lead to an appearance in the field which suggests slow spreading of infection from plant to plant, and, further, that the attacks of secondary fungi like *Fomes*, *Poria and Rosellinia* may heighten the appearance of spreading of infection and cause it to appear to be rapid.

With reference to *Rhizoctonia* disease of rubber, it has been suggested by Mr. Roy Bertrand that trenching may be beneficial inasmuch as it may be used to prevent the roots of healthy trees from entering areas known to be infected by *Rhizoctonia bataticola*. The possible value of trenching in such a case must be admitted, but it is limited by the fact that the roots of neighbouring trees have been exposed in all likelihood to the attacks of the *Rhizoctonia* by their natural growth and spread in the soil before it is apparent that the area which they have entered is infected. Again, the roots which it is desired to protect may be infected already or may be resistant to the attacks of the *Rhizoctonia* or may not have come into contact with the fungus in the soil. In either case, the value of trenching carried out after *Rhizoctonia* disease has been found is problematic.

Stumping, again, may or may not be of value. As far as *Rhizoctonia bataticola* is concerned, it is of little value because the presence of stumps and debris has nothing whatever to do with the presence of the *Rhizoctonia* in the soil. It is probable that its wide distribution is accounted for in a totally different manner. In this connection it ought to be made clear that "stumps" does not include remains of plants killed by *Rhizoctonia bataticola*. In the case of *Rhizoctonia* disease of, for example, tea and rubber, the removal of all the roots, even the smallest, of diseased plants is advised on the ground that their decay *in situ* will liberate in the soil the numerous sclerotia or resting bodies of the fungus and so will ensure in time a greater exposure of the supply plant to *Rhizoctonia* attack than need be. As far as the secondary fungi, *Fomes*, *Poria* and *Rosellinia* are concerned, stumping may be useful as a means of removing their points of origin and so of controlling, in the manner explained above, the secondary damage they may do. Against this consideration it has to be remembered that *Fomes* and *Poria* may be found in areas where there are no stumps. They must therefore live free in the soil, and, while they can do so, stumping can have only a limited effect upon their presence or absence. Similarly, they may be present on stumps and cause no disease. In short, trenching and stumping may affect the apparent duration in time of an outbreak of root disease by prolonging the life of diseased plants in the manner described, that is by checking the activities of secondary fungi, but they can never affect the magnitude of the outbreak because they do nothing to control the true cause of the outbreak.

It has been shown that the success claimed for the root disease treatment is more apparent than real and that the assumed cause-and-effect relationship between treatment and diminution of disease is not proved and, in fact, is unlikely to exist. There is therefore no basis for Dr. Gadd's remark that "there appears to be some evidence indicating the accuracy of the old ideas, that *Fomes*, *Poria*, etc. are the active parasites." It has yet to be proved that their treatment has any connection with the diminution of root disease and that the fungi themselves are parasitic.

Another point which has a bearing on this discussion may be mentioned, namely, that the supposed absence of root disease of, say, rubber in certain regions is attributed to the fact that the areas planted with the crop contained no stumps or were cleared of them before planting was done; in other words, absence of root disease is attributed to lack of stumps. The absence of root disease, however, is only apparent. Occasional trees or small groups of trees die in the areas mentioned, and their loss is attributed to *Diplodia* dieback or to lightning or to unknown causes. It is suggested that an examination of the roots of such trees will disclose the fact that they have been attacked by *Rhizoctonia bataticola*, and I may add that I am confident of obtaining confirmation of this view.

I now take up Dr. Gadd's criticism of my reasons for holding that *Rhizoctonia bataticola* is *the* parasite and that other fungi which have been blamed in the past are secondary agents which cannot cause root disease of themselves and can attack only plants already attacked by the *Rhizoctonia*. Dr. Gadd's first point is an objection to my use of the phrase "sole association" applied to cases of root disease in which, as I put it with reference to *Rhizoctonia bataticola*, only one fungus occurs. He attempts to show that there cannot possibly be a case of "sole association" on the ground that, "as soon as a tissue has been killed by one organism, other organisms appear and sometimes so crowd out the original invader that the latter is difficult, almost impossible, to find," and, further, that "every diseased root bearing *Fomes* and *Poria* is accompanied by numerous other fungi besides *Rhizoctonia bataticola*", and that "no claim has ever been made or is likely to be made by an experienced mycologist that *Fomes*, *Poria*, or other parasitic fungi occur in sole association with frequent cases of root disease."

Taking his statements in the order in which I have quoted them, I would point out with reference to them that, first, the crowding out of the original invader, in this case *Rhizoctonia bataticola*, by secondary organisms does not take place, and, second, that I doubt if every diseased root bearing *Fomes* and *Poria* is accompanied by numerous other fungi besides *Rhizoctonia bataticola*, and, third, that, if the accounts of *Fomes* and *Poria* disease given by experienced mycologists do not claim, or at least imply clearly, that these fungi occur in "sole association" with frequent cases of root disease, the said accounts are extremely misleading.

Returning to the first point, I would call Dr. Gadd's attention to the following facts. The most striking symptom of *Rhizoctonia* attack on roots is a drying and hardening of both bark and wood which is perhaps more apparent in wood than in bark (and cortex) but which, nevertheless, affects both. This hardening does not encourage the growth of secondary organisms. Numerous isolations from both bark and wood of roots affected by *Rhizoctonia bataticola* have been made. In many cases, secondary organisms are absent; in others, their presence may be dismissed on the grounds mentioned later, namely, that they are of minor importance. Secondary organisms, if present, might be expected to develop in numbers on *Rhizoctonia*-infected roots kept in damp sand for months; they do not do so either in numbers of species or numbers of individuals. Many sections of root wood and bark containing *Rhizoctonia bataticola* have been made and examined, and the closest scrutiny of the sections under the microscope has failed entirely to disclose the presence of the mycelium or other parts of the numerous secondary fungi mentioned by Dr. Gadd. In fact, the only fungus of consequence which occurs more than rarely along with the *Rhizoctonia* on or in roots apparently affected by the *Rhizoctonia* alone is *Diplodia*, a fungus which is regarded as, and is being shown by experiments in progress to be, a very weak parasite if it is a parasite at all. When specimens of roots killed by *Rhizoctonia bataticola* are taken from plants which are sickly but not quite dead, that is to say, roots which are in the most favourable condition for examination into the cause of their death because secondary organisms have not yet invaded them or have not taken the complete possession of them mentioned by Dr. Gadd, they are found to be quite free of secondary organisms. It can be asserted at this point that, as far as *Rhizoctonia bataticola* is concerned, secondary organisms do not crowd out the fungus or render it difficult to find or to isolate.

With regard to the second point, that "every diseased root bearing *Fomes* and *Poria* is accompanied by numerous other fungi besides *Rhizoctonia bataticola*," this statement may be doubted because it has not

been found to be either relatively or universally true. To mention only the case of *Poria*, tea specimens can be found, the collar and the larger parts of tap and lateral roots and even the small roots of which are so closely covered with *Poria* mycelium that there is no room or footing for fungi other than the *Poria* itself and the one which may be found underneath the *Poria* because it penetrated the root before the *Poria* attacked it, namely, *Rhizoctonia bataticola*. Other fungi may possibly find an entry at a later stage, that is, when *Poria* has rotted the wood of attacked parts, but that fact does not affect my present argument. Again, if "every diseased root bearing *Fomes* and *Poria* is accompanied by numerous other fungi besides *Rhizoctonia bataticola*," which other fungi Dr. Gadd would insist on taking into account, it may be asked why the other fungi are not mentioned at least in, for example, Mr. Petch's account of, say, *Poria* root disease of tea in his "Diseases of the Tea Bush." It must be concluded that Mr. Petch did not see the numerous other fungi and that they were not present, or that, if he did note their presence, he did not think them worthy of mention.

The latter conclusion seems more likely to be correct, but it is unlikely that *Rhizoctonia bataticola* was numbered among the fungi which could be thus ignored. A point which Dr. Gadd has missed, a most important one, is that the fungi associated with a case of root disease, particularly of a woody plant, can be divided into two groups, major and minor. An experienced mycologist will have little difficulty in deciding on grounds of relative frequency of occurrence or degree of penetration or presence of regular symptoms which fungi shall be placed in one group and which in the other, and it seems as if Mr. Petch regarded the problem from this point of view when he wrote his account of, say, *Poria* disease of tea. It is evident that he thought that in this particular case *Poria* was the only fungus worthy of mention and of the responsibility for root disease among all the forms he found on diseased roots. This brings me to the crux of the matter, namely, that the words "sole association" are not used in an absolute sense. They are used in the relative sense which is implied in Mr. Petch's account of *Poria* disease of tea. It is justifiable to conclude from his account of *Poria* disease that he meant it to be understood that the fungus *Poria* was in "sole association" with the cases of root disease he attributes to its agency, and it follows that, contrary to Dr. Gadd's assertion, a claim has been made by an experienced mycologist that "*Fomes*, *Poria* or other parasitic fungi occur in sole association with frequent cases of root disease." I have mentioned already that I consider such an account of *Poria* disease of tea as that given by Mr. Petch to be misleading if it is not in order to conclude from it that *Poria* is claimed to be in "sole association" with the cases of disease which Mr. Petch associates with it. In concluding this part of the discussion, it may be pointed out that Dr. Gadd has produced no valid objection to my use of the words "sole association." I have attempted to make clear the sense in which they are used and I have shown that another mycologist has implied "sole association" in his description of a certain root disease taken as an example. It is justifiable, therefore, to continue to claim that *Rhizoctonia bataticola* occurs in "sole association" with cases of root disease and to refuse to believe in the parasitism of *Fomes*, *Poria* and other fungi until cases of their "sole association" with root disease in the sense that a certain major fungus, *Rhizoctonia bataticola*, is not present at the same moment are encountered. In other words, the words "sole association" may be used legitimately as a criterion of the pathogenicity of a given fungus.

Dr. Gadd objects to such a criterion, for he says, "if we allow for the sake of argument that *Rhizoctonia bataticola* does occur alone, I still maintain that it constitutes no proof of parasitism." He then proceeds

to explain why he refuses to accept sole association as a proof of parasitism He says that small roots may die back from physiological causes and that *Rhizoctonia bataticola*, a fungus which is widely-distributed in our soils, enters them and takes up in them a saprophytic existence; in other words, it lives a non parasitic life in the dead roots. It is to be noted that he does not mention the possibility of its continuing its growth into larger parts of the roots and so passing from a supposed saprophytic to a parasitic condition. No matter in what state, saprophytic or parasitic, the fungus begins its life inside the roots of, say, a tea bush, it must become parasitic if it can be shown to advance and penetrate and permeate the roots or a proportion of the roots of the living plant. That it may and does do so is shown by specimens in my possession; in fact, if secondary fungi like *Poria* do not attack the sickly plant, the penetration will be complete enough to enable the fungus to permeate the above-ground main stem viâ the roots and to render the wood dry and hard after its characteristic fashion. My point is that such penetration and permeation as I have just mentioned, processes which can be shown to take place, constitute evidence of parasitism sufficient to satisfy an open-minded person. Such evidence has been accepted without question in the past, and it is difficult to understand why it should not be accepted in the case of *Rhizoctonia bataticola*. When penetration is accompanied by sole association, the evidence of parasitism is even more complete. If *Rhizoctonia bataticola* were the saprophyte that Dr. Gadd suggests it is and not the parasite that it is claimed to be, it would remain in the small dead roots which he says it may enter, and would not kill the plant by advancing in its tissues and interfering so seriously with its normal life as to lead to its eventual death.

It may be thought that the advance of *Rhizoctonia bataticola* from its first point of entry into the plant can be explained by saying that environment is at fault ; in other words that the plant must be in bad health to permit the fungus to make headway. Here another mistake would be made. The causation of fungus root disease may be regarded from two points of view which may be called the *immediate* and the *ultimate*. The immediate point of view considers the fungus or fungi which are actually found in association with root disease and decides which organism is responsible for the disease. The ultimate point of view, on the other hand, considers the causes, physiological or other, which enable the fungus to attack the plant ; in other words, it considers the question why there should be any root disease at all and endeavours to explain the conditions which enable a fungus to establish itself in the roots of a plant. These points of view are distinct from each other, and it may be insisted legitimately that this discussion is concerned only with the immediate point of view. The whole question at issue, namely, the responsibility of *Rhizoctonia bataticola* for root disease, is concerned only with the fungi which are associated with root disease and not with the reasons, physiological or other, why there should be such a thing as fungus root disease. To put the matter differently, the point is that *Rhizoctonia bataticola* is the fungus that enters by the smallest roots and so starts on a career which cannot be regarded as other than parasitic ; it is irrelevant to this discussion to introduce the question of the ultimate responsibility of environment for the presence of the *Rhizoctonia* in the root tissues of the plant.

One small matter remains before Dr. Gadd's next point is taken up. He talks of trees growing in infertile soil "such as is usually the case when rubber trees are affected by dieback." He means that rubber dieback (presumably *Diplodia* dieback) is due primarily to infertile soil conditions which so militate against the health of the tree that *Diplodia*, an acknowledged weak parasite, is able to attack it above ground and kill it to its base. I have announced elsewhere that I have found in all recent

cases of *Diplodia* dieback of rubber that the roots of the trees are affected by *Rhizoctonia bataticola*, and I assert that the *Rhizoctonia* is to be found in the roots of every tree affected by normal *Diplodia* dieback. I hold further that the root disease caused by the *Rhizoctonia* is a more satisfactory explanation of the ill-health and eventual death of the tree—in my experience, all dieback trees die eventually,—than the weak parasitism of *Diplodia*. But the point of this paragraph is that Dr. Gadd would explain the attack of *Diplodia* on the ground that the tree is so weakened by its being in infertile soil that *Diplodia* is enabled to attack successfully. Another explanation is that the tree is so weakened by *Rhizoctonia* root disease that *Diplodia* can attack it, and its claim may be based, first, on the fact that the penetration of the *Rhizoctonia* in the roots of dieback trees is so thorough and has gone so far that the fungus must be more than a mere saprophyte, and, second, on the fact that healthy trees in the neighbourhood of the dieback tree may be expected to feel the effects of the supposed infertile areas. That they do not do so is apparent in the field. Again, it is doubtful if it can be proved that every case of dieback is associated with infertile soil or that the converse, that dieback-affected trees are never to be found in fertile soil, is true.

Dr. Gadd attempts next to explain away the statement that, when more than one fungus is present in cases of root disease, *Rhizoctonia bataticola* is the fungus which is consistently present. The second fungus that may accompany it may be, to confine the list to the fungi mentioned by Dr. Gadd, *Fomes* or *Poria* or *Rosellinia*. He says that the *Rhizoctonia* is a saprophyte and that "the dead roots resulting from the attack of one of the established parasites form an excellent medium in which this common soil fungus can grow." Apart from the fact that such an interpretation ignores the facts of the case, its assumption that *Fomes, Poria* and *Rosellinia* are "established parasites" cannot be allowed to pass. Before these fungi can be regarded as "established parasites," two conditions at least must be satisfied. The first is that they must be shown to be in "sole association" (in the sense in which this phrase may be used) with cases of root disease; the second, that they must be proved to cause disease. The present position of *Fomes*, *Poria* and *Rosellinia* is that no one of them can fulfil one of these conditions, let alone both, and that theirparasitism, resting as it does on mere assertion or on a supposed sole association with root disease which does not exist, is far from established. *Rhizoctonia bataticola*, on the other hand, has been proved by experiment to cause root disease in India, Egypt and Uganda, and will be proved, I hope, to cause it in Ceylon, and it can be shown to be in sole association with numerous cases of root disease. These facts cannot be ignored and therefore the claim made for the parasitism of *Rhizoctonia bataticola* cannot be set aside as easily as Dr. Gadd imagines.

The next matter for discussion is the claim that *Rhizoctonia bataticola* precedes in time of attack the other fungus, say *Fomes* or *Poria*, that occurs along with it. It is noted that Dr. Gadd does not object to the importance attached to priority of time of attack. The priority may be difficult to prove but that is no reason why it should not be attempted from both field and experimental sides. He takes one of my field pieces of evidence, a case of rubber root disease in which *Rhizoctonia bataticola*, found to be well-established at the first examination of the roots, was succeeded after a few months by *Fomes* (two species), *Ustulina* and *Diplodia*. My explanation is that the *Rhizoctonia* was the original parasite and that *Fomes*, *Ustulina* and *Diplodia* in this case followed it in time. I also hold that *Fomes*, *Ustulina* and *Diplodia* would not have been found on the roots at all if *Rhizoctonia bataticola* had not prepared the way for them. In other words, *Fomes*, *Ustulina* and *Diplodia* were in this case (as they are in all others, I

hold) of only secondary account. In the first place, I can assure Dr. Gadd in reply to his suggestion that *Fomes* might have been present at the first examination that the presence of *Fomes* was not overlooked. The first examination of the roots of the trees in question was conducted with care because the case was one of the first of *Rhizoctonia* on rubber roots. I had been surprised to find in plenty on roots sent to the laboratory with the first report of the disease a fungus which had never been recorded on rubber in Ceylon or any other country. In the second place, the degree of penetration of the roots by *Rhizoctonia bataticola* was incompatible with the idea that the *Rhizoctonia* was present only as a saprophyte. In the third place, Dr. Gadd says that "after such treatment (meaning the first examination) the presence of any soil fungus on the roots merits no comment." The question cannot be dismissed in this fashion. Perhaps I ought to have explained that at the first examination the trees were moribund and that at the time of the second examination their outward appearance had changed but little if at all and also that between the examinations the roots of the trees were not left exposed. No doubt the first examination disturbed the roots, but, even so, it could not have had any serious effect on the trees because the majority of the roots were already killed and permeated by the *Rhizoctonia* to points varying from three to six feet from the trunks of the trees. The treatment, therefore, does not explain the presence of the *Fomes*. In any case, it must be admitted that the *Fomes* was in this case a secondary or saprophytic fungus. That, after all, is the point of importance in the matter. The secondary nature of the *Fomes* also explains why, in the case of one tree in which all the roots had not been affected by the *Rhizoctonia*, the *Fomes* occurred only on those roots which had been attacked by the *Rhizoctonia*. Other cases of rubber disease in which no disturbance of the roots took place have shown *Rhizoctonia bataticola* well-established in the smaller roots and advancing into the larger roots and *Fomes* and *Poria* on the outside of the larger diseased roots, and I hold that the explanation of the situation as a case of parasitism on the part of *Rhizoctonia bataticola* followed by the entry of saprophytic or secondary organisms fits the facts in every case. Further, Dr. Gadd does not explain the presence of more than one of his supposed parasitic fungi on the roots of a single diseased tree whereas the preliminary parasitism of the *Rhizoctonia* is sufficient reason why secondary fungi should appear in numbers and at the points where *Fomes*, *Poria* and others are found.

I am glad that Dr. Gadd admits the assertion that *Rhizoctonia bataticola* enters the plant by the smallest roots. Out of the fact that *Rhizoctonia* does attack viâ the small feeding roots arise two points. The first is the claim that that method is "the most natural means of entry" into the host plant, and the second is that I hold that, having established itself in the smallest roots, *Rhizoctonia bataticola* then invades the root system (or part of it, for, in a given case, all the roots need not be parasitised) and advances, so to speak, onwards and upwards, always getting nearer the larger parts of the roots.

Dr. Gadd objects to my "most natural method of infection." It may be pointed out that we are dealing with soil fungi and root parasites and that the introduction of forms like *Pythium Debaryanum* and *Rhizoctonia solani* which attack at the surface of the soil is irrelevant to the question in hand. The introduction of the question of infection of the shoots of a plant is even more irrelevant because it implies an analogy between root infection and shoot infection which does not exist. The entry of *Rhizoctonia bataticola* into a plant by the smallest roots is regarded as the most natural means because it has been proved to be the means actually adopted by the *Rhizoctonia* and I shall believe that attack on a larger woody root is not only natural to but possible

for *Fomes*, *Rosellinia*, *Poria*, *Ustulina* and *Diplodia* when these latter fungi are proved to be able to penetrate the larger root without the help of a wound or without the help of another fungus which has weakened or killed the large root and so prepared the way for them. Until such proof of their natural attack on larger roots is forthcoming (experiments which attempt to show whether *Fomes*, *Poria* and other fungi can attack larger roots are in progress), it is legitimate to believe that entry by the small feeding roots is the most natural method for the fungi under discussion in this paper. On these grounds the belief is more than mere opinion.

The next part of Dr. Gadd's criticism is a picture of a *Rhizoctonia* attack which is more fanciful than it need be, and is therefore misleading and inaccurate. He tries to make out that *Rhizoctonia bataticola* drops into a secondary position during its advance in the tissues of its host and that I have called in the aid of a toxin to help the fungus out of its difficulties. He says that the calling in of the toxin "affords a reason why *Rhizoctonia* is not to be found in the forefront of the attack." If he imagines that I have said or led him to understand that *Rhizoctonia bataticola* does not fight its own battles or lead its own attack, he is mistaken. *Rhizoctonia bataticola* enters the small roots, interferes at once with their water-absorbing functions, and proceeds to make its way into the larger roots and from the latter into the largest roots of all. It is perfectly apparent from the examination of numerous examples of *Rhizoctonia* disease that, if the fungus is left to itself, so to speak, it will continue to advance slowly but surely and to permeate all the roots it has entered until it reaches the collar of the plant, and it is further apparent that, in certain cases, of which tea (both old plants and seedlings) is one and *Grevillea robusta* is another it will be found eventually in the main stem of the plant in the form of hundreds of its sclerotia. Such cases of complete penetration and permeation are not uncommon. They are conclusive in their evidence that *Rhizoctonia bataticola* is primarily parasitic and that it does not occupy a secondary position. No other interpretation of its presence is possible. If *Fomes*, for example, was found in similar sole association with disease and was capable of being followed from its point of entry in its progress through the roots of the plant, there would be no hesitation in acclaiming it as a parasite. It is difficult to understand why similar recognition should be denied to *Rhizoctonia bataticola*.

In other cases, however, the harm done by the advance of the *Rhizoctonia* reaches a certain point, that is, it affects the larger roots. The larger roots feel, as it were, the advance of the fungus, and they begin to sicken. It must be remembered that, since the entry of *Rhizoctonia bataticola*, their supply of water and food materials has been interfered with. The general result is that they are rendered open to the attacks of other fungi that may be present in the soil. If the other fungi are absent, the *Rhizoctonia* advance goes on unhindered. If the other fungi are present, they may and often do attack the sick or weakened roots. It is in such a case and at such a time, and only then, that *Fomes*, *Poria* and company, the major secondary or saprophytic fungi, put in an appearance. They are unable to attack healthy roots, but they can attack unhealthy ones and they proceed to do so. By all accounts their growth is more rapid than that of the *Rhizoctonia*, and their entry upon the scene has the effect of hastening the otherwise slow death due to the *Rhizoctonia* alone, but their appearance does not mean that the attack of the *Rhizoctonia* ceases. In fact, it can be shown from specimens that it does not cease. In other cases, the *Rhizoctonia* can be shown to be well-established in a tissue before the secondary fungus appears on the scene. In either case, whether the *Rhizoctonia* remains in sole possession, so to speak, or whether it is aided eventually by secondary fungi, its original responsibility is unchanged,

and the idea that it occupies a secondary position and so requires the aid of a toxin to help it is absurd and totally unjustified. The suggestion that the *Rhizoctonia* kills in advance of its actual point of penetration with the aid of a toxin produced by itself was made as a possible explanation of the killing in advance and nothing more. Further, there is no objection to withdrawing in the meantime the suggestion that the fungus kills in advance of its actual point of penetration with the aid of toxin produced by itself, and it is to be noted that the essential facts of the case are unaltered. I hold that no unprejudiced person can examine a case of *Rhizoctonia bataticola* root disease without coming to the conclusion that the fungus advances from its point of entry onwards through the roots and kills as it goes. I may agree that I have offered less evidence on this point than on others, but I cannot agree that the evidence does not exist. It is only too plainly set forth in the field.

Dr. Gadd concludes his criticism by saying that the evidence which I have brought forward is insufficient "to establish the fact that the fungus *(Rhizoctonia bataticola)* is a parasite at all, and that the observed facts may be accounted for more simply on the basis that *Rhizoctonia bataticola* is nothing more than a common soil saprophyte." I have endeavoured, on the other hand, to show that the *Rhizoctonia* is a parasite and not a saprophyte, and I have criticised his reasoning and questioned his deductions. I have given reasons for my interpretation of the meaning of the universal presence of the *Rhizoctonia* in cases of root disease, and I hold that the reasons cannot be swept aside lightly. I hope it is only a question of time until the results of *Rhizoctonia* and other experiments will become available but I should like to remark that it is an established fact that successful infection of the host plant in experiment is often unattainable or very difficult to attain although the same infection occurs frequently in nature. Dr. Gadd's statement that "there should be little difficulty in reproducing at least some of the types of disease under laboratory conditions" may therefore be questioned. In fact, it is apparent both from experience in Uganda and attempts to reproduce *Rhizoctonia* infection in Ceylon that the conditions of laboratory experiments do not conduce to rapid infection. Meanwhile, it is unwise to ignore or brush aside such parts of the evidence as show that the presence of *Rhizoctonia bataticola* can be explained only on the ground that the fungus is a parasite.

One point remains, that of Dr. Gadd's classification of tea root diseases into three types according to their symptoms. His first two types, *Poria* and brown root, are founded on the symptoms induced by secondary fungi, and his third or *Diplodia* type corresponds closely in symptoms to disease caused by *Rhizoctonia bataticola*. Dr. Gadd has said that "it seems strange that brown root disease should crop up when solitary trees are to be finished off while *Poria* and *Fomes* occur only when a large patch has been prepared by the ravages of *Rhizoctonia batatiçola* for slaughter." It is not universally true, however, that *Poria* and *Fomes* are confined to large outbreaks or patches of root disease. Both have been found on single plants attacked at different points in a field by *Rhizoctonia bataticola*. It is to be remembered that the *Rhizoctonia* may attack single plants situated at

different points in a field or groups of plants growing together, and that it is possible that the common appearanee of *Poria* as a secondary fungus on tea or of *Fomes* on rubber may be explained on the ground that their growth is more rapid and their distribution greater than those of the fungus of brown root disease which appears less frequently.

Full descriptions of the three types of tea disease which began at the beginning, that is, with the first symptoms of disease, would be alike up to a certain point, and the first two supposed types would diverge from the third only when and because secondary fungi like *Poria* appeared and induced secondary symptoms, for example, the apparent rapid spread of *Poria*. This explanation is founded, of course, on the proved presence of *Rhizoctonia bataticola* in cases of root disease and on my view of the meaning of its presence, namely, that it is the primary parasite without the work of which the secondary fungi would be unable to attack, and I do not yet see any good reason for departing from my position in any respect. I am prepared, as I have said in another place, to believe in the independent primary parasitism of *Fomes*, *Poria*, *Rosellinia* and *Diplodia* when it is proved but not until it is proved.