

Reich-DuTeil Correspondence

Reich to DuTeil, 27 Feb. (*Beyond* p. 61-62, draft p. 107-8) DuTeil recalls (“The Bion Expts: An Afterword,” *Energy and Character*, Sept. 1973—**Reich sources 1 bdl**) that a group of his students brought Reich to hear a lecture of D’s in Feb. 36, after which Reich told D about the bioelectric expts. D informed the French Acad. of Sci. about Reich’s expts. (see letter of 2/28 to Irmgard Strauss) “Dear Professor, Through Victor Bauer I am sending you a copy of the manuscript on the experimental results of my work [The Bioelectric Expts], which is now ready for press. I heard from Dr. Bauer that you are taking a great deal of trouble over the matter; thank you very much indeed. I certainly hope that one day I will have the opportunity to discuss the ambiguities with you in more detail than was possible during our brief meeting.

At the moment, I am making all the necessary preparations to examine the problems relating to the charge and tension process and the expansion and contraction phenomena in mollusks [Weichtieren]. It is not an easy matter to create all the necessary technical conditions here. In addition, in the course of the preliminary testing, so many new and strange phenomena have shown up that I have to take great care not to lose my way in the undergrowth.

I shall take the liberty to keep you informed of the further progress of the experiments. For the time being, I prefer to report only the actual observed results before I publish the general theories that derived from these data. Dr. Baer has probably given you a reprint of the article “The Basic Antithesis of Vegetative Life,” in which about three years ago I summarized the clinical-theoretical preconditions for the tests on the basis of experiments carried out by other authors. I would be extremely happy to hear what opinions you have formed about the entire matter.”

Weds. 29 July 36, Reich to DuTeil (correspondence box 10, DuTeil flds) “I would be very thankful, if you would let me know, when the French publication of my work “Electrical Function of Sexuality and Anxiety” can be expected. I hear from Irmgard that you have been occupied by your professional work very much.

The way of the German publication is depending upon the time of the French one. In addition you will be interested to hear, that the experimental results of the work, that you have in the present paper, have been developed and reconfirmed by additional experiments in the biological field of research in the meantime.

You showed such great interest in the complex of problems and the procedure, that I may presume not to demand too much of your time and effort with this affair. Would you please be so kind as to let me know, if there occurred any problems, technically or thematically, with the translation. I will as well allow myself to send you some samples, as soon as the first Bio-Pictures of the new test series are available.”

Fri. 14 August 36, DuTeil to Reich (correspondence box 10, DuTeil flds) I have your letter, which followed me to Nice, on my return from a vacation trip. Excuse me for the delay on the translation which I have undertaken. I had to take on the organization of a new service at the Mediterranean University Center, a very absorbing task, and courses which were also very absorbing. I have nevertheless almost finished the translation of your remarkable work. And I was able the other day to inform a group of French scholars and academics about the substance of your discovery (a group with whom I was meeting at the Abbaye de Pontigny in the Yonne region, in order to establish a Research Committee concerning the human personality). The interest aroused by your work has been unanimous, and I have even received very interesting information on the possibility of an edition in France as soon as the work is finished.

I needn’t tell you that the scholars to whom I have spoken about you and your work are key figures and people whom I am absolutely sure of in terms of discretion until the work has appeared. If you have any new documents concerning your research and could communicate them to me now so that I may integrate them into the text as I am completing it, to make a coherent whole.

As soon as the work is finalized, I will have a copy sent to you, which I will ask you to proofread, in case you want to make any comments. Then I will make a résumé for the *Psychology Review*, as was agreed upon with Mr. Braunschvig during his trip to Nice, and I will present the complete text to the editor whom they have recommended to me, in Paris of course.

Excuse me for writing to you in French; I am always afraid, due to lack of training, to speak and write to you in incorrect German. Believe me, my dear colleague...RDT

Mon. 7 Sept. 36, Reich to DuTeil, I thank you for your letter of 14 August and apologize that I am only able to respond today. I was traveling and did not get around doing it.

I do not want to add anything to this paper, although I was able to make some important observations in the course of this year, which belong to it. In particular because of the tension-charge formula my work shifted into the field of cell biology, in which some important results have been achieved already.

As soon as I can close this chapter to some degree, I will not fail to send you a copy of the manuscript. It's a question of the relationship of the inorganic and the organic.

Your message about the interest that my work evoked was a big source of joy to me, and encouraged me to move on correctly, through this difficult field. I would be very thankful to you if you would let me know, in which journal you plan to publish the paper.

Reich to DuTeil, 12 Dec. 36 (*Beyond*, pp. 80-81, original in correspondence box 10, DuTeil flds) "I am writing to inform you about the current status of the experiments that have taken place since the work with which you are already familiar. I am almost reluctant to write you so directly and openly because a few months ago it still seemed incredible, even to me, that the results could be correct. After I had succeeded last year in confirming the tension-charge formula in electrical experiments, I set about studying protozoa and colloidal substances. Then in April, on the basis of the tension-charge formula, I was able to produce colloidal structures that behave in exactly the same manner as living cells. I did not want to talk about this to anybody until I had convinced myself by means of culturing tests that I was actually dealing with biological dynamisms. In the last two weeks, I have succeeded in observing the growth of cultures of cell formations that I had obtained by boiling certain substances. I can imagine that at this point you might be experiencing some distrust because the whole thing sounds mystical, and I myself did not believe for a long time that it might really be true. But the facts are so unequivocal that they can no longer be doubted, at least as far as the tests conducted until now are concerned. All the filmable tests and results have been captured photographically (16 mm format), so it will be possible to make copies available.

Two days ago, I successfully demonstrated the experiment for Albert Fischer at the Rockefeller Institute. The substances were mixed and boiled, and my colleagues detected cell-like moving structures immediately after the boiling. Giemsa staining of the preparation was positive. It will now take a long time to carry out the complete set of confirmatory and control experiments. Please be kind enough to inform me whether I might submit a provisional report, through you or directly, to the French Academy of Science for publication. A detailed report on the experimental work conducted last year would then follow in due course.

I would like to take this opportunity to inquire whether the work translated by you has already been published."

Reich to DuTeil, 8 Jan. 37 (*Bion Expts.* p. 61-63) report on the bion experiments to date. (correspondence box 10, DuTeil flds) I take the liberty to send you a sample of a sterile specimen for notion and evaluation by this mail. This specimen resulted from the cooking of substances, which are summarized in the attached arrangement. All precautions in terms of sterilization have been taken. In the last weeks the cultivation of the formations by inoculation of sterile specimen with the dry rest, which remained after complete decoction, was successful. I am preparing a detailed paper about this and send a note such as the enclosed one to the French Academy simultaneously. I request your opinion.

Before giving you a detailed report of my experiments, I should like first of all to tell you something about a heating experiment based on the tension-charge formula. Here I shall merely mention the experimental procedure and the result. A film is being made at the Institute for Sex-Economic Research which will clearly show what happens. At the same time, samples of the colloidal preparation will be sent off.

In order to lay the groundwork for further experiments, and in the light of the large number of findings already available, it was necessary to regard the formula of *mechanical tension* \rightarrow *electrical charge* \rightarrow *electrical discharge* \rightarrow *mechanical relaxation as identical with the formula of vegetative functioning in general*. The following experiment should be carried out to provide proof of the formula:

At this point in my experiments I start by preparing a mixture of 100cc sterile *Ringer's solution* and 100cc 0.1N *potassium-chloride solution*. A solution of *red gelatine* is added to this mixture until the latter turns pale pink. A tiny amount of *coal dust* picked up with a tweezer and an equal quantity of *cholesterin crystals* are added. The entire experiment is based on the principal of bringing together in a certain sequence those substances that are needed for cell synthesis.

We now dissolve a teaspoon of fresh, clear *egg white* in about 50cc of sterile potassium-chloride solution and add both to the previous mixture. The egg white dissolves after being [p62] stirred for a short while. Under the microscope it is not possible to detect any formation of shapes, nor is there any sign of movement or of plasmatic structures. Even at high magnification (1000x) it is only possible to observe the typical immobile crystals of coal and cholesterin.

We now add 1-2cc of milk and some egg yolk. The latter makes the previously clear mixture opaque.

Now a *second solution* is made: *lecithin in salve form is triturated in KCl solution*. At a magnification of 500-900x, we see strange objects developing and growing; namely, *tubes* that swell, bud, and bend. There is no discernible structure inside the tubes, although occasionally there are clusters of vesicles of definite shape. No organic movement occurs. These are purely physical swelling phenomena based on changes in the ratio of internal pressure and surface tension.

If we now add the lecithin solution to the first mixture, the latter *immediately and progressively* turns yellowish-gray opaque. Under the microscope, one is surprised to observe motile life forms: *quivering movement from place to place, buddings, divisions of cell-like formations, jerky to flowing crawling motion*. The organic character of the movement can only be observed at magnifications in excess of 1500x, *but it is most clearly visible at 2300-3000x using a binocular microscope*. If a formation is continuously observed at about 3000x, one notes brightly glimmering, vesicular, nucleated objects which change their shape. It is possible to distinguish four groups of vigorously moving structures: round, nucleus-like *vesicles*; *rods*; round, *cell-like nucleated forms* that move about but do not display any inner motion; and finally, *amoeboid structures*. These last are particularly interesting because at magnifications of 3000-3500x they reveal movements of contraction and expansion. At only 2000x, one can already see them crawling.

The structures formed in this way are, at least according to the experiments conducted so far, *negatively charged bodies*; they migrate to the anode. If the colloidal mixture is allowed to stand six to eight weeks, a thick sediment forms, and the liquid [p 63] becomes increasingly clear. When the sediment is examined at the magnifications mentioned above, it is found that the motility that was described has been lost and the objects are now 'dead.' A report on the control experiments that were carried out will be given in the detailed description. At present, the metabolism and the stainability are being studied. It has been ascertained that *the structures are culturable*. If, in fact, the mixture is boiled down until it no longer contains any liquid and the residue is inoculated with sterile colloidal mixture, growths are produced which so far have been cultured to the fifth generation. The structures, provided they are kept sterile, have proved to be innocuous in animal experiments when injected subcutaneously in mice and guinea pigs.

The question whether these are complete living forms can only be definitively answered in conjunction with other experiments, which will be reported on later, and only after all experiments and controls have been carried out. The described experiment was captured on film.

The artificial, lifelike structures were given the name 'bions.'

Tues 26 Jan., DuTeil examines the bions per Reich's directions, at the laboratory of Drs. Deel and Ronchese in Nice. Ronchese also examines the cells produced and suggests that the movement is merely Brownian. (*Bion Expts* p. 87-88, 89, 96)

3 Feb. 37, DuTeil finishes text of report on early control experiments (*Bion Expts.*, p. 84-9, original in correspondence box 10, DuTeil flds), to the Natural Philosophy Society of Nice, France. D to R (correspondence box 9, fld "English translations"; original in box 10, DuTeil flds) "...I must congratulate you most cordially on the magnificent results of your research. Also, I must thank you for the confidence you place in me by communicating them to me. Please let me know whether you want me to communicate your results to the Academie des Sciences or whether you will do so yourself.

There is good reason to note—and it will be necessary to make it known—that similar results concerning the 'forms' and the 'movements' were already achieved thirty years ago by a French scholar, Mr. [Stefan] LeDuc. But what seems new and essential in your own works is the 'reproduction' by culturing, up to the fifth generation, as you say you've achieved so far. In fact at this stage, nothing more should remain of the initial [inoculating] generation; therefore we find ourselves presented with an essential feature of the dynamism characteristic of life. I should be very happy to be informed of all the details relative to the culturing, which I see as the crucial experiment in your research, and if need be, to be myself put in possession of one of these cultures, along with all the necessary instructions to produce new ones. In Nice, there is amongst my contacts a laboratory which could pursue this experimentation.]

If you have no objection, I would like to give a paper concerning your work in the *Societe de Philosophie de la Cote d'Azur*, of which I am president. The report of the meeting will be conveyed to you.

3 Feb 37, DuTeil to Reich, R's German translation (correspondence box 10, DuTeil flds) "Forgive me that it has been some days before I could reply to your interesting Report. But first I wanted to translate your preliminary note correctly and then watch yourself on the microscope, the preparation that you have sent me.

Therefore I send you herewith a French translation of your note that you possibly in relation to each message you wish to make can be useful. I've closed out a brief report on the observations that I could make your preparations. These observations confirm that your, as you will, in all respects.

So I have to sincerely congratulate the great results of your research. And thank you for the confidence from which you have bestowed upon me by adding them to tell me. Do you want me, please tell whether you wish that I myself take over, the French Academy of Sciences report the results of your work, or if you want to make this announcement himself.

It is appropriate to notice—and it will be necessary to take note of it—that analogous results were in regard to the 'forms' and 'movement' has already received about 30 years ago by a French scholar, M. Leduc. But new and much more in your own

work, the 'reproduction' of culture is, as you have it, after your release, to get into the fifth generation. In fact, in the first stage dessem preparation can not be any left and we befnen well, in view of an essential manifestation of the life of some momentum. I would be grateful of all the details, this cultural concern to obtain release of that culture to me at the core [.] Seems your research experience to be and feel the need to possess even one of these cultures together with all information which makes it to use it to produce new ones. There are at Nice among the compounds that I have, a laboratory that could perform these experiments.

So I thank you most sincerely for giving me this inspiring work has informed and please keep Professor, devoted myself to you sincerely.

Reich to DuTeil, 8 Feb. (*Bion Expts.*, p. 90) WR informs D “that he had methodically cultivated the bions and that he would keep [D] informed of the results because [WR] shared [D’s] opinion that the success of these cultures was of extreme importance for the interpretation of his discovery.” (original in correspondence box 10, DuTeil flds) I have sent the provisional note and the specimen, that you received, simultaneously to the French Academy, biological department, and to a friend in Zurich, who is a physicist [Emil Walter]. So far I succeeded to breed three lines of Bions, the first to the ninth, the second to the sixth and the third to the third generation. But I still have a plethora of things to do with the realization of control tests, so that I will be able to present the matter perfectly. I would also like to point out, that the provisional note that you received, only shows a very small extract of a larger number of successfully conducted experiments about the development of vegetative life. I will allow myself to send you a more detailed report about two other experiments in the course of the next 2-3 weeks, including the exact description of the making of the culture medium. It will as well be necessary, that I report in detail about the difficulties that occurred and will occur in the cultivation of the formations. I ask you kindly to let me know, if you think it is better to have all reports being controlled by you in the first place, or being sent directly to the French Academy. I would prefer the former, but I don’t want to absorb too much of you; There is no doubt that you are very busy. I ask you to write frankly about it. At this point I would only like to foreshadow, what will be presented in the next memorandum in more detail, that the visualisation of earth and charcoal, after a certain treatment, as moving objects under the microscope, was successful. So I kindly ask you to be patient, until I get the second memorandum together with the biological cultures/samples underway. It won’t take too long.

Attached table: description of the attached cultures/specimen

- 1.) V/1 Agar-culture
of two-days-old Bions
clade V, 1st generation
- 2.) V/2 Broth-culture of two-days-old Bions
clade VI, 2nd generation (here the clade specification differs... V or VI? Tina)
- 3.) VI/2 Broth-culture of three-days-old Bions
clade VI, 2nd generation
- 4.) IVb/1 Broth-culture, 5-days-old Bions, cooked for 15 minutes, inoculated immediately
clade IV, 1st generation
- 5.) same culture as in 4.), inoculated after two days

Explanation: V, VI etc. specifies the clade

1, 2 etc. specifies the generation

All cultures descend from the colloid mixture 6b, according to the first memorandum about the making of Bions, and the different clades correspond to in each case newly made mixtures of the same specimen.

Reich to DuTeil, 16 Feb. (*Bion Expts.*, p. 90-91, mailed on 2/20, much of this in Bion lab notebook 2, ms. p. 57-61) includes PS of 19 Feb., accid. dated “Jan. 19” by Reich. “I should like to inform you today of some useful and unequivocal results obtained in my bion culture experiments. I agree with you entirely that the question of spontaneous generation cannot be settled by microscopic findings but mainly through successful cultivation of sterile—i.e., cooked—colloidal mixtures. I am happy to be able to report some positive results to you. The matter is simpler than it seemed to me in all the months of difficult experimenting.

I would be extremely grateful if, as promised in your letter, you would repeat and check my experiments at a laboratory in Nice. I am convinced that this can only help me and I am therefore only too happy to comply with your wish to verify my work. The same thing goes for the cultures.

Last week I succeeded in culturing fresh bion preparations on agar nutrient medium and in broth, and I found that all four types occurred. It was discovered that fresh, heated bion mixtures grow much more slowly and exhibit much less movement in the culture than two-to-five-day-old heated bions. I would recommend that you yourself prepare such a bion mixture

according to the instructions in my first letter. You should let the mixture itself heat for about one hour at 160°C in the dry sterilizer and allow the paraffin-sealed ampoules to stand for three to four days. About two droplets should be taken under sterile conditions with a Pasteur pipette from the sterile ampoule, and the surface of the agar nutrient medium should be coated with this liquid. After twenty-four to forty-eight hours, either a fine bumpy coating of the same color as the nutrient medium or a dense grayish-white growth occurs. I was unable to determine what causes this difference. The results of the cultivation in broth are much clearer. After only twenty-four hours, the broth liquid becomes very opaque and vigorously moving rods, round cocci, large nucleated cells, and, finally, amoeboid forms exhibiting internal motion are visible under the microscope.

Yesterday I cooked a broth culture of bions for a quarter of an hour in the sterilizer and even at 250x in the dark field both the motion and the shape of the formations were retained. I could not believe my own eyes, but we have repeated the culture experiments so often in different ways that there can no longer be any doubt. I am sending you herewith a culture sample. If you should come to the conclusion that my statements are correct, would you please inform the Academy of them. I will now complete the control experiments, prepare a detailed report, and send it to the Academy with a copy to you. [note that the table above seems to refer to these expts, including the one cooked for ¼ of an hour].

PS January [sic, actually February] 19, 1937 I waited a few days before posting this letter because the control experiments yielded some very unusual results and revealed some remarkable facts. However, it seems that it will take a very long time to complete all the control experiments and I did not want to make you wait unnecessarily. The cooked bion mixtures that have stood for two to three days in the incubator regularly produce very strong growth in broth. Fresh mixtures and freshly heated bions seem to be feeble, i.e., the growth on agar is not as strong as after three to four days. It is also confirmed that bion cultures heated for fifteen minutes continue to produce growth. I have not had one single failure in two weeks of repeated inoculation of broth. I must ask you please to be patient and to wait for the detailed report on these findings and on the control experiments until I have brought the latter to some sort of conclusion. Please write and let me know if you want me to send you sterile bion preparations for cultivation testing or whether you prefer to prepare and cultivate the mixtures yourself. I have so far propagated four strains of various bion mixtures in broth but mainly on agar (strains IV-VII). Strains I-IV were grown on egg-white nutrient medium, but this type of medium was discontinued because of its unreliability and the similarity of the substances. I would therefore be very grateful if you would write and let me know the results of your control tests. Judging by the reliability of the findings, there is nothing to contradict the statement that bions can be cultivated in broth. So far, I have only noticed that the rod form predominates in broth while mainly cocci develop on agar. I do not know what significance this has. It is possible that the nutrient medium exerts some influence on the selection of the various types. [He is drawing here on the widespread ideas of microbial pleomorphism from, e.g., Löhnis, Enderlein, A.I. Kendall, Hadley, Theobald Smith, Fleck.] I am in the process of finding out what is behind this phenomenon.

22 Feb. (Bion lab ntbk 2, ms p. 68, MB ts p. 35) New controls: Objection – to refute germ infection.

It has been proven up to now that germs die when autoclaved at 120° - 180° C. The attempt has never been made to compare the composition of autoclaved dry substances to living organisms and their autoclaving.

Experiment:

1. Autoclave the solution and dry sterilize the dry substances at 180°. Then mix together in sterile insulated container.
2. Autoclave the mix at 120°.

ms p. 69 = “!!!!We need not split up the natural process, to overdo it. Rather, only to reconstruct it, to decipher it, to gain control over it—to move it forward.”

Reich to DuTeil, 22 Feb. (*Bion Expts.*, p. 92-3) “I must bother you with a short postscript to my letter of Feb. 20. The further control experiments have yielded such unusual results [bion lab ntbk 2, ms pp. 66-72] that I must stress once more that the experiments involving the sterilization and cultivation of the bions were carried out as follows:

The basic materials which were mentioned in my first report on the composition of bions were heated prior to being mixed and after mixing were placed in glass vessels in a dry sterilizer set at 160°C. The liquids were heated for a half hour to an hour in the sterilizer at 100°C. The cultures were heated for one quarter or one half hour in the same way in the sterilizer set at 160°C. Since it is possible to object that heating for one hour in the dry sterilizer is not sufficient to exclude the possibility of infection from outside or from within by bacterial organisms, we carry out the following experiment:

The dry substances that make up the bions are dry-sterilized for two hours at 180°C. The liquids that are needed to produce the bions are sterilized for one half hour at 120°C in the autoclave and then combined with the dry substances. This mixture is stored under sterile conditions for forty-eight hours before inoculating it into broth. As a second control experiment, we first mix the substances and then autoclave them in the mixed state at 120°C.

The following additional points seem important to me. It is not the aim of these experiments to show that the bions move like living organisms, because that can be clearly established microscopically. The sole purpose of these experiments is to

exclude the objection that the cultivation results are due to infection from outside; that is to say, by so-called air germs. Furthermore, in order to test the accuracy of this objection, I am simultaneously conducting various experiments involving inoculating dust from a vacuum cleaner onto nutrient media. So far, the dust cultures have a different macroscopic and microscopic appearance from the bion cultures.

This is all the guidance that I can give you for the moment. I want to assure you that we are proceeding with all due skepticism, but we are also fully prepared to record what we find, and every possible effort is being made to establish that the culturability of the bions is an *unequivocal* result.”

23 Feb 37, DuTeil to Reich (correspondence box 9, fld “English translations”; original box 10, DuTeil flds) “...First of all, I want to repeat my admiration for the magnificent results of your science and your perseverance. I am very proud of having been chosen by you to be the one to make your work known in France. Its results are still incalculable [unberechenbar]. You know without doubt that the communications to the French Academy of Sciences must obligatorily be made through the intermediary of an Academy member. I am myself in relationship with several scholars of the biology section who are members of this organization; seeing that I translated your communications, in entirely scientific French, it seems to me that it would be preferable in all respects that you leave me the job of contacting one of these biologists for the presentation of your works to the Academy. Don’t worry about wasting my time. I shall be most happy to do this. There is another advantage to this approach: my personal observations, which I am going to make here in an official laboratory under the control of the Philosophical Society, could be joined to your own observations and thus bring them valuable support. This is why I think it also preferable for you to enable me to produce the bion preparations here myself, so that the evidence we bring may have indisputable value.

This does not prevent me, moreover, from pursuing at the same time a double culture testing, like that for which you have given me the means and the instructions. I also received the brochure in which your works on the electrical measurements of emotions are summarized, works for which I had a ms that I didn’t have time to finish translating, but which I’ll return to you at Easter vacation.

Anyway, I’ll present a first communication about your works Sunday 7 March at the Philosophical Society of Nice. I shall send you a report of the meeting. Colleagues with whom I have spoken are all very deeply interested in the question. Of course I shall remain in a close relationship with you all during the experiments to ask your advice as well as to inform you of the results.

25 Feb. 37 (Thursday), Reich telegraphs DuTeil (Bion Expts p. 93), that the cultures were successful and had been sent off.

27 Feb. (Saturday), DuTeil received a first shipment of cultures, together with a set of instructions, which was followed a few days later by a second shipment. 27 Feb. letter, WR to DuTeil (*Bion Expts* p. 94), enclosed with shipment and with a second report on the culturability of bions (report found in OI box 8, “Report on Cultivability of Bions, 1937” fld, 4pp, see below*: “As I told you last week in my telegram, the autoclave experiment was successful. In the enclosure I am sending you some samples of *autoclaved* bion mixtures and a second interim report on the positive result. Also enclosed are instructions on inoculation; a description is given in the report.

May I ask you to send some of the cultures—I will leave it to you which ones—to the Academy in Paris and also to let them have the second copy of the ‘interim report.’ I am very anxious to learn whether you were successful in producing and cultivating bions in accordance with the instructions given in my letters. In particular, I would like to draw your attention to the strange forms of agar-amoebae which I find astonishing.

* Zweite vorläufige Mitteilung aus dem Internationalen Institut für Sexualökonomische Forschung, betreffend Die Kultivierbarkeit der Bione von Dr. Wilhelm Reich (Oslo)

The following should only contain a synopsis of the facts about the cultivation of bions (compare to first report from 01.10.37), which, based on the control trials completed up to now, may be viewed as verified. I am thinking ahead with this second preliminary report presuming a very detailed description of all cultivation experiments, including control trials, and reporting only the positive results.

Following many productive yet not completely successful attempts at cultivating on egg-white yolk culture medium, bion cultivation on bouillon and agar succeeded initially in the following manner:

A test-tube full of the bion mixture (compare to first report) was placed in a dry sterilizer for half an hour and boiled at 160⁰. The cooked mixture was stored sterile two to three days and then inoculated on bouillon and agar medium using the following method: A drop of the mixture was transferred after heating the pipette and the opening of the test tube to incandescence. Already after 24 hours, the bouillon-culture from the three different bion mixtures produced regularly and without exception a very dense, partially flaky substance. A mostly connected, grayish-white, thin film was discharged on the surface of these

bouillon cultures. Under a microscope, one could differentiate [2/15]

- a) a cloudy density
- b) flakes
- c) a surface film at times
- d) by shaking the test tube, a fine string-like rising, dusty cloud that increased the haziness.

The growths on the agar medium were macroscopically less distinct. Often the results after 24 hours consisted of a very dense, yellowish-white growth; however, in most cases there was only a very thin, bumpy monochromatic growth that solidified after a few days.

In microscopic examinations all of the bouillon cultures produced essentially fast moving rods of different types, cocci, but very few amoeboid structures. The thick agar growths consisted primarily of microscopic cocci, very active and inducing position changes. Under the microscope, the weaker agar growth shows a lot less movement. However, if one further cultivates the weaker agar growth in the 2nd strain in bouillon, this produces the same structures in the bouillon culture and has macroscopically the same appearance as the one inoculated directly onto bouillon. It may be that the culture medium has a specific influence on the development of the cultures, although this must first be thoroughly examined. The inoculation from bouillon onto agar regularly produced a thick growth within 24 hours. Strangely enough, cocci were predominate in the agar growths by the repeated inoculation from bouillon onto agar, while recently the rods and amoeboid structures emerged by the re-inoculation onto bouillon.

To assure that the cultivation attempts were unambiguous, I turned to cultivating autoclaved bion mixtures. The following trials were carried out: The bion mixture was created using the method described in the first report with the one difference [p.3/16] that the charcoal was first grated very fine, boiled in KCl, and then added to the first mixture. The solution was filled into two test tubes and put into the autoclave. The autoclave was set at 120^o, and both test tubes were autoclaved at 120^o for half an hour. After 24 hours, solution was taken from one of the two test tubes and used to inoculate one bouillon culture medium and one agar culture medium. Both resulted in growths; however, much more pronounced on bouillon than on agar. As of now, the first autoclaved bion mixture strain on bouillon has been extended to the third strain only on bouillon, and each time results have shown it contained hefty growth, moving rods, cocci, and amoeboid. Simultaneously, an inoculation onto agar medium was made from the first bouillon strain. After only 24 hours, a microscopic examination of the agar medium showed dense, yellowish-white growths, which, to my great surprise, contained mostly, animated amoeboid formations. At a magnification of 3700X, one could see vigorous particle vibration inside the amoeboid formations, which slowly moved around and simultaneously contracted and expanded. Strain 2, obtained using the same methods, was re-inoculated onto agar and recently produced macroscopically and microscopically a dense, homogeneous growth. A strong structure of the amoeboid formation and their roughly even size were very prominent and positive results. At a magnification of 3000X over a long period of time, one could clearly observe repeated division of the [cell] formation.

The autoclaved bion mixture from the second agar strain is now re-inoculated in bouillon medium. Recently a dense, cloudy growth appeared in bouillon in which, to my great surprise, there were a far greater number of rods and cocci than were present in the agar culture. The main types [p 4/17] of rods are all represented. Found again, this time in all existing cultures, were club-formed, rod-shaped, short, and long bacteria rods. All of them moved from one place to the other more or less quickly with jerky, serpentine, or tumbling motions. There were not as many amoeboid formations in the bouillon culture as in the agar culture. The amoeba in the bouillon culture showed internal vibration, expansion, contraction, and creep movement. Noticeable was the following result: While the fresh bion mixtures primarily moved toward the anode or were neutral, an electrical examination of the 2 strain onto agar showed a strong attraction of the formations towards the cathode, the cause and essence of which cannot yet be clarified. During eventual analysis of the results reported here, it is important to note that two to three day old sterile bion mixtures prove to be more robust than very fresh mixtures; however, everything points to the fact that autoclaving most likely boosts cultivability. Only further cultivation experiments can answer these questions and more about further details of bion research.]

2 March, DuTeil telegramm to Reich (OI box 7, bion lab ntbk 2, ms. p. 91) “Ai bien reçu echantillons et lettres STOP Remerciements STOP Experiences commencent STOP Ecrirai incessament. RDT

6 March 37, Reich to DuTeil (correspondence box 10, DuTeil flds) I very much appreciate the suggestions you made in your letter from February 23 and welcome them most sincerely as a sign that one must not necessarily stand alone simply because one has come upon results that sound pretty unbelievable. It would be most beneficial if an official French laboratory, under your direction, could replicate the initial experiment, control the parameters, and perhaps produce additional results that I may have missed.

I already started experimenting with animals this week using participating laboratories, which we established within the past three months. I will submit the results to you as soon as they are available. We first check our work relative to cancer. I believe

that in previous experiments I was able to isolate phenomenon that justify the assumption that a very close and unusual relationship may exist between them.

If you wish to keep the cultures which were sent to you to work with, as you indicated in your letter, then I suggest, based on past experience, that you grow the samples every six to seven days. We have not yet determined how long the bions in the cultures will live without regular inoculations. I am especially interested in further cultivation of the unusually fast-moving amoeba, for we have only observed them once while autoclaving the bions, and I am not yet certain whether this was merely an accident or a characteristic behavior.

I would be most grateful if you would also send me samples of your completed bion mixtures and cultures. It is very possible, in fact most likely, that there will be large variability in the bions. I, myself, have already (produced) four different varieties through various cultivation methods in pure culture.

I don't know whether you noticed my comments in the notes stating that I did not completely dissolve the carbon in the first mixture, rather used a tool to grind it into a fine power and then boiled it in KCl. This seemed to increase the activity of the bion mixture. This is, though, only speculation. I will be most appreciative if you send your organization the report.

The second autoclaving experiment was a success. We produced cultures from autoclaved bions; however, this time the bions showed no pure amoeba cultures. I, thus, do not know whether the production of amoeba from autoclaved bions was a coincidence or whether we made an undetermined error this time.

If you cannot see a thick, yellowish-white growth on the Agar within 24 hours, then a very fine, mono-colored, uneven growth will appear in about 5-6 days, which, if you continue to inoculate it in bouillon, results in the usual bion culture. Please be aware of this **to ensure an exact replication.**

Enclosed within are:

- 1) A second-generation culture in bouillon inoculated with autoclaved bions.
- 2) A second-generation agar culture of bouillon re-inoculated with agar.
- 3) A bouillon culture derived from agar cultures re-inoculated back to a bouillon mixture.

Explanation of symbols: 6a – ungekochte Bione (raw bions), 6b – gekochte Bions (boiled bions), 6c – autoklavierte Bione (autoclaved bions)

7 March (Sunday), DuTeil presents his report of 3 Feb to the Nice Natural Phil. Soc., along with the update addendum in *Bion Expts.*, p. 89-98. (p. 94-97) “In addition, you have here the cultures themselves and you can examine them at least macroscopically. Let me tell you that the agar culture, which preferentially assumes this undulating shape, has undergone very strange development during the few days that it has been in my possession and it grows as one observes it. Remembering that the constituent elements were sterilized at 180°C, one is dismayed and rather inclined to doubt the efficiency of the sterilization methods currently employed. I say this without in any way wishing to offend our dear member from the pharmaceutical field nor any of our honored members!

This brings me to the end of the objective listing of the facts. Before I hand over the discussion of them to you, however, I would like to add some personal views of my own.

There are two ways of considering these experiments. We can concern ourselves either with the facts or with the interpretation of the facts.

Likewise, the facts themselves can be considered from two different angles. On the one hand, they describe the formation of organized structures from non-organized material, and on the other, they indicate the resistance of these structures to destruction by sterilization. Let us also note something that these two sets of facts have in common: the non-organized elements used in the experiments were sterilized beforehand and re-sterilization was carried out at each stage in the experiment, so that at each stage it is possible that the non-organized state was restored. Furthermore, this permitted a multiple number of checks to be carried out; with each new cooking, a completely new experiment was performed, establishing whether there was a transition from the organized to the non-organized state. In fact, when I questioned specialists on this matter—and I ask the specialists here today either to confirm or to disprove this information-- I was told that, according to the present state of biological knowledge, there is no known microorganism that can withstand a temperature of 180°C. At the very least, Dr. Reich's discovery has shown us that organisms which display all the characteristics of life and which can withstand these temperatures do exist. Now each of us can confirm this discovery with the aid of the bions and the bion cultures which Dr. Reich has sent me.

While we are discussing the facts and their interpretation, we should consider a number of other views relating to the endogenous or exogenous origin of the microorganisms discovered in this way.

As you have already heard, Dr. Reich has two ways of refuting the statement that the bacteria are organisms from the air. First, in the same way that he answers the other objections, he sterilizes the material at 180°C, a temperature which no known bacterium can withstand; second, he takes the atmospheric dust from a vacuum cleaner and cultivates it. The cultures obtained in this way have nothing in common with the cultures which Mr. Deel yesterday described as obviously 'pure' cultures.

The objection that the culture medium itself might contain bacteria can be countered first by pointing out that the bacteria would not develop just on the inoculated region. The second argument is again the sterilization; and the third response is to point out that such bacteria would be polymorphous and would not present the appearance of a 'pure' culture. Having thus presented the facts in a favorable light, we come now to their interpretation, which we expect to see confirmed unconditionally in the near future. Two objections have been made to Dr. Reich's hypothesis that these are truly living organisms, although he himself has not yet stressed this latter point.

The first objection states: What we have here are nothing more than electrical and chemical processes which exhibit movement similar to Brownian movement (this is the chief objection put forward by Dr. Ronchese). Reich's answer is to demonstrate the culturability of the formations.

The second objection, raised by Mr. Deel, is that lecithin is a living substance. In his view, therefore, Dr. Reich has not discovered the missing link in the chain of development from the inorganic to the organic but has merely succeeded in 'organizing' a living but still unorganized substance. One can counter this objection by pointing out that lecithin in egg yolk is only a nutrient, while the life force is probably located in the germ cell of the egg. The process of obtaining lecithin, stirring it, dissolving it in ether, purifying the solution with zinc chloride, which forms a practically insoluble double salt from which lecithin is regenerated by hydrogen sulfide, certainly gives the impression of being nothing more than a chemical process in which there are no signs of vital dynamism. And even then, the possibility of life would have to be excluded by the action of sterilization, at least according to the present state of our knowledge. Even if lecithin was something other than the material from which the yolk is composed, it would be killed by this sterilization.

Furthermore, if the objection was made that these are electrochemical processes which completely and perfectly imitate all the manifestations of what we call life, we would have to reply without a doubt: if two triangles prove to be congruent, which is to be regarded as the first and which the second? Which would be the true manifestation of life and which the false? Let me conclude this report, which already contains quite a lot of scientific material for discussion, with a brief excursion into the realm of metaphysics. Let us presuppose that we are in fact able by electrochemical means to construct unicellular organisms which exhibit all the characteristics of life *sensu stricto* as we have conceived of it so far. Perhaps then we should stop regarding life as being manifested by the material phenomena of movement, feeding, and division, and instead see it as a progression from a germ to the organization of differentiated structures corresponding to a type or a genus. And is it not the result of these experiments, which seemed to make us inclined a priori to choose a materialistic solution to the question of life, that we are obliged on the contrary to see life as having an 'organizing intention' and to assign it radically to the realm of the spirit?

In practical terms, I believe that our involvement and the interest that we show in this discovery cannot fail to be of great service to the cause of science by enabling Dr. Reich to make his discovery known in France at the Académie des Sciences. It would also help if we acknowledge the accuracy of his experiments and draw his attention to any errors we might find in these experiments or the interpretation of them. Either way we would have made ourselves useful.

Following the discussion, I would therefore like to call on you to appoint several members to assist me in the series of control experiments which I intend to undertake in order to confirm Dr. Reich's results.

[p. 98] PS. At the end of the discussion, the Philosophical Society, in acknowledgment of the obvious interest generated by Dr. Reich's studies, regardless of the interpretation to be put upon them, appointed the following members to assist Professor Roger DuTeil in his task of verifying Dr. Reich's work: Dr. Charteir; Dr. Perisson; Miss Femand, Associate Professor in Natural Science; and Mr. Claude Saulnier, Pharmaceutical Chemist. In addition, it is the wish of the Society that a large French laboratory—in particular the Lumière Laboratory in Lyon—be entrusted as soon as possible with this matter.

Meeting held on Sunday, March 7, 1937"

13 March, DuTeil telegram to Reich (correspondence box 10, DuTeil flds) "Vous prie envoyer urgence par avion formules composition detailee et titree vos milieux de culture agar et bouillon STOP Je entreprend experiences laboratoire officiel."

14 March, DuTeil to Reich, Reich's German transl., As my telegram from yesterday already indicated, our business is developing very well. Following difficult experiments under poor working conditions, I have finally received permission to personally conduct the experiments in the official Nice Health Department Laboratory (Service de l'Hygiene) supported by the lab's director. As soon as we have achieved positive results using the bion cultures sterilized at 180⁰, we have the intention, if it is alright with you, to officially approach the Pasteur Institute in Paris and at the same time, if possible, Mr. Lumiere's Laboratory in Lyon, the best equipped in all of France. Going this route will provide all the status and at the same time create the echo that you deserve. I have a binocular microscope available here at the Nice Laboratory that can already magnify to at least 2000X and has the capacity for further upwards adjustments—a totally new and also modern piece of equipment. Furthermore, we have access to Dr. Ronchese's laboratory for specific observations using an apparatus that magnifies more than 3000X. Besides that, I also have an 180⁰ sterilizer and two autoclaves. It is important that we have exactly the same environment for the cultures

as you. This is why I telegraphed you and asked 1) for the exact formula of your different culture mediums, including the peptone ratios, etc. I have also been asked to request an explanation from you regarding the gelatine from the first mixture used to grow bions. 2) Why red gelatine? 3) And what is the formula of the Ringer solution?

Regarding the gelatin, a member of our society, a physician whom I notified to be present on March 7, reported that during his time as an assistant doctor in Paris he had seen several cases of infections caused by gelatin, for example when treating hemorrhages, although the gelatin had been sterilized [prior to usage]. He asked me to inquire if you know something about this, in other words, if the sterilization techniques used on the gelatin are now so advanced that you can guarantee, in other words, if you are certain, that germs could not get into the mixture via the gelatin. He did, however, recognize that, should this be the case, you had the prodigious credit of discovering and cultivating germs that were, until now, unrecognizable and invisible to us. I would be very happy if you would tell me (4) what you think about these questions.

I am sending you the report that appeared in the paper concerning the Philosophical Society Conference where I spoke about your work in addition to the text of my statement. You will notice that I have discussed the questions with great prudence, as it was advised (comme j'y étais tenu). A committee was appointed and tasked with assisting me with the control experiments. As soon as they are accomplished, I will approach the Academy of Science in Paris.

In addition, I would be delighted if you would let me know whether you put sealed ampoules in the sterilizer (5) and whether they ever burst.

Finally, I would like to request that you give me exact, detailed particulars about the necessary materials for the growth mixture (Perhaps you could send me a small sample of the material) (6) and, if necessary, include the brand type (brand name) of the material (i.e. the charcoal, because there are many varieties), in short, information about everything that will allow me to conduct the experiments under the exact same conditions as you did.

I will keep you informed of all details and also send you samples of the results.

PS I request that you answer my 6 questions very precisely [numbered by DuTeil in the text above], as that is extremely important for our work.

[from French original: As my telegram from yesterday must have made you presume, our business is going very well. After difficult attempts under conditions of defective equipment, I finally obtained authorization to pursue the experiments myself with the help of the Director of the Laboratory, in the official Laboratory of the Service of Public Health of the City of Nice. It is understood, if you are in agreement on this subject, that as soon as we have obtained a positive result concerning the culture of sterilized bions at 180°, we will get ahold of the Pasteur Institute in Paris officially, and concurrently, if that is possible for us, the laboratory of Mr. Lumiere at Lyon, which is the best equipped in France. In this way, we will devote all the necessary weight to our research and all the attention which it deserves.

I have a binocular microscope here in the Nice Laboratory at my disposal with the capacity of at least 2000x magnification, and whose magnifying power we can increase. It is an all new and therefore modern apparatus. In addition, if need be, Dr. Ronchese's laboratory will place an apparatus at our disposal for several observations, an apparatus magnifying more than 3000x. I have in addition a sterilizer to 180° and two autoclaves.

The important question is to have exactly the same culture media as you. That's why I asked you for the exact formula of your different media, with the proportions of peptone, etc. Moreover, they asked me to ask you for explanations on the subject of the gelatin of the first mixture that generated bions [Prep. 6]. Why red gelatin? Also the formula of the Ringer's solution?

On the subject of the gelatins, a doctor who belongs to our [Nice Nat. Philosophy] Society and who attended my 7 March lecture informed us that formerly when he was an intern at Paris, he often noticed cases of infection by gelatins used, for example, as anti-hemorrhagic agents, gelatins that moreover, were sterilized. And he asked me to ask you if you knew anything on this subject; in other words if the technique of sterilizing the gelatin had made sufficient progress since that time, sufficient progress so that we could be certain about it and if, in sum, you were certain that by this process there might not have been germs introduced into the mixture. He recognized, moreover, that even in such a case you would have the great honor to have discovered and cultivated germs unknown and until then invisible.

I would be pleased if you would tell me what you think about this question. I am sending you the account which appeared in the press of the meeting of the Society of Philosophy where I spoke about your works as well as the text of my lecture. You will see that I discussed the question with the greatest of prudence. A commission was named, which has as its mission to assist me in my control experiments. As soon as these have succeeded, I'll get ahold of the Academy of Sciences in Paris. I would also be pleased if you would tell me if you place the sealed tubes in the sterilizer and if you ever have any breakage.

Finally, I'd like you to give me very exactly and in detail the products necessary to obtain the generating mixture of the bions, and even if need be, the brand that you buy (the coal, for example, because there are several kinds); in brief, all that would allow me to find myself for the experiment in conditions as analogous as possible to yours.

I'll keep you informed with a detailed report. Also I'll send you samples of the products obtained. I ask you to believe, dear professor and colleague and also dear friend, in my most cordial sentiments. RDT

Perhaps you can send me a small sample of these products. [illegible sentence]

Reich bion protocols (n.d.) (correspondence box 10, DuTeil flds)

Details of the charcoal and soil experiments:

1. Charcoal and soil is finely grated either with the fingers or mechanically using a tool in a basin.
2. Sterilize in a dry sterilizer for one to two hours at 180°.
3. 0.1 potassium chloride autoclaved in a test tube at 120°.
4. Add a tip of a spatula full of sterilized soil or charcoal to a test tube filled 3/4 with potassium chloride, boil in a dry sterilizer until a cloudy, uniform, colloid-type solution forms.
5. Examine under a microscope immediately after boiling.
6. If the majority of particles do not move, boil again.
7. Note whether the particles immediately sink or remain in suspension at least for a few minutes after removal from the dry sterilizer.
8. Culture trial: 2-3 drops from the colloid-type solution on meat bouillon.

Details for bion creation

1. First dry sterilize charcoal at 180° and then boil in potassium chloride until the solution is uniformly murky.
2. Autoclave gelatin-, Ringer-, potassium chloride- solution.
3. Separate egg white from egg under sterile conditions.
4. Create first mixture.
5. Autoclave lecithin, do not dry sterilize, because it changes the consistency. Prior to boiling, dissolve in sterile potassium chloride, and then add it to the mixture.
6. From this mixture, dry sterilize 5 test tubes or an open enamel dish.
7. Inoculation test. One immediately and one after 2, 3, 4, and 8 days.

17 March 37, Reich to DuTeil (correspondence box 10, DuTeil flds) Late yesterday evening I received your telegram in which you requested the exact recipe for the bouillon and agar cultures. I am responding with a telegram today and will give you a detailed description of the bion culture experiments. My main concerns are effectively communicating the problems we encountered and are attempting to solve, as well as sufficiently emphasizing those points which serve as proof of the bions authenticity. Likewise I will include a description of our completed control experiment and the results to save you time and effort.

1. Concerning the culture medium: As I mentioned in my second message to you, I was able to produce four unique mixtures resulting in multiple strains by directly inoculating the bions and then cooking them for thirty minutes using dry sterilization. At that time, I sent you several of those bion cultures. The bouillon and agar media were purchased from the "Government Institute for Health", as I had no personal facilities to produce my own. Each of the cooked cultures grew and could be successfully used to breed further cultures. In the same bouillon media, made from meat, (not from extract), the one-half-hour autoclaved bouillon culture grew immediately and promptly. This first generation strain of autoclaved bion produced in broth (not meat extract) strongly induced amoeboid structures, which, when inoculated with agar, resulted in the yellowish growth culture samples that I sent you. In this culture, we were able to produce a pure culture of amoeboid contractile structures for the first time.
2. From this point on, we produced the culture mediums ourselves. The bouillon medium is now, totally unintentionally, no longer created using meat, rather meat extract. We autoclaved six more strains, giving us a total of seven strains. We were then confronted with a situation, which, at first, made no sense. Although the first autoclaved strains of bouillon cultures grew promptly within twenty-four hours and produced a thick growth composed of all bion forms, and we were able to grow further cultures, given three bouillon-inoculated cultures, one would reproduce as well as the first generation culture, while the other two samples would present with much more limited growth. When we re-inoculated the intensely growing second-generation bouillon culture, the expected vigorous reproductive growth reappeared. However, a re-inoculation of the weaker, cloudier cultures produced no further growth. We do not yet understand why this occurs. Of the seven autoclaved bion mixtures, we are absolutely certain that three of them were successfully grown, but it is questionable whether the other four were, too. It is possible that the difference in the way the bouillon is made (meat vs. meat extract) influences the probability of reproducing the desired culture growth. We will control for that parameter in our next experiment. To assure the validity of the experiment, I am sending you some test tubes with both the bouillon and agar culture mediums and

including a detailed description of how I used them to create both the four cooked bion strains and the family of rapidly growing autoclaved bions. I am attaching an exact explanation on how to re-create the bouillon that we produced in our labs and used to grow the second to seventh strains of autoclaved cultures. Likewise, I will airmail you the exact recipe for the creation of the meat broth we initially used tomorrow.

3. I was able to produce the pure amoeba culture only once, the day before yesterday, using the first strain of the autoclaved bion culture. That result was not replicable for the next five strains, yet appeared again in the seventh strain of the autoclaved bion culture in bouillon just as it did in the first autoclaved strain. We further inoculated them on agar to determine whether the yellowish, pure amoeba growth would present.
4. We experienced the following results:
 - a. If the typical robust growth is not present after twenty-four hours in bouillon, then we let it (the culture) stand 8-10 days in a heat-controlled environment and then inoculate a secondary culture on agar and bouillon with very positive results. It is not one hundred percent certain whether a slight film will appear when inoculating a secondary bouillon culture.
 - b. If no measureable autoclaved bion mixture growth is apparent on the agar after 24 hours, then we let it stand 8-14 days in a heat-controlled environment. Usually a matt, cohesive, slightly contoured growth appears that does not deviate in color. From one such growth, re-inoculated in bouillon, we noticed clouding after a day and the formation of a film after two days.

I have only mentioned a few of the uncertainties, some of which we could overcome. I would like to summarize what I consider to be definite results of the completed cooked-bion, cultivated-bion, and inoculated secondary culture experiments and what I must consider open to question.

Guaranteed is:

1. That, immediately following the combination of the two solutions, one consisting of lecithin and the other a mixture of protein, gelatin, carbon, cholesterol, milk, Ringer-solution, and calcium chloride, the bions in their completed form present as moving coccus, moving rods, and contractile amoeboid structures.
2. That movement and amoeboid contractility of the structures are present while cooking in a dry sterilizer and autoclaving at 120°C for 30-60 minutes.
3. That the cultures of the cooked as well as the re-autoclaved bion mixtures were, for the most part, successful. Doubts exist where attempts at cultivation were either incomplete or did not succeed using the same mixture, although others did succeed. We have yet to determine why this happens and what causes it.
4. One could object that bions are developed as products of spores found in either the carbon or the gelatin.

We completed the following control experiments to test that hypothesis. Thus far, we tested each substance in the bion mixture individually by autoclaving for thirty-minutes at 120C until we wished to proceed further. Both times the autoclaved individual substances obtained from the first strain failed to produce culture. This behavior should be tested at least ten times in order to be completely certain that it is a valid behavior. One would then have solid evidence that the bions in the culture did not develop from presumed carbon spores. The most exact evidence against the development of bions and their cultures from so-called spores, as I believe I have observed, comes from the fact that the complete bion structures are immediately present following the combination of the two mixtures.

I intend to complete a sequence of experiments to answer the question concerning the origin of the bacterial and protozoa spore structures. Right now I would like to hypothesize that, although the presence of spores cannot be denied, the question concerning their origin must be answered through further experimentation. I believe at this time that the spore theory must be correctly interpreted in order to simplify my work. To accomplish this, I have already completed experiments, which were repeated for several days using soil and carbon, and I consider the results totally reliable.

1. If one puts soil crystals in a soaking solution, e.g. in 0.1 normal KCl-solution, and then observes the reaction daily for 6-12 or even 14 days, one will see that the previously unstructured soil crystals start to form a vesicular structure. I previously dry sterilized the soil for one hour at 160C and boiled the KCl for approximately thirty-minutes.
2. If one puts the soil in a KCl-solution and cooks it for approximately thirty-minutes in a dry sterilizer or autoclaves the soil-KCl-solution for a half-hour, a microscopic image shows an enormously agitated image, much more agitated than in the non-boiled preparation, immediately following this sterilization process

If one understands the Brownian Movement to mean the on-the-spot vibrational motion of bubbles without change in position, no flowing, spiraling movement, and no movement due to contraction, then we have different Brownian Movement phenomena here. The most unusual of these is the fact that, after about 5-8 days of soaking, soil as well as carbon crystals show an almost complete, constantly progressive, vesicular decay.

At this point, I must add a personal observation. I am very reluctant to simply describe what I have observed without further explanation. This sounds unbelievable, but it is, though, a fact.

If one watches the soaked and bubbly, decaying soil or carbon that were previously cooked or autoclaved over a time period of several days, one sees that the carbon as well as the soil crystals gradually change into heaps of bubbles, some of which creep

along very slowly with jerky motions. I have observed this hundreds of times, could not believe it at first, but then I could no longer question what was happening. At a 300,000 magnification, one could clearly see the movement of the vesicular, loose carbon and soil crystals, both self-contained and in situ.

I am sharing these observations with you, Professor, to explain my reluctance to announce all of my findings at one time. They are initially very confusing. However, I have learned to examine things again and again, and I am convinced that there is a vesicular decay of these substances. I am currently filming the experiment and accelerating the process 57,000 times. I will send you a copy of the film as soon as it is completed.

As you can tell, I have had to contend with immense and lengthy factual findings. Carbon and soil experiments seem, in my opinion, essential testing materials to understand the creation of bions. I am more than willing, if you wish, to include more details of the control experiments mentioned here. I would like to add that I have on-going experiments with cooked and autoclaved dust, the results of which I will be sharing shortly.

2 pages of tables ***

bouillon-culture media from meat extract

culture media for strains II – VII

½ % Liebig meat extract

1% Witte peptone

½ % NaCl

approx. 7 cc NaOH (regular)

Steam-cook all ingredients for about an hour. The bouillon should be adjusted to pH 7.4. Replace the condensed water with fresh water to prevent the mixture from becoming too concentrated. Afterwards, autoclave at 120C for about thirty minutes. The following day steam-cook the entire mixture again for thirty minutes.

Agar culture media

½ % Liebig-meat extract

1% Witte-peptone

½ % NaCl

2 ½ % agar strands

approx. 10 cc NaOH (regular)

Adjust the mixture to pH 7.4 and autoclave at 120C for one hour. If you have used more than 4 cc NaOH, then you must autoclave the mixture for another hour. Replace any condensed water with fresh water. Afterwards filter the mixture in a steam bath, fill it into bottles, and autoclave at 120C for an hour. A day later cook the entire mixture in a steam bath for another thirty minutes.]

22 March, Reich to DuTeil (correspondence box 10, DuTeil flds) [***8 pp] I would like to thank you from the bottom of my heart for your letter with the report in the Society of Philosophical Nature and also thank you for your extraordinary friendly yet, at the same time, objectively correct assistance. In these times of murky, scientific, and seldom conducive pioneer research, one does not often find this type of outlook, and after 15 long years of hard battle, I am all that more grateful to you.

I am thrilled that an official laboratory will assist in the control experiments, that these will take place under your leadership, and that such a, as I believe, good cause is being promoted.

*I am very concerned by which means we can coordinate both labs for parallel research, so that no confusion comes up and the controls [trials] can really be productive. Please allow me to initially group together the main problem areas to consider, at least in my laboratory. I have only described one experiment to you; however, it was the main experiment. But there were a series of experiments prior to this, which I conducted in the months of February and March of last year. They first led me to the bion-boiling experiment in May, 1936. I repeated all experiments without exception until Oct. 36. After I had verified that the basic observations were correct, I began the culture experiments in Oct. I already had the results of these cultures, as I mentioned in the first report I sent you. I tested and developed these from October to February. Since my second report concerning the cultures, I have concentrated my lab work on the control experiments, which should try **to explain** the relationship between bions and known bacteriological sterilization and should clarify the Germ Theory.*

Summary:

1. Experiments regarding the creation of bions.
2. Experiments concerning their cultures.
3. Experiments which test the expected objections against bions and which could eventually refute them, especially the Spore Theory.
4. Experiments regarding bion functions as compared to the known forms of protists.
 - a. Electrical reactions
 - b. Effects on animals

- c. Effects of bions on other bacteria and especially on cancer cells
5. Collection of old data from literature to facilitate the interpretation of bions.
To achieve this, I, together with friends, furnished a functioning lab in which, for 6 hours a day, the experiments that I undertook in my private laboratory at home are tested and completed in mass. Working there at the present are:
1. An assistant completely trained in bacteriology
 2. A doctor colleague for animal trials
 3. A chemical-physical trained assistant for pH determination and examinations with electricity.
 4. An assistant is creating a card catalog of all literature.
 5. An assistant to microfilm and gather all important phenomena.

Important for microscopic examinations are new microscopes, which I ordered from Reichert in Vienna, in addition to allowing comprehensive experiments in dark field as well as magnifications up to 4000X (model 1936).

The tasks have become very complicated over these past years. They were simple in the beginning. The controls regarding the function and nature of bions require the knowledge of the preliminary researchers. Without wishing to influence their viewpoint, I believe that the completion of these preliminary trials simplifies a lot of the bion work and can especially prevent the confusion brought on by some preconceived notions. The focus of my work here is not how one can kill life, rather how one can maintain it or evolve it while adhering to the strict rules of sterilization. I have written down the exact steps of my experiments from years past and wish to give you a synopsis of the preliminary trials and the control experiments.

1. I started guided by my vegetative formula: mechanical tension (swelling)-electrical charge-discharge-relaxation_by swelling soil.

A tweezers' point full of regular garden soil is added to about 5 cc of 0.1N potassium chloride. Using a cotton stopper, close a test tube and put it into a dry sterilizer set at 160-180°. Let it cook for one, sometimes two hours. At the same time, I put a few granules of completely unsterilized soil from the same sample added to water onto a specimen slide. I also compare the non-sterile to the cooked. The cooked soil shows endlessly more vesicles and rod-shaped structures than the uncooked. The cooked crystals show a total, vesicular decomposition in dark field at a magnification of 300X.

The uncooked show far less vesicular structure, mostly along the edges. I allow both samples to rest for a few days and make daily parallel examinations. During three to six days, the cooked soil decomposes completely vesicular in the form of cocci and moving rods. If one adds a bit of red gelatine to the cooked soil after one day, the cocci and rods pool to amoeboid, creeping structures. It is necessary to use red gelatin in order to judge by the pink color whether too much gelatin has been added. Most extraordinary is the fact that, after 6-7 days, the soil granules morph into contractile, sometimes creeping amoeboid structures. One cannot clearly see this at a 2500X magnification. One often sees a piece of crystal that is still preserved, while another piece of the same crystal very subtly moves away, bends, stretches, etc. I have filmed this process for about four months and have now compiled the process as well as the decomposition and the morphing into amoeboids. The uncooked soil also completes this process, but much slower and not as noticeably. One gets the same results by autoclaving the soil.

What is created with soil happens in exactly the same manner with charcoal. The cooked charcoal always shows more movement than the uncooked. This proves the first part of the formula, the mechanical tension or swelling.

[p.3] I discovered the second part of the vegetative formula, electrical charge, almost by coincidence in this manner: unswollen soil and charcoal crystals are for the most part almost completely neutral. They were not in the electrically galvanized current. On the other hand, charcoal and soil cooked in potassium chloride or soaked for a long time moved towards a cathode. The dissolved materials are electrically different. I am sending you a table with absolutely certain, repeated, completed and tested results. Strepto- and staphylococci are positively charged, change their direction and polarization after some time, become negative. I declared the developing movements a result of the influence that electrically charged particles, vesicles, have on one another. It is important that the larger vesicle clusters are often neutral. The individual vesicle always migrates.

2. I examine a lot of material: all the material in the bion mixtures and a lot of others, cooked and non-cooked. It turned out now that all matter without exception, some from the beginning such as egg yolk, others through cooking such as soil and charcoal, consist of round, cocci-shaped vesicles when observed in a dark field. This discovery will probably become very important when evaluating the Spore Theory. It does not appear to be the case that perhaps in charcoal or soil there were already round spores present, rather one can prove that the vesicles are created quickly through cooking and gradually through slow swelling. This appears by continuous microscopic observation as well as through slow-motion in the film. 3. If one allows grass or moss to swell in the same way, movement and vesicular decomposition are also created. I beg you to not become impatient. This all sounds totally insane; however, I have observed the process long and uninterrupted, and it presented time and again with a vesicular-type of material decomposition, vesicles gathering into clusters, vesicles taking on a positive or negative electrical charge. One can experimentally prove the creation of amoeboid movements of vesicular clusters by gathering vesicles of various kinds using a suitable substance. As of now, I experimented with gelatin or lecithin, which forms edges and shapes. One can get contractile amoeboid structures by adding:

1. Cooked soil and gelatin
2. Staphylococci and lecithin that has not been cooked.
3. Charcoal and lecithin or gelatin.

The "spores", which are viewed as bacteria microbes, somehow get into the substance, then in addition to the "germs," there are cocci-vesicles in which the matter decomposes.

Or, however, what I believe to be more probable, the “spores” are vesicles which form when matter swells. One must question the origin of the spores. It is metaphysical thinking to view them as eternally unchanged. It would be materialistic to determine their progression. [p. 4]: Those were simply thoughts that intruded in my mind during the experiments. I adhere to dialectical materialism as the methodical basic principle of the experiments. It addresses the basic question of mechanism and/or vitalism. Essentially, both separate Life from non-Life into its own absolute realm without thinking about a transition, about the evolution of Life. It seems to me that the tension-charge-formula solves a contradiction. Life and Non-Life have mechanics and electrical energy in common. However, the specific combination of both, the jump from the mechanical charge (vesicles) to electrical charge and further on to discharge and elimination does not occur in non-living matter. Life is, thus, a special combination of non-living mechanical functions. It includes a physical-chemical function in similar manner as non-living matter; it would simultaneously be a separate field in special capacity simply due to a combination of inorganic actions.

An absolute contradiction also does not exist between abiogenesis and germ theory. A grain of dust is lifeless matter that becomes living matter through swelling and charge. At the same time it is a seed of Life. Although probably not all lifeless things can progress to Life, also be seeds. I only wanted to share some of my thoughts with you. I am, however, determined not to take any steps into theory that cannot be substantiated through experiment. I was very pleased with your comparison between two isosceles triangles. It logically refutes the absolute mechanical separation of the chemical-physical from the processes of Life with great accuracy.

As far as bions, the germinal argument is no longer necessary because the structures form immediately after mixing the solutions. Germination needs an incubation period.

If it is, as I must assume, a development of Life from non-living [matter] through many transitional phases, then one must experimentally determine the point when the vesicles not only move, but also pro-create through cell division through performing control experiments on cooked substances. Perhaps one can in this way determine which vesicles are also seeds and which are not.

I believe, dear Professor, that I should share the reflections with you that accompany my experiments because our trials will certainly not be countered with control experiments at first, rather with theories. I have often heard arguments such as “Brownian movements,” “flowing crystals,” and “spores,” already, although those concerned had not bothered to conduct control experiments. I will endeavor to experimentally test all possible objections.

I would now like to answer your questions as detailed as possible.

First question: It turned out, as I mentioned in my last letter to you, that bouillon-culture medium made from meat extract and produced with phosphates impedes the creation of bions. I mailed you the recipe for meat extract in bouillon- and agar- culture mediums, so you could establish the correctness for yourself as you experiment. In the enclosure with today’s letter, you will find the recipe for the meat-bouillon upon which each culture promptly sprouted. I am returning to this type of bouillon after having not used it after Strain 2. The meat-bouillon is apparently the better one.

Second question: Why I use red gelatin. I can determine whether I used too much or too little red gelatin to control the bion preparations by checking the shade of the color. I already wrote you that we are beginning first with the standardization of the materials used. Until now, we worked more “by sensing,” there was no other possibility.

Third question: The formula for the Ringer solution is on enclosure III. We used sterile Ringer solution and changed it every 8-10 days.

[p.5] Fourth question: The possibility of microbes in the gelatin. I already heard this argument from the bacteriologist, Thjöta. I cannot comment from personal experience, but wish to only share that we sterilize the gelatin three different ways. We either store it dry in a sterilizer at 160-180⁰, or second by putting it in potassium chloride and the test tube in a sterilizer at the same temperature, or, third and last, by autoclaving it in potassium chloride at 120⁰ under pressure. I have personally seen coccisimilar shapes at large magnifications, but my experiences up to now show them to be immobile, and they should have nothing to do with the very mobile and electrically charged bion-cocci.

Fifth question: In the beginning of my experimental work with bions, I stored fresh solutions in bottles that were closed air-tight with a glass stopper. I bound the glass stopper tightly to the neck of the bottle using strong binding yarn and put the closed bottles in the sterilizer at 160-180⁰. At times a stopper would pop out, but in most cases they held. After sterilizing one to two hours, the bottles were taken from the sterilizer and immediately sealed with paraffin. Once I attempted to sterilize the melted vials, but the vials shattered. Now we do this by dividing the fresh bion mixture between test tubes, tightly seal the test tubes with hydrophobic cotton, and, so sealed, place them in a sterilizer at 160-180⁰. Test tubes filled with the bion mixture are placed in an autoclave at 120⁰ using the same method.

Sixth question: Concerning the material that I use to produce the bions: I did not use the least caution. The first bion experiments that I completed almost a year ago were done by this means, by getting an egg from the kitchen, coal out of the coal cellar, lecithin and cholesterol as well as potassium-chloride and Ringer solution from the pharmacy. At the present, I am using ordinary coal dust from black coal [this is NOT charcoal!]. In our laboratory, we are now considering a change to graphite, which is also pure carbon. It is a fact that after complete sterilization movement, especially in the grainy, amoeboid formations, increases the finer the coal dust is grated and the better cooked it is prior to adding it to the mixture. Either today or tomorrow I

will send down samples of the lecithin, cholesterol, gelatin, and some other materials. I do not believe that it will make a big difference whether you use French or Norwegian lecithin.

I believe I have now thoroughly answered all of your questions.

And now I would like to ask you the following quite frankly: As I could only report in brief overviews in this letter, many trials with soil, coal, lecithin alone [p. 7] preceded the bion experiments. I believe that an interpretation of bions cannot be made based on the bion experiments alone, rather initiated only after a total review of all previously completed and still to be completed experiments; above all the question whether bions represent a different type of a not yet discovered microbe or are truly organic inorganic material. I tend to assume the latter, and that is because the bions are immediately present after both mixtures are combined and do not need a long time period to develop. After I had convinced myself that all of the assessable materials at my disposal were vesicular in nature, I started to doubt the originality and characteristics of living spores. Anyhow, I am filming the development of the vesicles in slow motion.

I am requesting you, dear colleague, to allow me more time for a summation of all the phenomena I have noticed up to now. Perhaps it would be good if I would send you a 50-page manuscript that I drew up in November. In it I attempted to describe the logical development of the facts, and I believe that it could serve to refute a list of possible objections, which will be superfluous after knowledge of the facts.

I would like to demonstrate with a few examples just how strange these things are. It reminds me time and again how terribly carefully and patiently I must work. The known pasteurization from milk occurs, to my knowledge, at about 60°. I have now autoclaved milk an hour long and, in spite of several negative results, obtained bouillon growths three times. When such actualities occur, everything around me begins to quiver, and I become twice as careful in interpreting what I observe. However, I, personally, can, even may not doubt, the originality of bions or their cultures, especially the cultures from the first autoclaved strain (yellow amoeba growth). As critically and as doubtfully that I confront the entire project, so much more must I hold onto the positive, undeniable facts, rely on them, in order to have the strength and the courage for further unbelievable observations.

I am allowing myself ample time for the interpretation of bions and the relationship between the inorganic and organic. In this letter, I have given a short outline of the deliberations which occupy my thoughts. I have a readied manuscript about the conflict between Vitalism and Mechanism, which attempts to solve the contradiction purely from the standpoint of a dialectic-materialistic model of knowledge. I was determined not to publish it and will first publish after I have proven the dialectical nature of the Tension-Charge-Formula, meaning the similarity and simultaneous dissimilarity between the organic and inorganic, through experimentation. I can, obviously, not say whether I will succeed or not.

I am appealing to you to report observations to me that you make during your experiments and which are not mentioned in previous reports, so that the projects run as parallel as possible. I wish to especially emphasize the necessity of beginning detailed observations of swelling soil and swelling charcoal, so you can convince yourself of the vesicular nature of cooked matter. Pure intuition tells me that these observations will be decisive for the final interpretation of bions.

Summary of enclosures: Answers to question 1-6. Microscopic photos of vesicle decomposed grass, recipe for Ringer Solution, recipe for meat bouillon, sketch of electrical discoveries of different material. Techniques of electrical experiments: isolated wires from a Pantostat [Scandinavian machine-see photo] are attached to two copper spatulas, which are shielded by glass. [p.8] Platinum spatulas, which protrude from a glass tube, are melted onto the ends of the copper spatula. These platinum spatulas are firmly attached to an examination specimen slide like the one included. Electrical investigations proceed by turning on a galvanic current. Uniform movement stops when the current is interrupted; the direction of the movement changes when the current changes. This method prevents errors caused by the flow of liquids. I observe the electrical reaction through a 10-15 magnification object lens and an approx. 16-25 X ocular lens, done best in dark field.

I am ending my report for today. We are continuing with control experiments here, and I will inform you immediately if significant changes or new observations should occur. I wish you success throughout your work, be it that you authenticate me, be it that you suggest beneficial corrections or point out mistakes that were made. I will send you the manuscript about the historical development of the experiments in the near future.

I gladly accept your offer of friendship and thank you for this. Of course, I put myself at your disposal at any time for any type of information. Should you encounter any difficulties with the cultivation of bions, which sometimes happens, then I can gladly send you prepared culture mediums that have proven to be the best directly from here.

PS It is important to compare both cooked and uncooked lecithin when evaluating the influence of lecithin and the vesicular nature of cooked material. Furthermore, one must immediately observe the lecithin in potassium chloride, then one can see swelling and movement, and after 48 hours, most of the movement and swelling has stopped. In addition, it is important to examine the bion mixture for movement and division immediately after autoclaving. Inoculating the bions is best 48 hours after autoclaving. They develop further to more lively forms within the first 8 days.

PS I would like to ask you to view my report in the last letter concerning the negative results in cultivating individual substances as questionable for the time being because, as it turned out today, autoclaved bion strains that did not sprout in meat extract bouillon produced cultures in meat broth bouillon. It is, thus, not ruled out under all conditions that sterilized soil or charcoal could produce cultures in meat broth bouillon. I will report about this upon completion of the experiments.]

23 March, Reich to DuTeil, Today I am sending you a small postscript to my letter from yesterday.

- 1.) Today I determined that the Ringer solution recipe you received yesterday is a bit different than the Ringer solution recipe I am sending you today. Yesterday's recipe was based on the Ringer solution that was used to store cancer tissue. The second recipe, which follows today, is based on the Ringer solution that I *make* from the beginning for the creation of bions, I cannot assess this because I have not yet tested it. [I cannot assess whether the second recipe, which follows today, is based on the Ringer solution that I used in the beginning to create bions because I have not yet tested it]. In any case I would suggest to use today's recipe.
- 2.) At the same time I am sending you a specimen slide, which I had made for the experiments with electrical charge.
- 3.) An overview of the electrical reactions of different materials, like they have been completed again and again until now.
- 4.) You will receive in the same post lecithin and cholesterol like we use here to create bions. Besides that, you will receive soil, grated charcoal, and a piece of coal. [Reich refers to his Kohlenkeller and the fact that he got his coal from there. This is not charcoal! You do not heat with charcoal, and there is no such thing as a charcoal cellar, at least I have neither seen one nor heard of one].

In addition, I would like to add a brief comment to my letter from yesterday: I believe to understand after conversations with different colleagues that it is better to report observations in small doses rather than too many at once. Perhaps it would be advisable at this point to not discuss the observations about charcoal, soil, vesicular nature of cooked substances, etc. which I wrote you about in yesterday's letter.]

2 April, Reich to DuTeil, Major concerns afflicted me in the days after I reported the preliminary soil and coal trials to you. Those of us who have worked so long with these things are so used to and sure of the methods and results of the trials that we make a mistake when we make statements to third parties: We report the final results and in doing so forget the many small and large headaches that I had at first just with the bion experiments, and that I and my co-workers experienced later with cultivation and control experiments. After consulting with my assistant, I decided to share with you the particular subtleties and possible mistakes made during the bion trials. First to save you time and effort, second to prevent inhibitory doubt, that can lead to such important questions should such experiments fail. I am, therefore, sending you a short manuscript in which you will find the most important details, especially the errors that one must avoid if clear and unequivocal phenomenon is to be obtained.

Furthermore, in the past two weeks I was successful in further clarifying the question of spores through special experiments with soil and coal. All of us here are keenly interested in how you will do with the first requests, and we sincerely beg of you to share the difficulties as well as positive results with us as soon as possible.

I would like to use three examples to illustrate justification of the concern that I wrote about above. A short time ago, I was visited by a renowned plant pathologist from Copenhagen [Neergaard], who had heard of my experiments. I was in the act of repeating the soil and coal experiments. I could show him the crawling soil and coal, which greatly surprised him and inspired him to participate in the test. I gave him precise instructions; he drove back to Copenhagen and wrote me then after about 10 days that, although he had tried his best to reproduce what he witnessed with me, he was only partially successful.

Another example: In addition to my private laboratory, I have set up a larger operational laboratory. It happens from time to time that experiments are completed by my microbiological assistants in my presence with positive results, while the same experiments in the operational laboratory are not so quickly successful when I am not around. This is due to small actions of which we are barely aware, for example, bion cultures did not develop for a while because, as we later found out, an assistant had created a different bouillon than the one I had used in the first cultures.

A third example: I was already very familiar with crawling soil pseudo amoeba when I trained my microbiological assistant. I showed her such a structure at a 2500X magnification. I could clearly see the contraction and expansion movements of the edges. She looked in the microscope about 10 minutes and claimed again and again to see nothing. I asked her to look at it again. She suddenly let out a cry of surprise and said, 'Now I see it very clearly.' It was comparable to people who look in the sky and cannot see an airplane, although it is in their field of vision.

These examples, dear Professor, should be enough to clarify my concerns to you, for I know very well how much of the total issue depends on your success with the experiments. And, thus, I am sending you some of the 'small' measures known to me for facilitation purposes.

Very often I have determined failures with my colleagues' experiments here and identified that, although everything applicable to destroy any possible germs was done correctly, they still forgot that the main goal was not the destruction of

possible germs, rather another: By maintaining the most correct sterilization [methods], not to destroy the fundamental conditions for the creation of bions, but rather to sustain them.

On Saturday evening I am sending you soil and coal that was sterilized at 180⁰ and boiled in potassium chloride until the liquid turned cloudy. They were controlled for microscopic movements. Because I cannot determine how long and under which conditions the soil and coal structures display movement, I can only hope that you can still determine these phenomena in the shipped preparations. However, you can readily complete the soil and coal experiments aided by a good binocular-microscope. I am sending you further bouillon-culture mediums from the type that proved to be relatively reliable. I can do this so long until you succeed in finding the best possible bouillon composition for the bion culture. Please write me about this.

3 April 37, Reich to DuTeil, [I thought that it would be advantageous to send you soil and charcoal preparations, the production of which I described in my last letter to you. The enclosed preparations were created with one very important deviation from previous instructions: the soil and charcoal were grated very fine, dry sterilized at 180C for 1 1/2 hours, then boiled (in 0.1 N KCl) at 180C for 1/2 to 1 hour until the liquid showed a dense turbidity. The potassium-chloride was previously autoclaved.

At the same time, you are receiving agar cultures from non-sterilized charcoal and soil-inoculations together with sterile inoculations of the same charcoal and soil: as proof of sterility.

Hopefully the mobility will not be lost during the few days of transportation. One can clearly see them at a magnification of 2000-3000 X.

I have been working with the same charcoal and soil doing cultivation trials for several weeks. I have several positive results: 5 times from seven inoculations from charcoal in meat bouillon sterilized at 180C and 3 times from 5 sterile soil inoculations. I would like to continue these experiments before I submit a comprehensive report. A detailed report about possible errors made while producing bions and their cultures will follow at the beginning of next week.

The same shipment contains 15 test tubes of meat bouillon.

- A. Same: yard soil dry at 180C, 2 hours both from March 30, 1937
- B. Coal from the coal cellar, treated likewise
- C. Same soil
treated the same way as on April 3, 1937
- D. Same coal
Best wishes...

6 April, DuTeil telegram to Reich (*Beyond* p. 104): bion cultures successful in Nice. **DuTeil letter to Reich**

(correspondence box 10, DuTeil flds) I have spent my time in Pontigny talking with scientists [savants] who, like myself, are here for a little rest, about the main results of our experiments. I have learned that it is preferable to “separate out” and to spread the revelations out at intervals, and I have not yet spoken about the experiments on coal and earth. I can tell you that my paper aroused the most intense interest with these gentlemen, and they asked me to keep Mr. Paul Desjardins (the philosopher who directs Pontigny) informed about the results of our parallel works in order that they themselves might be informed. There were, moreover, at Pontigny diverse thinkers and foreign scientists, in particular a young German philosopher, Mr. Kuhn, who founded an office of International Philosophical Collaboration over there and who knew your name very well as being very important in the psychoanalytic movement. Also a Swedish writer, several Norwegian friends of whom have been cared for by you [i.e., as patients]. He wrote them to point out to them that he had heard of your works through me at Pontigny. I report this to you in order to show you that Pontigny is a very important place for the diffusion of ideas. I go there often, and this is the time for me to make you known in a milieu which has intellectual and scientific relations with the entire world.

I am closing this letter, which has the sole purpose of reassuring you about my activity and to ask you to wait still a few days before having detailed information from me about the experiments I have undertaken here, and which I think, not your works [sic] but the certain and guaranteed diffusion of your works in French official scientific milieux. By proceeding slowly, and with a few precautions, our diffusion will take on a character of certainty such that the results obtained could be irrefutable.

I received your shipments by airmail; I found them when returning here. They forwarded to me at Pontigny only two of your letters, those of 22 and 23 March. I particularly found impressive the slide [stage?] for the microscope that you contrived for the study of the electrification of organisms—which is very ingenious. I intend to undertake it right away, to supplement your research. Right now I ask you to send me a culture of *Staphylococcus* and a culture of *Streptococcus* for comparative study. I can hardly procure them [I'd have difficulty procuring them] at Nice, not being a doctor. Moreover, I have agreed with the Ronchese Laboratory for the inoculation of the guinea pigs with the cultures we're going to obtain. It is Dr. Ronchese himself

who will supervise this work. In the beginning, I had some difficulties convincing these gentlemen about the necessity of furthering this research.

The routine is well-established [rooted]. But I succeeded in creating a movement of interest which as you see is translated by tangible results, and we are going to enter into the active period.

I thank you again for the great facilities that you are giving me for these experiments. I hope they may be of some use for your works and their diffusion, and I remain wholeheartedly your RDT

PS I want you to get the book entitled *Le Géonou: the Living Earth* by Dr. Helan Jaworski. In France it is edited by the Nouvelle Revue Française. Look at pp. 128 ff (chapter 5 “The Amazing Lives of Minerals,” by Mr. Albert Mary. You can find described the research of Cartaud, Dautère, Mary, Otto von Schrön—as well as of Harting, Rainey, Dastre, über die ‘Eoben’ von Raphael Dubois, die ‘Barium-Cytoden’ von Martin Kuckuck, die ‘Protobien’ Alfonso L. Hererras usw., die an Ihre Versuche mit Erde und Kohle erinnern. (See also the *Dictionnaire de biologie physicieste*—Artikel “Kristall, Eobie, Protobie” von Albert und Alexander Mary).]

Reich diary, 9 April, “Several sterile and ‘non-sterile’ (e.g., esophagus) cancers exhibit the same picture: motile and non-motile cancer cells, also bion-like rod-cocci and pseudo-amoebae in the liquid. Started to culture the cancer liquid this week. Result of sterile cancer (breast)—metastasis in a 60-year-old woman.

The theory of ‘infected cancer tissue’ appears false to me: The cocci and rods from the cancer tissues bear no resemblance to rot bacteria.

In addition, putrid decomposition should occur in the presence of strepto- and pneumococci. Also, sterile cancer tissue yielded cultures with peculiarly motile structures (jerky lateral movements).

Assumption: The so-called post-infection in cancer is probably nothing other than cocci or rods that form from tissue, with decay and swelling occurring. According to this assumption, it was not the cancer cells at all but the bacterial autogenous structures that were the fatal agents. Autoinfection.” (italics in draft)

10 April, two letters from DuTeil to Reich (box 10, DuTeil flds, French original.)

Letter 1: [Reich’s German trans.] “Today I received your letter from April 2, which, of course, first left Oslo on April 8, and I would like to immediately thank you for all the trouble you have gone to so that we may avoid the errors and concerns that made your own work so difficult. I record all of your information very carefully, just the same as I translate all of your letters and reports in French, so that we can use them for our own control examinations.

I am not sending you the compiled details of our first results today because various experiments are still on-going and because the situation is such that we have had to begin again in order to verify the first results. I will only tell you today that we are on the third generation of bions created here through extraction from agar. In two or three days, I will send you a test-tube sample of this third generation, so that you can determine via microscopic examination that it is, indeed, the same creation. Our microscope is, indeed, very good, it is also binocular, but at this time we only have a magnification from 2000X at the most, while you can get up to 3500-4000. On the other hand, you are already very familiar with these formations and will be better able to recognize them again than we.

Macroscopically the result is in every aspect absolutely remarkable. The culture develops after 24 hours. It is yellowish and of a creamy texture from the beginning, and during the next few days it gets even more yellow. I have the impression of a stronger vitality than the culture that you sent me displayed and from which this culture was taken. The vitality appears to increase with each new re-inoculation.

The results in bouillon are a bit less clear; however, even here the cloudiness appears within 24 hours. One of the inoculations onto bouillon, which, however, was autoclaved for half-an-hour, resulted in nothing but turbidity and a re-inoculation from this bouillon onto agar likewise produced an absolute negative result. I am now going to repeat this trial again under the same conditions and after autoclaving the bouillon.

In return, a control tube without inoculation sample... [sic] onto bouillon resulted in a whitish pellicle, which I have transferred onto agar, from which a new culture with a completely different appearance grew, from a whitish color with branches that were definitely similar to subtili. I have not as yet been able to identify them or to determine whether they are actually subtili-rods. I am reporting this to you to show how error sources occur despite all precautions! Anyhow, this culture has absolutely no resemblance to the bion culture.

As of now, it has been impossible to directly sterilize the bion mixtures at 180C. The ampules burst, and I fear that one might find still other error sources in the procedures that you use and described to me. The glass stoppers do not close hermetically; furthermore, using cotton produces mildew in the long run that gets into the culture. I am searching for the possibility of having ampules manufactured, which can be melted over a flame and which are able to withstand sterilization at 180C. Up to now, only one of them endured and burst when used a second time. I will see if it will succeed using Pyrex glass. But Pyrex glass is very difficult to melt in a flame, and one also needs special tools, which I do not possess right now. On the other hand, the laboratory where I am working, the Nice Municipal Hygiene Laboratory, is thoroughly equipped, though there is a lack of space and the personnel has a lot of on-going work, so that I can only proceed slowly and in the course of this must take advantage of the rare free moments that the personnel has. This should explain why I cannot work very quickly. I spend on average two or three mornings each week at the laboratory. If I were able to interest various important persons whom I have

been considering in this issue and establish certain funds, I could hire some help and could complete the experiments without interruption.

When I mention the ampules melting over the lamp, it's not because I believe that the precautionary measures used in your procedures were insufficient. Just the opposite; I consider them sufficient. But because I have to be fully responsible for announcing this undertaking in Paris, I prefer to make any objections, which one could make to me regarding the sterilization, impossible and render them irrefutable from the very beginning. The more caution that I apply-meaning officially controlled precautionary measures-the more weight my report will carry. I have, in any case, already succeeded in creating a certain movement of opinions in Nice, and that is very beneficial. Even the satirical magazines are addressing this, and that is the beginning of recognition!

I will stop here for today because I do not have as much time as I would like to report the experiments (soil-coal) to you, which are of great interest to me. I must save this for my next letter. Today I only wanted to describe the difficult conditions under which I work, so that you do not become too impatient."

letter 2 (shorter): "I'm sending this follow-up to my letter from earlier today to ask you if it would be possible for you to send me a few ampules of fresh bion solutions. We have lost a lot because the ampules burst, and I only have a few of the ones you sent me in January left over. On the other hand, I fear that these could already be old and would not give us enough conclusive results.

I would be especially pleased if you could bring the cultures in ampules that are as sturdy as possible, so that we can attempt to gradually heat them up without them bursting. It would be of the greatest interest to have fully sealed ampules that one can put directly into the sterilizer, for that would allow researchers, who wish to attempt a simple verification check, to employ very simple manipulations for the sterilization, opening of the ampules, and the transfer onto agar or bouillon, a procedure so simple that no one could refuse to try it under the pretence of complexity or due to a lack of time or equipment. As a matter of fact, we must envision a method that makes a psychological approach using the maximum amount of ease and certitude regarding public opinion possible. And that is why a simple and quick re-examination itself constitutes the foundation of our actions. This is what brings me to eliminate complicated manipulations, especially when transferring to ampules with haphazard stoppers, because these stoppers can always be questioned, not, however, closure through melting them over a flame." **This suggestion from Reich may be what inspired DuTeil to come up with Sy-clos system.**

[Trans of French original: This morning I received your letter dated 2 April, but which left Oslo only on 8 April. And I want to thank you right away for the trouble you are taking to avoid errors for us here and difficulties which have assailed you in your own works. I take care to note all the information as soon as I translate into French all your letters and reports in order to use them for our own control research as it proceeds.

I am not yet sending you the classified details of my first results, because certain experiments are still taking place, and there are some we have had to repeat in order to verify the initial results. I'll tell you today only that we are at the third generation of bions obtained here, derived from your culture on agar. In two or three days I'll send you a sample tube of this third generation in order that you may verify, by microscopic examination, that these are really the same formations.

Our microscope is in fact very good—it is also binocular—but at present we only have a maximum magnification of about 2000x, whereas you have 3500-4000x. On the other hand, you have a great familiarity with these formations and can recognize them in detail better than we can.

Anyway, microscopically speaking, the result is absolutely remarkable. The culture develops in 24 hours. It is from the outset yellowish and creamy; and it becomes more and more yellow after several days. I have the impression of a vitality stronger than that of the culture which you had sent me and from which they proceeded and the vitality seems to increase with the reculturings. [SAPA bions!?!?]

The results from bouillon are a little less clear; nevertheless, the turbidity appears at the end of 24 hours. Anyway, one of the inoculations from bouillon placed in the autoclave for a half hour did not yield any turbidity, and an inoculation on agar from this bouillon also gave a purely negative result. I am starting the experiment again right now under the same conditions and after autoclaving the bouillon.

On the other hand, a test tube without inoculation gave us from bouillon a whitish pellicle that I recultured on agar, in which the pellicle produced for me a culture of an entirely different appearance, with a whitish color, some branches—entirely resembling *Bacillus subtilis*. I have not yet been able to identify it and verify if they are really rods of *B. subtilis*. I point it out to you to show how, in spite of precautions, you still find sources of error. Anyway, this culture has no resemblance to the culture of bions.

It has up to now been impossible for us to stabilize the mixture of bions at 180° directly. The tubes burst, and in the process you say you used, I still fear sources of error: the glass stoppers not being hermetically sealed, and besides, the cotton

producing mold which contaminates the cultures. I'd like to explore the possibility of having tubes manufactured which could be sealed by spirit lamp flame and which would resist sterilization to 180°. Up to now, only one tube resisted once, but it was a test tube that only contained water, and the second time it burst like the others. I'm going to see if a Pyrex tube would not. But the Pyrex tube might be hard to melt shut by a spirit lamp flame and require special equipment which I don't have right now. [Note DuTeil early on is thinking about apparatus, in a way that eventually leads him to conceive Sy-clos and H-tube.] On the other hand, the laboratory where I work, and which is the municipal laboratory of health in the city of Nice is rather well equipped but they lack space and the personnel has much work right now, so I am forced to proceed very slowly, using the rare moments when personnel is free. This is to explain to you that I can't go very quickly. I spend on average two or three mornings a week at the laboratory. If I could interest different personalities that I've seen there and could obtain some funds is the question, I could get an assistant and carry out experiments in an uninterrupted manner.

If I speak of tubes sealed by lamp, it isn't that I think your procedure isn't sufficiently cautious; I think, on the contrary, that they are sufficient. But as I must be responsible for publicizing these works in Paris, I prefer to quash the objections they might make regarding sterilization ahead of time and make them indisputable. The more precautions I will have taken—officially controlled—the more value my lecture will have. I have, in any case, already succeeded in creating a movement of public opinion at Nice, and it is very favorable. Even in the satirical newspapers which are involved there, which is the beginning of glory!

I'm stopping for today, not having the time to speak to you as I would like about the experiments—earth, charcoal, which interest me very much. This will be for my next letter. I only wanted to inform you about the difficult conditions in which I work so that you would not be too impatient.”

10 April, Letter 2: I am sending you this PS to today's letter to ask you if it would not be possible for you to send me a few tubes of the mixture of fresh bions. We have lost much through the breakage of tubes and I have only a little of your January shipment remaining. Moreover, I fear that they may already be old and won't yield any conclusive results.

I'd be pleased if you could put them in the most solid tubes possible so that we may try on some of them to heat them progressively without breakage. It would be very interesting if we could have some tubes that you could put in the sterilizer directly, all sealed, for that would allow us to have precedence over those scientists who would want to try an easy verification for a very simple operation, sterilization, opening of the tube and reculturing from agar on bouillon, an operation so simple that none could refuse to try it under the pretext of lack of time or equipment. This is what we should occupy ourselves with: in fact, it is the method which will allow us to act psychologically with the most facility and certainty of opinion. And for that, an easy and rapid verification constitutes the basis even of our action (i.e., the basis of our action consists of an easy and rapid verification).

It's what prompts me to eliminate complex manipulations and in particular decantation into recipients equipped with haphazard closures, for these stoppers could always be disputed. Whereas sealing with a spirit lamp cannot be.

13 April, Reich to DuTeil (corresp box10, DuTeil flds) [***5 pp.] [I would first like to verify in one the receipt of your telegram from last week with the positive, general report, the letter that immediately followed, and both of the letters from April 10.

I had a vivid sense of your situation as I was reading your last two letters. It is certainly not easy. The problems that you are facing now: lack of space, no assistants, etc., I had to overcome about 3/4 year ago using all the strength I had available. We are even familiar with the worries that you have, however, we find ourselves in a better psychological position at this time, since alongside the large technical problems we also have many wonderful and continually new, astonishing, positive results.

I have made a note of the most important points of your last two letters and would like to answer them in sequence.

1) Regarding the question of correct sterilization, probably the most important of your problems: To not make our work more difficult than necessary, we should no longer require more of a guarantee of sterility during the creation of bion cultures than is now required from bacteriology and hygiene during production of normal bacteria cultures. In the beginning even I attempted to sterilize ampules sealed by melting in order to eliminate the question of air-borne infection; however, I believe this to be technologically impossible because the liquid evaporates and the container shatters. But I also think that it is not necessary to sterilize in ampules melted closed because in a sterilizer set at 180-190C, the air is so hot that any airborne germs that might be present cannot survive under any circumstances. One can, thus, calmly boil in an only loosely closed or even in an open ampule or dish without risking the allegation of airborne contamination. One stipulation is that when cooking bion mixtures in an open dish, do not remove from the sterilizer, rather only quickly

open the sterilizer for the purpose of inoculating, and inoculate a bouillon glass held in readiness within the sterilizer. By the way, the question of sterilization is simplified by the fact that the materials from which the bions are created are sterilized completely prior to using them. I would like to describe the steps for you once again:

Dry charcoal is placed in a dish, put in the sterilizer, and kept at 180C for an hour. Autoclave gelatin solution lecithin, cholesterol, KCl, and ringer [solution] beforehand. Likewise the milk. The egg-white is to be taken immediately out of the egg, sterile, and added to the mixture. If sterilization is completely carried out first, then the question of contamination from milk, the air, or even from germs will no longer be an issue. I then place a dish in the sterilizer, fill it with the autoclaved Ringer-KCl-solution; add the autoclaved gelatin solution and all other autoclaved materials except for the lecithin. The sterilizer should be closed immediately after each manipulation. The lecithin, that was autoclaved (not dry-sterilized) is stirred into the KCl and immediately added to the previous mixture. This way the mixture out of the previously sterilized materials cooks at a temperature of 180C in an absolutely germ-free environment. My assistant still prepares the bions in such a manner by filling the mixture into test tubes, plugging them with hydrophilic cotton, and letting it cook in the tubes. As of yet, we have not been able to detect infection from fungi. In any case, such an infection would be easy to distinguish from typical bion structures. Should you be able to cook the fresh bion mixture in tightly sealed ampules, then you have certainly made progress. The result definitely will be the same.

2) Inoculating the bions occurs directly from the mixture in the ampules inside the sterilizer after about one half to one hour of cooking them in the sterilizer. Here, too, contamination from outside is ruled out. In order to eliminate possible false results, we have now made it a habit to use the following two inoculation methods:

a) Bouillon is inoculated three to four times from one and the same bion mixture.

b) On several consecutive days, the same bion mixture is immediately inoculated from anew again and again. It turns out that different bion mixtures are optimal for inoculation on different days. We could not determine the reason for this. If it is okay with you, may I suggest that you try the technique of cooking in flame-sealed ampules perhaps on the side; however, were it successful, it would be an advancement. Bacteriologists consider an inoculation to be sterile even when it occurs in an open room.

3) Today I had three different bion mixtures made here in the laboratory, from which I will send you samples and likewise keep samples here as controls. I cannot guarantee the results of inoculating the bions you sent me in January, because I do not know whether they are still living.

4) I did not completely understand the passage in your letter where you spoke of autoclaving bouillon and the culture from it failing. Should you have autoclaved the bouillon culture itself, I am not able to voice an opinion, for I have not yet experimented with autoclaved cultures. However, I have succeeded with some cultures from cooked bouillon cultures. I have made it a principle in my work to always move forward slowly, from step to step, when experimenting was all too complicated and to delay very advanced experiments until I was far enough along that I could understand all the connecting views. I believe I have taken all requirements for sterilization into account by taking all the materials used in the mixtures and sterilizing them beforehand using the highest possible temperatures.

At this point, I would like to draw your attention to the fact that we found coke better suited than pure coal; however, that is not yet certain. I would, thus, recommend using 180C sterile coke dust for bion mixtures.

Allow me, dear professor, to make a general statement concerning the experiments that you are undertaking. The bion experiment is only one part, granted the main part, of this total undertaking. I cannot and may not expect you to go to the trouble of taking on the mission as we do here. I only wish to mention to you which support points we have in addition to the bion experiments and the experiments with their cultures: First, the comparative methods of the investigations. We compare the bion mixtures as well as their cultures continuously both macroscopically as well as microscopically with all obtainable forms of previously known structures, also especially electrically, and we determine again and again that bions and their cultures differ from every other form of known bacterial structures in every way. The growths on agar are stronger, develop faster, have a totally different consistency and color. The electrical reaction is, except for yellow amoeba of which you have a sample, negative as opposed to streptococci, staphylococci, tubercle bacilli, etc., which are positive. Especially important to us here is the comparison with unsterile inoculations. If one inoculates rotten meat in bouillon, one is immediately aware of the difference. Putrefactive bacteria are malodorous, behave electronically, and are in other ways totally different. In addition we inoculate to control sterility with unboiled, boiled, and autoclaved plain tap water from the faucet. I am writing this to you to give you the assurance that we are working here with absolute caution and the correct supervisory approach. There are many doubts and worries concerning details; however the inoculation series with bions are absolutely affirmative.

May I make a further comment concerning the possible objections from the side of the authorities? At the beginning of this mission I depended very much on verification of the completed experiments through official specialists in bacteriology and biology. In the course of this very difficult time, I learned to give up hope that it would be easy to convince [them]. The more unbelievable matter I learned to see, the clearer it became to me that verification or even thorough post controls of the experiments would not be easy to achieve, and that I would also be making a mistake if I allowed my work to depend on that. True, it would be an immense relief for all of the work that follows if such recognition would follow quickly and relatively smoothly. However, at the same time I came to terms that it would perhaps cost even a year-long battle to carry the issue through that, as I realize today, seems to break down due to very many, perhaps all too many, deep-seated opinions. Should a victory quickly follow, it would in essence be due to your energy and initiative.

The scientific enthusiasm and the friendly help with which you approach the mission and intend to complete it tell me directly how valuable and essential your collaboration is. For precisely this reason I may not neglect to share with you my personal opinion concerning an eventual negative verdict from the side of official authorities. I am convinced that you are not at all upset with me, especially since I am prepared for a very difficult and prolonged battle. I ask that you, when you have encountered problems, to describe them exactly to me, and I will try to help you as well as I can with local expert knowledge.

Within one to two days, you will receive fresh bion mixtures along with bouillon sent from here. Furthermore, I would like to share the good news that I was able to develop highly unusual cultures from autoclaved moss that I mixed with water boiled for an hour. I am sending you a sample of these agar cultures, strain 4, and am requesting that you ask a hygienist there whether such structures are already known and, if so, which type they are. I, personally, and also my assistant, Dr. Havrevold, who is very well educated, have never before seen such structures. We inoculate all formations that we get from autoclaving materials and their swellings onto mice and guinea pigs in order to immediately determine harmfulness or harmlessness.

I do not know how much extra time you have, but if you wish to have a lot of fun with this task, then please cook every substance you have available, meat, moss, grass, and examine it under a microscope immediately after boiling. I have gotten the most persuasive help with these observations when faced with difficult and dubious situations. Especially the so important psychological foundation for this work: It can be proven that Life develops from non-living matter. In the process, I avoid claiming that I artificially create Life. I limit myself, in fact, by saying that through experiments, I was able to recreate the natural process of abiogenesis experimentally. The wording “create Life” carries an air of mystic and incorrectly assumes that Life is something totally and in every way separated from non-living matter, the disproof of which is the main objective of our work.

I fear that I have already delayed you too long; however, I believe that carrying out such debates can only clarify the work and propel things forward.

Media representatives have already touched base with us. I regularly refuse to share anything concerning the experiments and refer them only to the publications, which are in preparation. I fear that any wide-spread press campaign that might erupt could all too easily discredit the entire mission. At the present, only a rather general preliminary report will follow in a supportive newspaper like the one you sent me that appears in Nice. I assume that this position in the matter is acceptable to you.

PS Please send future correspondence to my direct address...(attached list '15.IV.37 Herrn Prof. DuTeil, Nice')

Symbol key:

6 c/b dry sterilize charcoal dust at 180C for one hour
lecithin
cholesterol
potassium chloride
Ringer solution
gelatin
milk
autoclave at 120C for half an hour.

Remove yolk and egg white from a fresh egg using a pipette heated to incandescence. The bion mixture from these sterilized materials is placed in a dry-sterilizer heated to 180C and cooked for one half to one hour.

6c/b/9 created on April 14, 1937

6c/b/10 created on April 15, 1937

6c/b/11 created on April 15, 1937.

Microscopic results on April 15, 1937.

From all three mixtures, 6c/b/9 is somewhat weaker in movement than the other two.

If the solution is completely clear and filled with sediment, please do not inoculate.

I ask to be notified whether I should send homegrown meat-bouillon?"

Reich diary, 19 April 37 (*Beyond*, draft p. 205) “Metastases from the *liver*—after autopsy.

1. *Metastasis* cells are stronger. 2. The great diversity of forms is staggering. 3. All forms are found, from the coccus to the cell (clusters of vesicles, etc.)”

19 April 37, Reich to DuTeil (corresp box 10, DuTeil flds) [***1 p] [Strick transl: “I want today to make a short report, that the 3 sent cultures (6c/b IX, X and XI)—X and XI promptly grew up in bouillon, while IX after the third inoculation still did not grow up. I wrote you already along with the shipment that IX appeared poor and weak to me microscopically, which the culture results confirmed. In the same manner I described in my letter, six previous various mixtures were produced, all grown up, thus from nine 6c/b preparations one did not grow up. Please report the results of your inoculations to me. I’d also very much like to know whether for you the production of bion preparations has been successful.

I content myself today with this report and give you heartfelt greetings.”]

20 April (*Beyond*, p. 105, bion lab ntbk 2, ms p. 144-146, OI box 7) “Animal Tissue Tests:

Autoclaved (1 hour) meat with hypotonic KCl gives amoeboid structures + culture broth

Autoclaved meat (bovine) with hypotonic CaCl₂ gives motionless clusters [of vesicles] culture broth

Fresh meat with hypotonic KCl gives bions + culture +

Liver, sterile + 5 KCl [gives] strong bions +

Pasteur merely verified the effect of the air.

The theoreticians of spontaneous generation did not perform any experiments. [He’s wrong here, I think.]

Nobody attempted to culture boiled material.

There are problems: How can the specific association of protozoa with the parent material be *proved*? Why do amoebae not form from germs that are not on moss? Why does a tulip leaf give no living organisms? Why do algae give no living organisms?

Fresh, green grass and moss, juicy and moist, do not yield protozoa, perhaps just cocci. [Easy to imagine his critics jumping in here without waiting to hear any more: appearance of this extreme pleomorphism (a la Naegeli) was long ago dismissed by Brefeld: ‘If you don’t work with pure cultures, you get only molds and nonsense.’ Forgetting conveniently Pasteur’s refusal to work with pure cultures. See Mazumdar *Species and Specificity*, Mendelsohn diss. For Tyndall’s response to the fresh grass vs old, fall grass paradox, see *Floating Matter of the Air* p. 150]

On the other hand, in the fall, protozoa are easily obtained—i.e., *the structure must first break down and then swell again. This is no doubt what happens in the case of cancer: first the tissue breaks down by losing tumescence, and then it swells up again.*” (italics in draft, underlined in lab ntbk entry)

26 April, Reich to DuTeil (corresp box 10, DuTeil flds***) [Strick transl.: “We wait here tensely for the outcome of the researches carried out by you. I write today only to keep up our contact on another matter. We have provisionally completed the production of bions and the culturing of bion mixture 6c/b, and they lead us simultaneously to further consolidation of the results and the controls. Just as we simultaneously continue the earth and coal experiments, so we are shifting the main emphasis now to systematic experiments on animal tissues. I proceeded from the thought that since grass, earth, coal, and bion mixture 6 produce life via cooking, I ought not to refrain from research on animal tissues. Thus we experience surprise after surprise. Up till now all animal tissues we’ve studied—muscle, lung, pancreas, spleen, thymus, intestinal wall—regularly produce structures of bionous character, and indeed also by autoclaving in isotonic KCl, as well as by the following procedure: We take the organ in question out of a recently dead deer, dog, ox, guinea pig, etc., burn away the surface with a red-hot spatula, cut it in sterile fashion and remove a small piece of tissue from within in sterile fashion, which we place in autoclaved KCl, as well as in calcium chloride (this is the control). Every different type of treatment of the tissues yields results varying in the details. Movement from the spot [von der Stelle—from place to place? In place?], contraction and expansion (seen particularly well in darkfield) have been typical in all our studies up till now. Today in our laboratory a bouillon culture created by one of my assistants, Dr. Havrevold, from guinea pig lung autoclaved in KCl, yielded a pure culture of strongly motile cocci and clumps of cocci of the same type. We have now decided to add bouillon cultures for every autoclaved and sterilely swollen tissue. The structures that we see can be clearly distinguished from cultures of putridity and other typical bacteria.

We are continuing the culture studies with the separate, individual ingredients of bion mixtures. Up till now, neither from lecithin, nor cholesterolin, eggwhite, etc. have we gotten any cultures. We have also inoculated simply water from the water line [tap] into bouillon, and found out thereby that the growth in bouillon takes much longer to develop and that it yields many negative results. Also, the structures of the bions are totally different. I am now working as well with the cultivation of cancer cells surgically removed by sterile means, from places which, unlike the intestine, stomach or esophagus, did not carry the threat of infection of the mixture from the outset. I will happily report to you about that, once the first series of experiments is finished.

[Its from this series that the S-bacillus was first cultured from liver sarcoma metastasis?]

PS Please write me if you want to have preparations or cultures. I’d also like you to tell me whether your microscope is an inclined binocular microscope, or whether the binocular tube is straight like a monocular tube. I believe I have determined that the microscopic observation of bions is very difficult with a single tube and with a non-inclined binocular tube. In contrast, the inclined binocular tube gives wonderfully 3-dimensional [or vivid] images and makes possible observations that cannot be had with a straight tube.] See Reichert Z Mikroskop Catalog, 1937, [pdf in Reich papers] [Discuss here Hollaender’s Sept. 35 warnings about difficulties created when highly variable biological materials do not behave with the same easy replicability as physical or chemical methods, plus the critical importance of seemingly small details of the technique.](#)

5 May, Reich to DuTeil (corresp box 10, DuTeil flds) [***2 pp] I sincerely thank you for your last letter and verify the receipt of your bouillon cultures. Microscopic examination verified they are completely identical to our own corresponding cultures. I would like to use this opportunity to report that a few offshoots of this same culture changed, meaning that the large amoeba-like structures decomposed very vigorously into individual cocci.

Today I am sending you the following culture samples:

- I.** A culture based on coal from the cellar, treated in this way: First dry-sterilize coal dust for an hour at 180⁰, cook in previously autoclaved KCl until the liquid shows colloidal cloudiness, as with all other decoctions set the dry sterilizer at 180⁰.
- II.** [inoculate???] a sample of the same fully unsterile coal onto agar. The comparison shows completely heterogeneous growths both macroscopically and microscopically. II, thus, contains the microbes that were killed during sterilization, I the microbes as pure culture created by cooking.
- III.** Treat a soil culture in the same manner as the coal after re-inoculating from bouillon. It is a pure culture when compared to [??? Maybe section IV is part of III?]
- IV.** Where the same non-sterile soil is inoculated directly onto agar.
- V.** A culture from a freshly extracted guinea-pig's lung autoclaved in NaCl for half an hour. We are continuously completing tissue sample trials to verify the regularity and/or the irregularity of the culture results. This culture is also a pure culture of vigorously moving cocci, which we have not yet studied.
- VI.** The culture of a liver cancer metastasis extracted sterile from a cadaver, first inoculated in bouillon and re-inoculated in agar. The microscopic finding and the macroscopic comparison shows their characteristics as that of a pure culture, which has nothing in common with putrid decomposition bacteria.
- VII.** A non-sterile soil-agar culture for comparison.

I urge you to further inoculate these cultures as a control in order to determine whether a change in the shape of the structures and the color of the growths takes place over time from one strain to the next. We are working full-steam ahead on tissue culture experiments, as I reported to you in my last letter.

We are very much interested whether you have succeeded in getting a culture from the bion mixture we sent (IX, X, XI) IX did not grow for us, X and XI produced new cultures time after time.

Tomorrow you will receive from us the requested supposedly non-shattering vials, in which one can cook after flame-sealing. We are pre-testing the claims of the company at our conducting laboratory."

12 May, (bion lab ntbk 2, ms p. 166***) "Kalbsleber von frisch geschlachtetem Tier, steril, geschnitten, nach Abbrennung der Oberfläche, je ein Gewebstück in a) CaCl₂, 0.1 normal b) Kaliumchlorid, 0.1 normal c) direkt in Bouillon, ergab bei a) bei 2500 facher..." [calves' liver from a freshly slaughtered animal, a small piece snipped out in a sterile manner after having seared the surface...]

15 May, Reich to DuTeil (corresp box 10, DuTeil flds) [***1 p] We are continuously repeating the control tests for the soil-coal- and bion preparations here. We now also have worked through all of the literature about abiogenesis and microbe questions from Pasteur's time and found a multitude of very interesting and important aspects that speak for us, but also arguments that, while not completely refuting us, must still be clarified through special experiments.

I am writing you today to share a few facts, so that you can see how little by little the task moves forward. For the longest time, we could not explain to ourselves why some bion mixtures grew poorly or not at all compared to others. It is not the degree of sterilization. In contrast, we found that bion solutions containing structures with no electrical charge, which behaved cataphoretically neutral, grew no cultures. It is surely not the complete explanation, but a very important part of it. Why some bion mixtures result in neutral and non-charged structures continues to be a puzzle. The bion mixture IX we sent to you proved to be electrically neutral and produced no culture in contrast to X and XI. We are now testing both microscopically and electrically prior to setting up cultures, so as not to unnecessarily impact the statistics of the successful cultures negatively. I would appreciate it very much if you would report back how you are doing with the production of bion mixtures.

Another bit of news that will interest you. I wrote you that swollen or boiled soil and coal becomes electrically positive (determined once up to now, is still being further tested). It was a pleasant surprise to find that the culture structures from the coal and soil that were dry-sterilized at 180⁰ and then cooked that we got a few times were likewise electrically positive in contrast to the rest of the usual cultures from bion mixture 6c/b, which are electrically negative, as were the cultures that we have obtained from tissues. As you see, we are in the midst of a vast amount of questions and necessary new experimental designs.

Did you receive the ampules that I sent you week before last? We tested them, and it showed that they did not shatter when cooked in a dry-sterilizer at 180⁰.

From time to time, official agencies display signs of animosity towards our work."

18-24 May, (OI box 7, bion lab ntbk 2, ms pp. 159-165***) “Kohleglühversuch am 18.5.1937...”

Reich to DuTeil, 24 May (*Beyond*, p. 106-107, draft p. 208-11, corresp box 10, DuTeil flds***) “I am writing today because I have not been in touch with you for a long time and also because I am very concerned. At the risk of immediately losing your friendship, which is so valuable to me, I must inform you of what has taken place in the last few days here at the laboratory.

I have gradually become more aware of my faculty colleagues’ complete astonishment and even hostility, which is not based on any specialized knowledge. Again and again the objection has been raised that all we are dealing with are very well-known spores or development-products of spores. It occurred to me that I could conduct an experiment with coal that would make it impossible to talk of the presence of spores. [Here’s the rationale for his initial use of incandescent coal as a bion material; perhaps iron as well.] I am sending you a copy of some extracts from our records so that you can see the entries that we made here immediately after the experiment, and I would like to give you a brief description of the facts at this point.

The test set-up is very simple. The phenomena can be clearly observed as long as that a binocular microscope with inclined tubes, providing at least 2500x magnification, is used....[that one can adjust and use with immersion] and to unambiguously observe. I will start by saying that the following experiment has been completed five times up to now always with the same result. Today it is being concurrently completed tenfold. The matter is this:

I took a thin metal spatula tip full of coal dust that was previously dry sterilized for hours at 180⁰ and held it so long over a gas flame at about 1000-1500⁰ until the dust-coal mass was red-hot and incandescent. In the meantime my assistant had produced the phosphate-bouillon solution, which she mixed with the same amount of previously autoclaved 0.1N potassium chloride. In the process, I was following the rationale that in this experiment not only nutritive substances but also swelling substances must be present. The incandescent coal was immediately introduced into the bouillon-potassium chloride solution. In all previously completed test cases, it presented with a colloidal turbidity of the solution, which I was never successful at obtaining by merely boiling. A further phenomenon that was very educational and surprising was the following: The coal particles render the solution dark black at first. But after 15-20 minutes the black color begins to fade and the solution becomes steadily lighter and lighter/brighter/clearer.

In all cases a cloudy, gray-whitish turbidity of the solution remained. I examined this solution microscopically after 24, 48, and 72 hours. It showed that immediately after production in dark field, much more movement was present than by the non-incandescent coal combined with potassium chloride. However, I experienced the greatest surprise on the following and next following days, as it became apparent that, in all cases, the solution was filled with robust, moving cocci, different types of rods, and even contractile amoeboid structures. The structures proved to be electrically charged. We will now go on to culture experiments. Although I have a suspicion that these structures can eventually produce growths only on a very complicated and all-purpose culture medium.

Thus, I am producing a polyvalent culture medium, the recipe of which I will send you shortly should it turn out to be good.

I had to form an opinion concerning the observed phenomenon, which is totally hypothetical and for the time being the following: In coke crystals that have already experienced high-grade sterilization and distillation, the individual particles are tightly bound to one another. During cooking, and especially when heated to incandescence, the connection between the particles is destroyed and through this electrical energy is apparently set free, because individual particles could not otherwise be electrically charged, as the larger pieces are not. When potassium chloride is mixed with dry incandescent coal, the particles into which coal decomposes when heated to incandescence voraciously absorb liquid, viewable under a microscope at a magnification of 2000X. Strange electro-magnetic phenomena appear. Individual small coal particles move vigorously until they come close to a larger one.

At a certain point the small particle either suddenly attaches itself to the surface of the larger particle, or it remains immobile at a certain distance from it. The manner in which the smallest coal particles rush towards the larger ones is clear evidence of electromagnetic forces at work.

This experiment seems to me to completely refute the spore theory in the form in which it currently predominates, because nobody can assert that spores can survive such temperatures as those reached by incandescent heat. In addition, I am in possession of the important proof that every single one of the substances I have so far observed forms vesicular structures when boiling, swelling, or heating to incandescence are carried out. And the vesicles I observed do not differ in form under the microscope from [the spores of] fungi or other spores. I do not deny the existence of spores as postulated by the old theory. All I claim is the vesicular character of the material and the disintegration of swollen, heated, or boiled inorganic material into vesicles is the central point from which further research into the formation of viable microorganisms must proceed. The spores themselves must therefore be derived from matter—namely, by swelling. This appears to be an inevitable assumption.

You have not written to me for a long time [since 10 April, about six weeks] and I do not know why. In view of the complicated and difficult nature of the work I am doing here, I can certainly understand that you are not having an easy time carrying out the control experiments, especially in the presence of prejudiced individuals. If you do not immediately end the friendship on receipt of this most recent report on the coal-heating tests, then I would be very glad to make you the following suggestion. Would you be interested, say in August, in taking a trip to the beautiful northern part of this continent so that you could personally convince yourself of the scope, nature, and results of our work? I do not wish to put you under any kind of

pressure, but I am prepared for an unbelievably difficult struggle with official opinion; the simpler and more self-evident the results of the work, the more gloomy and difficult that struggle will be.

Are you aware that Italian farmers prefer using lava rock [Lavamasse] to fertilize the soil? I would imagine that because of the glowing [incandescent] heat of the material, the cohesion between particles has broken down, and the electrical charges released are directly manifested in the growth of the plants. [Note that this line of reasoning is more than a YEAR before he actually looks at lava, 14 May 38. Thinking how to control for spores leads to the incandescent coal expt., the results of which were so strikingly positive—extremely motile, immediately after preparation—that they lead him to this reasoning about lava.] May I make another comment? The coal-heating experiment is only astonishing if one proceeds from the assumption that the living structures that are observed afterwards, during the swelling process, were already contained in the coal in the form of spores, because then it is impossible* to understand how spores can withstand such high temperatures. This theory, however, is false. The elements—let us call them bions—from which the motile structures are then formed are not previously contained in the coal, but formed by the disintegration of the heated material. So, the spores to which people constantly refer must also once have formed and—please permit this hypothetical leap in reasoning—they must have formed when the Earth was in an incandescent state. They must have developed when the previously incandescent material came in contact with water and swelling substances.

I needed to write and tell you this because, however things turn out, I believe that any progress I make, which never fails to astonish me, will only be made by struggling against counter-opinions, but also with the support of understanding friends.”

*the word ‘nicht’ appears to be missing from the German; otherwise the statement would make little sense.

PS added 25 May: I merely wish to add that we have now slightly modified the bion mixture itself. To the first mixture, we add coal dust heated to incandescence and phosphate broth (10 cc). We then wait about ten minutes before adding the lecithin solution in order to allow the coal particles to swell.

25 May 37 (*Beyond* p. 108, OI box 7, bion lab ntbk 2, ms. p 167***) [Already Reich thinks that radiation tests are necessary.]

“The breakdown of matter into vesicles when heated to incandescence undoubtedly releases large amounts of electrical energy.

The glowing heat must correspond to the emission of radiation by this energy.

Combustion = release of energy

Organization = binding of energy

Radiation tests are needed!!!!”

About this time, **film captions to films of Prep. 6, incandescent soot bions:**]

29 May, DuTeil to Reich (corresp box 10, DuTeil flds) Dear Professor and Friend,

Excuse me for having left you without news for some time, but I had to absent myself in order to return to Pontigny, where a meeting of the Center for the Study of Human Problems, for which I am the administrator, was taking place and where I met large numbers of scientists who had come from France and abroad. Unfortunately, I fell ill during my trip from a particularly serious conjunctivitis which necessitated an eye operation, which prevented me from working as I would have wished. I am better today and can pick up the thread of your correspondence.

I’ve read all of your correspondence with deep interest, and I follow the development of your thought very attentively. I am of the same opinion as you: that the interpretation by the development of spores existing previously in the ingredients that you use ought to be absolutely rejected. There is no reason, in my opinion, to worry about the opposition which we are experiencing to this subject in the scientific world. The events themselves will serve to overcome it.

Moreover, I could see at Pontigny how this opposition arose and even how we might classify it. There are, on the one hand, narrow-minded people who adhere to the Pasteurian science which has been inculcated in them. These people will be forced sooner or later to accept the facts. There are the spiritualists, who think that such experiments may only be interpreted as radical materialism, and who resist on principle. I worked up a rescue metaphysical interpretation which may reassure them and render them less resistant to the facts. Finally, there are materialists, who, for the opposite reason [from the spiritualists] are quite ready to accept what they believe can serve as a basis for their metaphysical opinion. These people are for the time being our natural allies. Finally there exist, but in very small numbers, true scientists who recognize only experience and who greet all possible progress with a critical mind and sympathetic consideration. I was able to interest three persons at Pontigny in a positive way, three scientists who are ready if you authorize me to communicate the necessary information to them, to replicate, for verification, such experiments as we might want. There is Dr. Martiny, who has a laboratory in Paris that is outstandingly well set up and who is known in France for his work on the specificity of tissues. There is Dr. Mr. George Matisse, assistant professor at the Museum of Natural History, which is part of the Sorbonne. Finally, there is in Belgium, the General Secretary of the

National League against Cancer, which the orientation of your work has literally impassioned and who is ready to replicate immediately the production of the bions and the experiments on coal/ charcoal. As soon as I have your authorization, I will give them all the necessary documentation. They will then send you directly the products of their work. They expect photomicrographs even, which will allow them to compare the results very accurately. The scientists in question are, all three of them, very sympathetic.

While I was working this way on the—as it were—moral and psychological development of the matter, assuring in this way of a diffusion [of the information] in an appropriate place, which would be both prudent and useful, I was pursuing the verifications here as much as my health would allow. While the development of the first cultures is proceeding in a normal way, the test tried on your last shipment of fresh bions bearing the numbers 9, 10, and 11 gave less positive results:

a) preparation number 9 produced nothing. It was [lost? illegible]

b) preparation number 10 produced, without having been placed in the autoclave, a whitish culture which I am sending you. The same preparation placed in the autoclave at 120° produced nothing.

c) preparation number 11, without having been placed in the autoclave, produced the same whitish culture which did not withstand the autoclave. I am satisfied with this experiment. This white culture has the same macroscopic aspect as the one which we had found in the test tubes and that I told you I thought was possibly [*Bacillus*] “subtilis.” It is an aerial germ which is very abundant in our country. I would appreciate your telling me from the microscopic point of view if we are really dealing with *B. subtilis* or if, on the contrary, this culture, whose origin clearly seems to be in the tubes sealed by the lamp which you sent me (numbers 10 and 11) corresponds to one of the kinds of bions which you yourself have found. I need not tell you that all precautions have been taken to prevent any contaminant culture from being produced. [i.e., the white culture was contained in the tubes Reich sent him]. The culture was the same on bouillon and on agar, and the inoculating wire used was heated red hot between the two operations.

I consider that this last experiment is not conclusive, be it only by the fact that the germs were killed by a sterilization in the autoclave to 120°. On the other hand, there does not seem to be any relation between the white culture and the creamy yellow culture that we have [seen]. It seems to me we can interpret this to mean that this fresh culture of bions did not survive the trip. I ask you then, to please send me via airmail a series of 2-3 tubes of fresh bions, including with this if you can, a tube on agar of the culture you obtained with this same preparation. Please indicate both the date of the preparation and the date of the culture. In this way I would be able to immediately compare my results with yours. On the other hand, as soon as the cure of my eyes is complete (for I had an almost astounding infection, which prompts me to be very careful in the handling of unfamiliar germs), I will undertake the three original [preparations 9, 10, 11] experiments myself, taking into account what you told me in your last letter received yesterday.

Here then is all I can say to you today on the status of the question, and which will show you that, despite my silence of these last weeks, I have not remained inactive. I still have several days of heavy work, my students' graduation exams for the license run from 5 June to 12 June. [After that] I'll be in a calmer state of mind to resume this work. On the other hand, the Municipal Laboratory where I work leaves its present venue for a few days for a very modern and much larger set-up. I will be in much better shape there.

I am yours with all my heart, RDT

PS I considered a visit with you this summer myself, and I am very deeply touched that you also thought about such a visit. I don't dare promise you that I will make this trip, my health being affected right now by overwork as by the eye infection which I told you about. But if it is materially possible, I shall certainly come to see you.]

1 June, DuTeil completes “Life and Matter,” later presented at Academie des Sciences Morales, on 18 Sept. 37.

5 June, Reich to DuTeil (corresp box 10, DuTeil flds) [***4 pp] “I thank you very much for your letter of 29 May, which I received yesterday. I must admit that it relieved my anxiety.

Beginning with your control experiment results concerning bion preparations IX, X, XI. As I wrote you back then, mixture IX was spoiled, electrically neutral, and produced no culture. On the other hand, X and XI apparently produced the same cultures as yours, specifically primarily cocci and rod-type structures. I would like to repeat what I wrote you back then concerning the creamy yellow amoeboid bion growths. Those do not represent the typical bion growths. I got them only once through autoclaving a fresh bion mixture and, unfortunately, never again since then. Most of the cultures from autoclaved bion mixtures are of the type that you have seen in X and XI, that is, structures whose form cannot be differentiated from that of familiar common bacteria. We are now trying to determine whether these common bion cultures are electrically or in some other way differentiable from common *Bacillus subtilis*. Just because they are similar to *Bacillus subtilis* does not mean that they come out of the air because, as I have already often reported, the materials used for the bion mixtures are individually autoclaved at 120°, charcoal dry sterilized at about 180°, and besides that, the mixture itself is either cooked or also autoclaved again. An infection from the air is completely ruled out following these preparations. It is most probable that inorganic matter

spontaneously organized in forms that are the same as the structures originating from the air. As a matter of principle, these so-called air-borne germs must be created in a similar way as the organized structures of bions.

One of the biggest problems in our work is that culture results in isolated forms only appeared once, almost coincidentally, without me being able to say why this particular, special form was created. Our work is not yet that advanced. For example, I once obtained a very unusual rod-structure that I could not find described in bacteriology by mixing autoclaved moss with boiling water. We cultivate it further, and it does not in essence change its shape. The same applies to the creamy, yellow packet-amoeba growths.

Now to [address] the question concerning your attempts at autoclaving immediately following the addition of the lecithin solution to the so-called first mixture. The substances are previously autoclaved, and the mixture is immediately either cooked or autoclaved. During the next 2-6 days, a complete formation of specific shapes occurs: rods, amoeboid structures, cocci in sterilized, autoclaved composites. Autoclaving immediately following creation of the mixture does not hinder the development of structures, may perhaps even boost it. On the other hand, in many preparations, autoclaving again destroys the structures after several days. Confusing is another experience I had now, specifically that some autoclaved mixtures showed no destruction after several days. I beg you, dear friend, not to forget that we are continuously dealing with a vast amount of new facts that, although we can observe, we cannot yet immediately explain. I would suggest that you do not autoclave the bions that I sent you again, for that already occurred here. Only if you create fresh bions yourself, then please autoclave those immediately after you create the mixture. You can then verify for yourself that the structures are not broken down by autoclaving. I hope that I have completely and clearly expressed myself.

We are struggling hard with the culture medium, and until now I have not been able to find out which culture medium is the best. Confusing is the following up to now unexplainable situation: Series of autoclaved bions promptly grow in bouillon and then in agar; however, other series do not sprout, without us being able to say why. Our control experiments are grouped in two ways:

1. We continuously control whether the growths could be a result of insufficient material and mixture sterilization.
2. On the other hand, we continuously control why bion mixtures that previously grew promptly do not develop.

That probably is either due to the type of mixture, the particular type of material (i.e. too old, dried out lecithin) or due to mistakes during the creation of the culture medium.

Thank you for the precise explanation, which fully verifies that the development of formations from pre-existing spores is totally ruled out based on the creation method used and the way the bions are handled. I would like to provide you with a short explanation about the different methods we use now to create bions.

- a) Non-sterile mixed and then autoclaved: preparation 6a/c
- b) Autoclave the material initially, then mix: preparation 6c
- c) Autoclave the material initially and cook the mixture afterwards: preparations 6c/b
- d) Autoclave the material initially, autoclave the mixture afterwards: preparation 6c/c

This has given me different results, but still no definite picture which of the different treatment methods is the best. At the present, 6c/b has been the best and given us the most cultivation results. Preparations IX, X, and XI, which you inoculated, were of this type.

We will proceed to inoculate the bion mixtures onto all imaginable types of culture mediums in order to determine which culture medium is the most favorable.

My heartfelt thanks for your endeavors amongst the scientific specialists. It would naturally be extremely gratifying and beneficial if an official institute in Paris or Belgium, as suggested, would join in the bion and charcoal experiments. You will, though, understand that I have a few concerns about immediately accepting this offer of collaboration, as much as I would like to, given the confusing number of problems, facts, questions, and doubts that I continuously experience. Believe me that this is neither false pride nor isolation tendencies. My thoughts are as follows: Here in the laboratory, we are devoted solely to this type of work and battle hard and difficultly with the material and the problems. Things which I accomplish are often only accomplished with difficulty by the next available assistant. A failure in a far away laboratory due to insufficient knowledge of the overall difficulties would do more harm than good.

I make the following suggestion: I will compile a large, detailed preliminary report for this autumn about the overall work together with Dr. Havrevold and another assistant. The most important experimental, theoretical, and hypothetical principles will be compiled, so that everyone can get an impression of how we work and upon which basis we do it. As much as the experiment is our focus, it is controlled and lead by a specific viewpoint, specifically the dialectic-materialistic. Besides that, we always attempt to secure and maintain an overview of all observed and still questionable detail results. Through this, our judgment is intensified with each specific case. I now believe that after presenting this report comprehensive monitoring will probably be much more feasible. Please write me what you think about this. Anyway, I wish to cordially thank the gentlemen for their willingness to collaborate in this task and ask them only for a little patience.

It is important for you to make the trip here. With one swift move, the entire task would be easier, more supported, and speedier. You could obviously verify the overall scope and type of the task. Of course, you would be my guest and have

everything here you want or need. In addition, Norway is very pretty and you can totally relax here. Please notify me by letter as soon as possible. This may not fail due to financial difficulties.

PS [signed D.O.]

1. I am sending you 2 culture medium samples from the different bion 6c/b mixtures with this post. You can verify for yourself that it is the same structures as those that you described to me. It is the usual growths.
2. I would like to inform you that I was denied permission to perform animal trials here through the official bacteriologist Thjøtta because I am not a Norwegian. In reality, Professor Thjøtta commented to Professor Schjelderup from the Psychological University that I would be better off not getting involved in questions of bacteriology because everything in that field has already been researched. This is simply for orientation purposes concerning situations with details.
3. Please send me the recipe for creating the culture medium that you used to further cultivate the yellow growing packet amoeba. We will guard it with our lives, as we only obtained it once. [added in Havrevold's handwriting?]
4. Did you receive the tissue samples and the vials that burst [when I used them]?

To Professor DuTeil, Nice, June 5, 1937

Preparation 6cb XI

Prepared on April 15, 1937

Inoculated onto bouillon: April 16, 1937

Growth with film: April 17, 1937

Re-inoculated onto agar: April 17, 1937

Continued on agar.

Preparation 6cb VI

Prepared April 8, 1937

Inoculated in bouillon: April 8 and 9, 1937, no growth

Inoculated in bouillon: April 10, 1937: initially very weak growth

Re-inoculated onto agar April 13, 1937

Growth: April 14, 1937

Continued on agar.]

8 June, (bion lab ntbk 2, ms pp. 173-177) “Versuche zur Feststellung der bestwirksamen Art der Bionherstellung Präparat 6”

12 June, DuTeil to Reich, A3 Last shipment of bions, inoculated directly onto agar.

T Ia: Tube [illeg.] culture developed spontaneously on an indicator (test) tube (bouillon) sterilized at 180°.

Three tubes of agar according to *your* recipe. Note: The recipe of the culture medium which you are asking me about is exactly what you had sent me in the beginning, with Liebig's Meat Extract.]

15 June, Reich to DuTeil, “I feel compelled to share a short report with you, which sounds very improbable, and for this reason is all the more gratifying.

I wrote you a short time ago that I heated coal to incandescence and added it to an anapartis [??] mixture of bouillon and potassium chloride. This experiment has been completed ten times up to now, and the microscopic and macroscopic results were positive each time. Incandescent coal in bouillon and potassium chloride “lives.” We have now started different cultures from the cloudy bouillon solution and, after 14 days, observed turbidity in the liquid media. This verifies that cultures from the incandescent coal succeed. I will contact you immediately as soon as these conjectures are verified for certain.

We are now moving on to autoclave all types of the bacteria on hand and all of the structures we won as well as their cultures at different times, so as to determine where the biggest resistance is to heat and which types eventually resist subsequent autoclaving.

A continuation of the autoclaved tissue culture experiments shows a high percentage of positive cultivation failure. [I do not like this translation, but this is what it says. Positive failure is an oxymoron, but that is what is written].

Please write me whether or not you received the several lung, coal, etc. cultures I sent some time ago.

[entire line cut off on original carbon copy]

whether you want to keep your promise to come here in August or September.”

16 June, DuTeil to Reich, I'm bringing you right away a piece of news, which I think will please you. The Academy of Moral Sciences has accepted (for July or September at latest) your works up to the present, and the philosophical conclusions which I draw from them. I preferred to start with the Academy of Moral Sciences where I have connections, and where Baron Ernest Seillère (the Nietzsche historian in France) agreed to present the communications himself. There are 11 pages, the first four of which lay out our work in the context of other works on this topic [OOL]. The next two pages deal with possible interrelations of our results. The final 5 pages propose my new concept of 'life.' In other words, I took the pretext of my philosophical work in order to present your scientific work to the French Institute. We shall have then a very large facility for developing the purely

scientific part in a technical way before the Academy of Sciences. What's more, all the great French press is going to reproduce the report of the meeting, and thereby your name and the Reichweite of your work can be made available to the European public.

When the notification has taken place, we will not wait for it to appear in the Bulletin of Academy Issues, that is always at least a year behind (my last report from November, 1935, just appeared last week!) and, as far as I am concerned, I think I will have something printed myself, two to three-hundred copies, concerning the meeting of the Academy and containing the contents of the report. I will send it to you-if I don't bring it myself if I can visit you-and for your part, you can have it translated into German and English so it can be distributed throughout the scientific community. That would mean official validation of the importance of your discovery, which can only serve you very well, and all the more so, because, although I have partially verified your experiments, I totally disagree with your philosophical interpretation-which gives a lot more weight to my agreement with you regarding the factual parts.

The longer I think about it, the more I agree that it would be very useful if I could visit you. As you have inferred and implied with the utmost courtesy and finesse, and although it is not directly a question of money, I am still forced to calculate precisely and to make arrangements, the results of which I am not yet completely certain. I have, though, already determined that a boat travels every Saturday from Antwerp to Oslo, the price of which is not too high, and I can hope to afford it. On the other hand, it would be absolutely necessary to bring my secretary and assistant with a degree in philosophy; for I wish her to see how the conscientious and difficult recordings and annotations of the control experiments take place, so that she can, if necessary, supervise any help that I consider hiring when I return to the Municipal Laboratory. My friends are currently trying to get me material assistance from the French government to facilitate the experiments that I intend to complete parallel to yours. And I believe that this will succeed, thanks to this report to the Academy of Ethical Sciences (in Paris, at that). Then I would have all of the necessary materials, so that I can duplicate any trials you might send me and at the same time secure the necessary controls and preparations.

Returning to the topic of the planned trip, I could, for example, board in Antwerp on Saturday, July 24, and would arrive in Oslo on Tuesday, July 27. I first considered leaving on Friday afternoon, July 30, when the ship sails, but feared that three days would be a somewhat short time period to follow the processes of a completed experiment and could, if it's not too much for you, stay until next Friday, August 6, and then sail. If you think, though, that three well-planned days could be sufficient, I could either first leave from Antwerp on July 31 or also even return from Oslo on July 30th. Then I would worry that I would be bothering you for more than a week. On the other hand, your very gracious offer to host my visit embarrasses me a bit. I would have come were I solo, but now that there are two of us; I really have scruples to do it and will only partially and gratefully accept your invitation-at the same time in the hope that I, on my side, will have the opportunity to have you stay with me when you come to France.

Please be so kind as to answer me via airmail whether those dates are alright with you, for without a doubt it would be good if I could already reserve my seats on the ship (there seem to be only a few cabins), and tell me whether you think it would be better for me to stay three days or ten days. Furthermore, we must reach some understanding concerning our plan of operation regarding the preparation of the academic paper and the number of copies needed. By all means, procure a copy of the Saturday evening edition of "Temps" published in Paris (with Sunday's date). They publish a report of the meeting of the Academy of Ethical Science each time under "Latest News." This way you will also know the day on which the report went through. I am looking forward to the possibility, which seems to be developing, of seeing you again and am very sincerely yours...

PS I have sent you two tubes per registered mail of which one contains a re-inoculation from one culture onto agar which spontaneously started to develop in a control tube of bouillon during our first culture trial! The tube was (empty) sterilized at 180° and the bouillon was simply poured in using the same precautionary measures as with the tubes that were later inoculated. You can compare this airborne infection with our last experiments. I have the impression-macroscopic-that it is absolutely not the same. Tell us what you think after examining it microscopically."

17 June, Reich to DuTeil (corresp box 10, DuTeil flds***) Thank you for sending both cultures. I examined them immediately the next day using the included protocol section. I did not understand why the one tube was labeled 0¹ α when T1a was written on the note. Summing up, I would like to say that the structure that resulted in your spontaneous growths have not been noticed here up to now. We continued with both cultures. Your bion growth A3 is known to us in its shape, color, and electrical reaction to be a pure cocci culture.

I would like to mention that today our colleague Dr. Havrevold, who works with me, reported "quite a stir" amongst the academic staff at the local university. Not even one of the local experts has taken a glance at our work and yet this big commotion. I personally have not announced anything about our work. You will receive an exact report concerning this matter shortly.

That's all for today.

PS[in Havrevold's handwriting?] Please verify my shipment of cultures was coal and tissue."

17-19 June, (bion lab ntbk 2, ms pp. 178-180***) specific lists of cultures sent to DuTeil at this time.

19 June, Reich to DuTeil, I'd like to thank you above all for your extraordinarily friendly and productive help that you have provided to our issue. The value of your scientific and tactical assistance is in this moment especially of great meaning because the eugenicist and genetic scientists at the university here are immensely upset about what goes on in my laboratory. I had, for

example, requested permission for animal trials, likewise my assistant, Dr. Havrevold, who already had prior permission but wanted it verified by another authority. The Medical Director told me, under pressure from some of the University's professors, that I would under no circumstances be given permission for animal trials. Dr. Havrevold was directed to withdraw his request so as not to embarrass the university staff. The local professor of bacteriology, Professor Tjhötta, told Professor Schjelderup from the University of Psychology with whom I am friends that we would be wise to cease all work, for everything in this area had already been thoroughly researched. I understand the distress very well. These examples should be enough to show you that my work is not carried out as peacefully and smoothly as I wish it were in the interest of the issue. Specifically for this reason is the work that you have and will carry out of all the more importance.

Now to the business part of your letter: We will adjust ourselves here totally according to you. If you wish to be here on 07.27, then everything needed for your work will be ready. I would be delighted to have both you and your assistant as my guests. You can look at the house where I live yourself and decide whether it offers you the comforts that you desire. There is absolutely no chance of you disturbing me in any way. Quite the opposite, I am looking forward [to showing you] the positive results as well as the questionable, the uncertainties as well as the certainties. Three days are in no way enough to do this, and besides that, it would be much too strenuous to undertake such a long journey and only stay three days. I would even suggest that you take more than ten days, for I am convinced that not only will you enjoy the work here, but you will also really like the country of Norway. Of course, I do not know how you handle your schedule. In any case, ten days is the absolute minimum amount of time we will need in order to get some degree of overview about the whole nature of the problems. However, I think that we can decide the length of your stay and where you will stay when you get here, if that is alright with you.

[line missing]

to question it due to money concerns. I will think about a plan for completing the experiments, so that we have the best possible arrangement to work together and complete the most important experiments ourselves.

I have not told you everything that has happened here during the last 1-1/2 years and would like to use this opportunity to show you some very questionable yet extremely promising facts of the case. By then we will have copies of three films ready that you can watch here and eventually take with you.

I would now like to inquire whether it would be alright with you if I include your lecture in Nice and current drafts of the work in the first reports about the biological experiments that I wish to publish in the fall. I must know now, so I can prepare it. Your papers will, of course, be translated and published throughout the world through our publishing house. We have connections to almost all countries as well as universities and institutes outside of France.

Next week I plan on going to the country to relax for two to three weeks. Please write to me as always at the address Drammensveien 110H.

You are absolutely correct in your evaluation of spontaneous growths. Day before yesterday I sent you the findings from both tubes. They are definitely different things.

In closing one suggestion: If possible, leave yourself enough time so you can travel through the Norwegian country with me. Please send me your lecture as soon as you can to be translated into German and English. I spoke to a woman from London who studies under me and has connections to biologists there. She can be of great use to us.

Now I believe that I have reported everything necessary to you. I am thrilled that you are coming.

PS A preparation consisting of incandescent coal in bouillon and potassium chloride is being sent to you today.

Reich note: Sent to Professor DuTeil, Nice, on June 18, 1937, 3 ampules of preparation 2 IX e and 3 ampules of preparation 2 IX e/c

Production: Grate coal to dust, put on a spatula and heat over a gasoline gas flame about 1/2 minute to incandescent red, immediately put in a solution of equal parts 0.1N potassium chloride and meat bouillon.

2 IX e not autoclaved afterwards

2IX e/c afterwards autoclaved one half-hour at 120°

Observe in dark field at 3/400, in bright field at 2500-3000 X magnification curved binocular lens barrel after 24, 48, 72 [hours], and after 10 days.

Shake prior to observing.]

Reich note: "Am 18.Juni 1937 an Professor DuTeil, Nice gesandt: 3 Ampullen Präparat 2 IX e und

3 Ampullen Präparat 2 IX e/c

Herstellung: Koks zu Staub zerrieben, auf einem Spatel über Benzingasflamme etwa ½ Minute rot geglüht, sofort in Lösung von gleichen Teilen: autoklaviertes 0.1N KaliumChlorid und Fleischbouillon.

2 IX e nachher nicht autoklaviert

2 IX e/c nachher eine halbe Stunde autoklaviert bei 120°.

Zu beobachten im Dunkelfeld bei 3/400, im Hellfeld [bright field] bei 2500-3000 facher Vergrößerung geneigter Binocular-Tubus nach 24, 48, 72 Stunden und nach zehn Tagen.

Vor dem Beobachten schütteln.

June 37, DuTeil French trans of Reich to DuTeil (summary promised in 24 May letter? Or sent with 18 June culture shipment?, E. Sellers trans.) "Experiment with coal at red heat

As the experimental work on coal and earth subjected to incandescent heating is pursued, always the first observation to be confirmed is to find out if these substances manifest much more movement or 'life' if they have been subjected to firing or not.

However, the objection that spores preexisting in these substances could be liberated by firing was always and again formulated, not concerning the precise case, but with a particular predilection in bacteriological milieu [media], from the moment that one made known the succession of completed observations. In other words, [they admit] nothing unusual there: some spores pre-existed in the substances used, which firing liberated.

It became evident, then, that the simple observation consisted of the firing test, followed by comparison with the same substances not subjected to firing, was not enough to definitely refute the interpretation of preexisting spores.

I'd like from the present time and above all to indicate that I never doubted for a moment the existence of spores, from which living things can be developed. But all my observations were contradicted if one maintained that these spores had always preexisted there.

There was, in fact, much to think about against this hypothesis. How then would these spores have been able to penetrate to the interior of the coke distilled at thousands of degrees temperature? How could the spores appear in sprigs of grass or in laterally striated and consequently structured muscles when they were invisible, even with the highest magnifications under the microscope in the state that these substances were in before the firing? And why were they there, all of a sudden, right after the muscle, the moss, the grass, the coal or the earth had been subjected to firing? The more rigorously these considerations contradicted the absolute theory of spores, the more the objection remained then theoretical, speculative: it was not founded experimentally.

I was finding myself, then, in need of imagining an experimental arrangement which would let me clarify the matter.

What I'm going to describe in the following pages might give the impression that the work developed in a unified fashion without difficulties or concerns. It is the contrary which is the truth. There were weeks and months of cessation of work during which everything seemed closed in an impasse until a very simple circumstance resolved the issue.

In the previous experiments with firing, the sterilization and the liquefaction only formed a single and same operation. I had then the idea that one could separate the one from the other, effecting ipso facto death by destruction of preexisting spores, and the liquefaction by which life manifested itself. And this in the following manner!

First I sterilized the coal, the dust, the earth, the moss, etc. in the dry sterilizer at 180° [190°] for two, three, or four hours' time. It was from that time certain that, in this way, I had followed the strictest rules of sterilization, that is to say, of destruction, by death, of the germs.

Then I mixed the substances which had been treated in this way with previously autoclaved chlorure of normal potassium at 0.1 [0.1 N KCl], and then I subjected these substances to incandescent heat until a colloidal turbidity resulted.

Observations undertaken immediately by microscope revealed life of a great motility. All the forms of cocci, of vesicles, of rods [bâtonnets] of all sizes were present (with the exception of the crawling forms) and mobile amoeboid constructions, like peripheral mobile parts of larger formations at rest. The motion was quite different from this tremor in place [i.e., Brownian] which milk not subjected to firing sometimes shows.

The notes about the experimentation which follow this test show that coal and earth as well as moss treated in this way have produced several cultures on bouillon. But the results of the cultures remained by and large uncertain. However, the culture formed one of the most important elements of all the work.

If a culture did not succeed, there were two possible hypotheses: Either the substances, completely dried by previous sterilization, dry, had not been subjected to firing a long enough time to attain a necessary softening of the particles and their liquefaction.

Or, a second hypothesis, and in particular for coal, the substances forming the nutritive milieu of the culture was not complex enough to nourish and preserve life [that is just] in the process of starting up. The examination under the microscope, on the other hand, always produced the same typical result provided that you had pushed the firing to the production of the colloidal turbidity.

I'll assemble in what follows the operations that I have undertaken for the said experiment of coal and earth. As for the moss either placed in the autoclave or subjected to firing, it will show, as opposed to the moss not subjected to firing and *just as soon as* the preparation is produced, all sorts of movements. One of the moss preparations, placed in the autoclave, produced a pure culture of very peculiar rods [bâtonnets], moving in a jerking way and gathered in vesicular formations. Then we decided to place all the tissues possible for us to obtain in the autoclave and to experiment with them carefully, both under the microscope and also by cultures. Doctor Havrevold will carry out, in a separate work, experiments effectuated on tissues.

21 May 1937: I thought all of a sudden that we could test the metaphorical theory of spores in a very simple and also radical way. If not, our work would always be troubled by the objection—expressed by foreigners to these works—that it is a question of

preexisting spores. It was necessary then, and we could do it, to make an experiment with coal after which it would be impossible to invoke even the smallest pretext of speaking of preexisting spores.

The experimental method adopted is very simple. Its results can be 'seen,' observed visually in an indisputable way on the sole previous condition that you possess a binocular microscope capable of 2500-3000x magnification by immersion. I note in passing that this experiment was carried out numerous times, ten times in succession by series and always produced the same result.

The coal dust which I usually keep at 180° was placed on a thin metal spatula and heated in a gas flame at 1000 to 1500 degrees until the piled up mass of coal dust became red, without however falling in cinders.

However my assistant made a phosphate bouillon, which she mixed with equal parts of 0.1 N KCl, placed in the autoclave. In this way any fears were disposed of, that the nutritive media or the substance needed to produce liquefaction contained any preexisting spores.

The red-hot coal was immediately incorporated into the bouillon-KCl solution. To our immense astonishment, in all cases, when we renewed the experiment a momentary turbidity of the solution manifested itself, such as we had never previously obtained from a simple firing.

In addition, a phenomenon which followed this was extremely rich in lessons and surprised us very much. The coal particles colored the solution black, but in 10-20 minutes' time the black coloring began to disappear, to be replaced by a cloudy turbidity of a gray color. The solution remained turbid and cloudy, as though it contained milk. The turbidity increases at first, then the numerous particles fall to the bottom, but by shaking you can easily bring them back into suspension. By microscope an animated picture of rods and cocci of all kinds was revealed each time.

Research under the microscope was methodically carried out on this solution, starting immediately, then after 24, 48, and up to 72 hours, at a magnification of 3000x. It was then revealed that immediately after the establishment of the preparation, a very motile life was born at the same time. A sample of burned coal dust—burned but dry—was placed on the microscope slide. Already at a mere 200x magnification, we saw in dark field some very small vesicles from which crystals were escaping. As soon as we added KCl, we saw movement begin. Small vesicles which had the appearance of spores were moving. The large particles of coal sucked up the liquid avidly.

At the end of one or two minutes we saw how the large particles of coal, which on their edges had become entirely vesicular, attracted the isolated vesicles to themselves out of the liquid. The movements of the isolated vesicles toward the large particles of coal gradually sped up as the distance diminished. Quite at the end of the experiment, the vesicle rushed onto the large particle, and remained attached to the edge of it. It was a question, undoubtedly, of electro-magnetic phenomena. At a magnification of 3000x we could not distinguish coal particles from living formations. They were crawling all around the field. The median layer and the upper layer of the preparation were clearly more motile than the layer resting on the bottom. At the end of 24 hours, the KCl solution was full of cocci and rods moving and changing positions. The microscopic organisms showed themselves to be positively charged electrically. We recognized immediately their blue-black color, which was appearing. On the edge, on the periphery of the largest formations, there were strong vibratory movements even after the preparation had been refrozen. It was not a question, then, of heat phenomena. One of the preparations, produced after an interval of 24 hours, produced a rather strong turbidity with a thin pellicle. In the others there was no pellicle, and a culture inoculated in bouillon did not produce any result for the other preparations, which were also turbid, for reasons still unknown.

In order to prepare the rest of my work, I must form at least a provisional hypothesis for myself from the totality of these phenomena.

In a coke crystal, which has already undergone a distillation at a very high temperature, individual particles are very strongly bound, one to the other. The heating, in particular the ignition, destroys the cohesion of the particles, which liberates the electrical energy, for without that the particles would not possess an electrical charge individually if the large pieces don't have any charge at all. These particles, in which the red-hot coal disintegrates, absorb the liquid quite avidly; you can see this directly under the microscope at 2000x, if you add to it some KCl solution. You then see appear what is undoubtedly electro-magnetic phenomena. The particles have absorbed the liquid and have been transformed into vesicles, which from the point of view of form cannot be distinguished from spores.

This operation seems to me to completely contradict the spore theory, at least the interpretation of it which prevails at present; for nobody could maintain that spores can resist a temperature such as that attained by the red-hot coal. To this would be added the other test, to find out if there existed among the diverse substances I've considered any of them which subjected to firing, placed in the autoclave, liquefied or heated to incandescence would assume a vesicular structure. And the vesicles cannot, as I have already said, be distinguished from spores in appearance.

I do not deny then the existence of spores in the sense that the previous (former) theory understood it. I affirm only that the vesicular character and the disintegration of the organic matter obtained by liquefaction, firing, or heating to incandescence, constitute the central point from which one must depart on the deep study of the formation of spores which are capable of living

(fit for life). The spores themselves should also take birth and, in truth, by the liquefaction [swelling and bionous disintegration?] of the substance. This seems to me to be an entirely indispensable hypothesis.

The experiment with incandescent coal is only astonishing if one starts with the principle that living formations—seen after liquefaction—were already in the substance employed in the form of spores, for it is then unexplainable that spores could endure such temperatures. But this point of view is incorrect. The vesicular elements—let's call them simply (calmly) 'bions'—from which the motile organisms of the bacterial kind then formed themselves, were not at all preexistent in the interior of the substances, but took birth first via the heating to incandescence, then during the liquefaction [swelling] by the disintegration of the matter. [This reminds me of Richard Owen 1868, mocking those who believe in 'germs' to avoid facing the reality of heterogenesis and saying they really amount to a revival of preformation theory, discredited for over a century!]

On this subject, the idea involuntarily presented itself, that all forms of spores presently known must have appeared one day, and, in truth, at the time when the Earth was red-hot. They must have developed at the time when substances previously having become incandescent, came in contact with water and substances capable of moistening them. I grated some pebbles into fine dust; I burned them in the gas flame until they were visibly red-hot, and I mixed the red-hot dust with KCl solution. There also one saw a molecular disintegration of the vesicles in movement, without doubt of very short duration of life.

In the battle of ideas, they considered up to the present time either that the living was something completely separate from the nonliving, or that, to the contrary, 'everything is probably alive.'

There is nothing to be gained from such generalizations. We must say provisionally: It is unlikely that all unorganized matter contains within itself the possibility of becoming living, according to its state. Life may suddenly appear everywhere if the necessary conditions are fulfilled, for a longer or shorter time. You may see biomorphous movements which last only a few seconds. Elsewhere you may see the same movement which only dies out after weeks or months. The maintenance of life which appeared suddenly in this way depends on two essential conditions:

1. The chemical composition of the substance
2. The biological conditions and the milieu of the culture in which life finds itself installed."

23 June, (bion lab ntbk 2, ms. p. 184-185) "Versuche zur Entstehung der Frage der Herkunft von Luftkeimen

Bisher waren Bionversuch keine Widerlegung der Luftsporen—Nun steht die Frage:

Was gibt es in der Luft??

Wieviel Typen der Bakterien?...

[ms p. 185] "Verschiedseitigkeit der aves Aufwuchs der Prus bleiben der offen Agn sprould gegen Luftkeime—Aller- Theorie"

Experiments addressing the question of the origin of airborne germs.

Up to now bion experiments did not disprove airborne germs—Now the question remains:

What exists in the air?

How many types of bacteria?...

[ms p. 185] "Lateral differences of [???] growth of crusts the open [???] remain [???] against airborne germs-general theory."

DuTeil to Reich, 28 June 37 (includes letter from Ernest Seillere to DuTeil, saying sorry there are no more openings at the meetings of the Society, up thru at least the time of our vacation. I hope that, as I told you, you can read your paper, with the change to p. 4, at the meeting of 18 September.) D to Reich: "I enclose a note from Baron Ernest Sellière, Perpetual Secretary of the Academy of Moral Sciences. It sets the date of Saturday 18 September for my talk. Nevertheless, we shall be able to print it, all ready except for the cover bearing the date, so as to be able to send it to the scientists whom we wish to reach, as soon as the paper has been read, profiting in his way from the reports which will have appeared in all the great French press. Also, you will be able to have it translated ahead of time so that the circulation may take place for the October session.

I send you then this text: but, of course, completely confidentially since it cannot be circulated before the meeting of the Academy. Also the text of the short note which M. Selliere sent to me this morning, who also confirmed to me that he changed page 4, of which I sent him anew text, as you will see.

I am sending you by this mail a tube of agar with a culture of liver. The carbon did not produce any change, as foreseen.

I am planning my trip. It's hard for reasons of economy, to go via Stettin. I'll go by car, by my own means, to Berlin where I'll leave the car. We'll take the train then from Berlin to Stettin, where on the 27th we have a boat which is the least expensive of all and which should put us in Oslo on the 29th. The price is half that via Antwerp. Despite that, I shall be very exact, both for the departure cost which I am collecting here now and for the sums of money for the return, for which I shall secure here my pay of 1 August. I tell you that very simply as to a friend, from whom I have nothing to hide. I am, in any case, very touched by the kindness with which you offer us hospitality, which I accept with gratitude for my assistant and myself if, as I firmly hope, our trip can be made. The trip via Stettin gives us more latitude for fixing the date of our return. On the other

hand, we can make it from Strasbourg to Berlin in a day and a half, which reduces the expenses since my car consumes very little [gas].

I'll be at Nice until [Weds] 7 July.

PS The text of the communication is, according to the custom of the Academies, 'ne varietur.' But anyhow, it will be possible for us to add to it whatever we want, in notes, while specifying that it is a question of subsequent addenda.]

DuTeil to Reich, Weds 14 July 37 (corresp box 10, DuTeil flds) "You have read my manuscript very well, and you are right when you say that we are less removed from each other than it might seem. The solution I propose is indeed only provisional, and I point this out very clearly for anyone who knows how to read. But while waiting for monism, toward which I tend, to be clear enough to satisfy reason, I think that it is necessary to maintain a dualist interpretation as possible, if only to avoid the dualist spiritualists from feeling obliged to deny the reality of the experiments.

On the other hand, it isn't bad that we appear, you and I, to have different tendencies. That lends much more might to the guarantee that I bring about the material facts. That is exactly the significance and the aim of the Communication that I had the fortune to have accepted by the Academy of Moral Sciences. And I think that upon my return from Oslo I shall have the possibility of a Communication at the Academy of Surgery, about which was able to interest an eminent member, Dr. Prof. Charles Monod. At this time an with the films which I shall bring, we shall have easily won over the Academy of Medicine, I think. And the Academy of Sciences will also be obliged to welcome us. They will do so all the more willingly so that I'll be able at this time to report an effective participation in your works which will involve France a little in this affair! There is a whole diplomacy to be deployed with these old scientific organisms! Fortunately, I pretty much know all these detours. It is also true, on the other hand, that all these precautions and these difficulties lend a solidity and an incomparable certainty to the discovery which is accepted.

The second part of your letter touched me very much and also reassured me. For I can now confess to you that I have had great difficulty in finding the necessary funds for our round-trip tickets across Germany and our navigation tickets from Stettin to Oslo. But I was wondering how we would do to return from Oslo to Stettin; so much did these unfortunate bank notes, obtained with great trouble (indebting me for three months), melt away literally in my pocket—especially with the new evaluation of French currency. Consequently the acceptance of your offer is for me today a true necessity, to which I apply myself all the more willingly since you claim that there are the funds from your Institute and that this won't inconvenience you personally. I had certainly intended to come alone, which would have evidently facilitated the matter. But for the smooth running of the works that I should carry out at Nice, it is absolutely necessary for my assistant and collaborator to be seriously initiated into the running of as important a laboratory as yours. And besides that, the notetaking that I want to report requires me to have a stenographer at my side, a talent which this person also possesses. The only problem is that at this time she's a little sick. But hope that this will be of short duration and that she may accompany me, seeing that the tickets to Oslo have already been purchased.

I should leave Stettin on the 24th on the ship 'Vistula,' but I change ships at Copenhagen, where we will board the 'King Haakon,' which should arrive at Oslo the 26th at 1 o'clock in the afternoon. Theoretically, we should reembark at Oslo on Saturday August 7th in the evening, which will give us twelve and a half days to spend with you.

I am resting at this time at the Abbey of Pontigny, about which I have several times spoken to you, for one meets many intellectuals and scientists there. There is a very friendly curiosity about your works. I have been invited to Paris for the meeting in a few days of our Commission of Biology. I expect to be very reserved and to ask for a restarting of the works upon my return from Oslo. There is, besides at Pontigny, at this time an all-Swedish and Norwegian college, with different professors from Oslo. I am very prudent, but I do not lose an opportunity to affirm the seriousness of your work. This work is useful. It has already borne its fruits. The sole fact that I said I conducted the experiments in parallel [with you] at Nice and that I am making the trip to Oslo has made a deep impression, just as at Pontigny the French intellectuals who stay there or pass through represented me to the Nordic people as a master scientist. These people are going to return to Oslo reporting that you are well-known in France and that the most authentic scientists admit the validity of your work. A well-known French writer with whom I spoke last evening spontaneously pronounced the word 'Nobel Prize.' When the question comes up, there will be a very serious movement in this direction.

I'm leaving Pontigny without doubt on the 22nd of July, departing from Strasbourg on the 23rd, I'll be in Berlin on the morning of the 24th, and in Stettin at 5:45 pm. Perhaps an airmail letter could reach me at Pontigny before I depart. My assistant will join me at Strasbourg.]

Reich to DuTeil, 16 July "I interrupted my vacation for one day and found your letter from the 10th of this month at home.

I would like to wait until our oral discussion to express my deep appreciation and my great admiration for your enthusiastic help and, of no less importance, responsiveness. Your work on this matter is, indeed, of invaluable importance.

I will be expecting you on July 26 at 1:00 p.m. on the ship arriving from Copenhagen. I have refrained from preparing a special program for our work here. I thought it would be better if we thought about the best course for experimental events together. It is excellent that you wish to stay here until at least August 7; confidentially, I hope to have you here even longer, so that I may show you Norway.

The work that you sent to the French Academy [DuTeil's article "Leben und Materie"?!], has been translated very nicely in the meantime and will be combined with the publication materials for the coming fall. I will now take you with me to study it thoroughly and completely.

I am extremely happy to see you again. Write or telegraph, if necessary, to Post Box 2806, Oslo-Kampen. Your best chances of reaching me are there."

Sunday 25 July, DuTeil telegram to Reich: "My assistant is sick: I'm coming alone."

26 July- 7 Aug. 37, DuTeil visits Oslo lab of Reich. Travelled by way of Stettin and Copenhagen (Pulse p. 62) See photo Aug. 37 (*Beyond*) with Brandt, Berggrav, Berle, Hauser, E. Kohn, Odd Havrevold, DuTeil, WR, Odd Wennesland.

Tues 27 July, "First Day of Experiments with Professor DuTeil" (OI box 7, "Partial English translation" fld, ***18pp) 2pp missing from the beginning of the second day (not translated) are copied in orig. German from "Protokolle der ausgeführten Versuche vom 27.7 bis 5.8.1937 unter Mitwirkung von Professor Roger DuTeil am Ausführenden Laboratorium" notebook/ fld (also in OI box 7), to insert between pp. 6 and 7 of the partial Engl transl. ["Egg-white proved to be non-sterile in the control experiment and is unusable due to the contaminated egg-white. Culture was not started, as it could not be used for verification purposes.

Control experiment ++

1. Non-sterile moss to be used as a control for autoclaved moss must be freshly produced daily. Even one-day old non-sterilized moss already contains considerable movement.

Autoclaved moss from 7.27 filled with motion in dark-field, much denser as the day before, even denser as the non-sterile moss from the day before.

The structures in autoclaved moss are formed very differently from those found in non-sterile moss.

Sterile water is necessary as a control for autoclaved moss compared to non-sterile moss.

2. The annealing charcoal [incandescent carbon] was apparently not heated enough, and showed less motion today than yesterday.
3. Microscopic examination of cloudy control bouillon # 3 egg-white
Results of the microscopic examination of the control medium showed long, stringy, moving substances originating from the egg-white.
The question remains whether the long rods were produced by egg-white decomposition or by an external infection.
Experiment with egg-tar to determine source of infection.
4. Demonstration of tuberculosis bacilli as induced structures.
5. No motion apparent in the non-sterilized charcoal and water mixture from yesterday.
Argument for Brownian motion refuted because, depending on the conditions, non-sterilized charcoal can show movement, whereas annealed charcoal is dead, and vice versa.

Were the movements of annealed charcoal due to Brownian motion in the traditional sense of the word, then each type of finely ground charcoal must produce motion. This is not the case.

Fresh non-sterile moss added to sterile water, when compared to yesterday's autoclaved moss, shows no movement and no or only very few, individual vesicles. The non-sterile moss will be filled with bacteria tomorrow, whereas the autoclaved moss was filled with bacteria immediately after autoclaving.

6. The non-annealed charcoal in water from yesterday's experiment has completely sunk to the bottom. The annealed charcoal in bouillon and KCL still shows colloidal turbidity.

Necessary for inoculation experiments with incandescent charcoal:

- a) Pronounced colloidal turbidity in the liquid.
 - b) Particles with a strong positive electrical charge.
 - c) Intense, microscopic movement
7. Previous day's bion culture shows pronounced sediment in the liquid, which has an uneven turbidity.
 8. White staphylococci – negative. This diagnosis is always based on simultaneous testing with electrical charge, staining, and culture medium and never based solely on color change.
 9. Magnification 3000X with an immersion lens demonstrates the motion of tubercle bacilli.
 10. Microscopic observation of pseudo-amoebae movement in an entire tubercle bacilli cluster."]

Appendix (pp. 11-16 of Engl. trans.) is dated 1 Dec. 37*. From the same original German notebook of DuTeil's visit: 6pp*** from 6 Aug. 37 "Zur Mitnahme fuer Herrn Professor Roger DuTeil, Nizza"

[Oslo, August 6, 1937

Copy designated for Professor Roger du Teil, Nizza: Protocols of completed experiments from notebook

Vials

- | | | |
|------------|------------------------|---|
| 1. Soil db | in KCl N/10 | db: Dry sterilized at 180C for two hour boiled in KCl for ½ hour |
| 2. Coke | db in KCl N/10 | db: Dry sterilized at 180C for two hour boiled in KCl for ½ hour |
| 3. Soil c | in KCl N/10 | c: Autoclaved non-sterilized soil in KCL at 120C for ½ hour |
| 4. Soot e | in bouillon + KCL N/10 | e: Annealed (heated to incandescence) |
| 4. Soot e | in bouillon | e: Annealed (heated to incandescence) |
| 5. Bions: | 6ab XXXIII | ab: Boiled mixed, non-sterilized materials for ½ hour. |
| 6. Bions: | 6ac XXXIII | ac: Autoclaved mixed, non-sterilized materials for ½ hour at 120C |
| 7. Bions: | 6ae XXXV August 2 | ae: Mixed non-sterilized materials, autoclaved ½ hour at 120C |
- August 4 Autoclaved again two days later at 120C for ½ hour

Test tubes

- | | |
|---|-----------------------|
| 8. Lung tissue in KCl and Ringer solution (1 : 1) | Autoclaved for ½ hour |
| 9. Lung tissue in CaCl ₂ + Ringer solution (1 : 1) | Autoclaved for ½ hour |
| 10. Liver tissue in KCl + Ringer solution (1 : 1) | Autoclaved for ½ hour |
| 11. Liver tissue in CaCl ₂ + Ringer solution (1 : 1) | Autoclaved for ½ hour |

Culture Media:

12. Packet amoeba (6 as I/40)
13. 6 ch IV/34
14. Coke db on agar
15. Soil db inoculated on agar after 6 weeks
16. 6 cc XXXII/1 direct in blood-agar
17. 6 cc XXXII/2 in agar on top of egg medium (protein medium)

Controls:

- | | | |
|-------------|---|--------------------|
| 18. Moss a | inoculated on agar, in water | a : non-sterilized |
| 19. Milk a | inoculated on agar | a : non-sterilized |
| 20. Milk c | inoculated on agar | c: autoclaved |
| 21. Agar g | left uncovered for 8 days at room temperature | |
| 22. Water c | on blood agar | c: autoclaved |
- 3 blood agar
3 egg medium]

Liebeck to Siersted, Weds 4 Aug. 37 from Oslo (Pulse p.62) "DuTeil's visit influences everything nowadays. He is a marvelous man and we all like him. He is simple, natural, and very serious in his work.

Thurs 5 August, Reich to President of Commission on Biological Studies, CEP, Paris, I would like to include a few words of explanation with the conveyance of two films by Professor Roger DuTeil. It is extremely important to me to stress the fact that earlier mistakes are noted again and again and improvements made throughout the entire task, including film production, in accordance with the fluid and continuous nature of the experiments.

The film of Preparation 6 on the whole shows the main features of an experiment as it is completed in the here and now. Only in one section of the film, there where long, moving rods are seen amongst the amoeboid formations and cocci, do we have a justifiable doubt whether these rods correspond to actual bion constellations or whether they are a result of uncontrolled processes from decomposing egg white. We could deactivate them in later bion productions; however, there is a possibility that these formations are endogenous in nature, a question which we will finally clarify in experiments that will follow in the fall. In contrast to the long, moving rods, the amoeboid formations and especially the cultures on agar, the so-called packet amoeba, have been determined absolutely without a doubt to be results from the bion mixture.

We do not view the Preparation 8 film as strict proof of the origin of protozoan from the process of abiogenesis, although one can make clear observations in the film that indicate this; for example, the different forms of vesicular clusters and round, uniform surrounding formations which touch the stems of the plant fibers, grow out of them, and have the same structure. There are only two places in the film that I would like to present as proof. In slow motion, one shows a formation enclosed with a border that displays interior organization phenomenon. The other point of evidence is the one following the text "Watch Closely": There we see the edge of a blade of grass pop up and organize itself. We also see the beginning of movement. I would also like to point out that the Preparation 8 film suffers from technical defects caused by incomplete equipment at the time of its manufacture.

I hope in the course of next year to provide complete, rigorous proof by using slow motion filming to record the entire process. We are currently faced with large technical problems in the production of the film because coating the necessary gathering preparations with paraffin hinders the development of protozoan.

The statements above should express my assurance that I consider the task as neither rounded nor successfully completed, rather I see gaps in many places that can only be completed and strengthened through very complicated and lengthy control experiments.]

From 5 Aug. (next fld after letter to CEP, Paris), List of Films, Preparations, Cultures, etc. [sent with] Prof. Roger DuTeil on 5.Aug.37 to be turned over to the Biological Commission of the CEPH for demonstration and other necessary applications.

- I. Films Film of the Preparation 6: bion experiment (copy)
Film of Preparation 8: pictures from an unsterilized protozoan preparation
- II. II. Preparations:
Vial and preparation of paraffined soot heated to incandescence
Vial and preparation of paraffined charcoal heated to incandescence
Preparation 6a, 6cb, and 6cc in vial and preparation
All cultures that were set up and evolved in the presence of Professor DuTeil.
Two vials of egg white culture medium
Two vials of blood agar
6 cl of packet amoeba
Amoeba culture from the first autoclaved bion mixture
Autoclaved moss
Autoclaved tissue

III. Protocol of experiments [signed, Roger DuTeil]

(next p. in same fld) August 6, 1937, "Professor Roger DuTeil, University of Nice, as of today assumes possession of a Reichert Z inclined binocular microscope (production number 159620-1937) with optical equipment up to a 3750X magnification (microscopic objective lens 10X, 45X, 80X water immersion and 100X oil immersion, and eye pieces 5X, 16X compensation and 25X compensation) and likewise the corresponding Reichert spotlight for the purpose of controlling the biological experiments of our institution at the University of Nice and to demonstrate these results from case to case to interested parties. The microscope is lent to Professor Roger DuTeil until further arrangements and remains property of the International Institute for Sex-economic Life Research." [signed, Roger DuTeil]]

Sat. 7 Aug., (bion lab ntbk 2, ms pp. 186-191***) Schedule of events during DuTeil's visit to Oslo.

Sat. 7 Aug. 37, "Der Besuch Professor Roger DuTeil's von der Universitat Nizza in Oslo vom 26.7 bis 7.8. 1937" (corresp box 10, DuTeil flds*** "The biological experiments at the Institute for Sex-economic Life Research in Oslo were confronted with very different mindsets in the scientific community long before they were published in their totality.

Two very unscientific types of reception of the first reports concerning biological tasks will be mentioned because they took on a certain practical effect. Specifically, strange rumors concerning the type of experiments were circulated in different venues, for example, it was told that the experiments were totally insane, were implemented fully indiscriminately, that, while the work of the institution in the field of psychotherapy might be good, its work in the area of biology was total nonsense, etc. These rumors originated in part from scientific assistants that viewed the experiments in a highly superficial manner. One cannot usually counteract rumors with rationality, but one can establish facts that are well-suited for eliminating their credibility.

The first assistant in the electrical experiments on the human body was Dr. Löwenbach, who, after being fired by the Department of Physiology at the Kaiser Wilhelm Institute in Berlin, accepted a position with Dr. Reich as an assistant in the experiments with electricity. Wilhelm Reich eliminated this assistant position in September, 1935. The final episode, which was preceded by many other episodes, was as follows:

On that occasion, it involved the deciding question whether the potential fluctuations that Reich had observed on human skin during different experimental trials were specific biological phenomenon or whether they could be produced in non-living material. After a period of great initial enthusiasm for this task, Dr. Löwenbach hindered the experiments by denying the existence of indisputable phenomenon that he had observed and verified himself.

The oscillograph, all of the equipment belonging with it, were at that time in the Insane Asylum Dikemark (Head Director Dr. Jessing). A catatonic patient was hooked up to the apparatus, and tickling the palm of his hand with dry, insolated cotton showed the familiar potential fluctuations already known to us ("Tickle Phenomena). Drs. Löwenbach and Hoffmann, who witnessed the experiment, could not decide whether or not to acknowledge the organic nature of this phenomena and attempted to prove that the sheet upon which the patient was resting was also "alive." The electrode, previously resting on the palm of the hand, was put on the sheet and next to it "tickled" with the cotton wad. To my great horror, this experiment showed exactly the same potential fluctuations as touching the palms of the hands. Dr. Löwenbach and Dr. Hoffmann were triumphant, and Dr. Reich was very surprised because he could not explain the anomaly. It contradicted not only his assumptions but also his previous experiments on inorganic matter. After 24 hours, it occurred to Reich what error was committed by the experimenters and how it had resulted in the anomaly.

- 1.) The neutral electrode that lead to the amplifier's cathode was not removed from the patient's leg. His body was, thus, still part of the circuitry.
- 2.) The patient's hand rested on the bed sheet, which was moistened with potassium chloride. It was no wonder that one had seen reactions on the bed sheet. With this, a new problem arose: By which biological means could the body's electrical charge be transmitted through the bed sheet.

These are the true facts of the case that were the grounds for Dr. Reich's termination of the team work with Dr. Löwenbach and Dr. Hoffmann. Later Drs. Löwenbach and Hoffmann witnessed the experiment which measured the arousal curve during masturbation. (Experimental results S.VI, table 21). Neither Dr. Hoffmann nor Dr. Löwenbach saw an increase in the potential and the uniform intervals of the curves, which proved to be typical formations in later experiments. In a discussion in which Professor Schjelderup, Dr. Hoffmann, and Dr. Nic Hoel and also Dr. Wilhelm Reich were present, Dr. Löwenbach literally declared that the experiments were incorrect because even sexual pleasure must result in negative charge. Only two months prior, Dr. Löwenbach had enthusiastically reported to Dr. Reich that he had measured a strong positive charge on his own person during sexual arousal. These statements are indispensable with respect to the rumors buzzing around at present.

In October, 1936, after a satisfactory number of successful experiments to create bions were completed, Wilhelm Reich wanted to seek guidance from Albert Fischer, the Director of the Rockefeller Institute for Biology in Copenhagen, concerning the continuation of his research. On the occasion of a trip to Copenhagen, Reich demonstrated the bion experiment for Albert Fischer and his assistant. During the trial, Fischer asked Reich if he was mixing toothpaste. Reich then asked whether they wished for him to end the demonstration. Fischer apologized, and the demonstration continued. Reich requested a magnification adjustment between 2500-3000X. Albert Fischer's institute, however, only had microscopes without tilted binocular lens barrels and only magnifying from 1500-1700X respectively. Because of this, the demonstration was more complicated; however, the formations, at least to the practiced eye, were clearly observable. One of Albert Fischer's assistants immediately carried out a Giemsa staining, which had positive results. The same assistant made suggestions regarding the measures necessary for performing for the scientific community. Also attending this demonstration were Dr. Leunbach and Dr. Philipson from Copenhagen. After a while, Reich received a letter from Leunbach reporting that Fischer had made very derogatory and rude comments. He indicated that it was only his common courtesy that prevented him from immediately throwing Reich out. He had never before encountered such outrageousness. The formations seen in the demonstration were nothing more than simple lecithin formations, and, besides that, it was preposterous to demand such magnifications from him. Reich responded with a letter to Albert Fischer in which he very objectively stated his opinion of this type of control observation.

After the first autoclaved and highly sterile bion mixtures succeeded, Professor Thjötta from the Institute for Bacteriology in Oslo was sent some samples to evaluate. Professor Thjötta deemed these as highly irrelevant, dealing merely with simple subtili. Wilhelm Reich then submitted a request to expand the experiments to also include animal trials. It was a matter of fact that the structures he obtained must be tested for virulence. The request went to Oslo's Medical Director, Heitmann. Professor Thjötta was supposed to provide the expert opinion. Thjötta told Professor H. Schjelderup, through whom the request was processed, that the experimenters would be better off stopping their work because everything had already been researched in that area. Permission to conduct animal trials was later rejected by the medical director.

These encounters, just a few examples from many similar ones, should not only show how "scientifically" one approached an, after all, not unfamiliar work, but also point out that there is another way, a correct way of approaching the issue scientifically. Professor Roger DuTeil from the University of Nice was the first representative of this approach.

When Professor DuTeil received the first report about the sterilized bion mixture, he thoroughly examined the preparation together with a bacteriologist, verified the experimental design, and immediately sent a report to the French Academy. He wrote the following to Wilhelm Reich: The experiments were correct and interesting, however, such experiments had already been carried out earlier, though not in this manner, and, most importantly, were cultures from sterilized structures. At this time, the first cultures had already succeeded. Consequently, he immediately called together a commission following his presentation at the Natural Philosophical Society in Nice on March 7, 1937, composed of pharmacologists, bacteriologists, and hygienists under his leadership. This commission was tasked to perform control experiments parallel to those at the Sex-

economic Institute in Oslo and, after their completion, to report back to the French Academy. The first statements made by local bacteriologists are contained in Roger DuTeil's Nice presentation. Further inoculations using the forwarded cultures showed positive results, likewise the verification that the cultures were pure. The experimental design was also deemed correct. However, the work of creating bions and cultivating them, a job that was already difficult due to their nature, bogged down. In the summer of 1937, DuTeil made the decision to travel to Oslo in order to spend 14 days directly on the spot where he could verify for himself the working procedures, their results, and the possibility of completing control experiments in France. He spared neither money nor time in order to achieve the possibility of complete control. For ten days, DuTeil worked in the laboratory at the Oslo Institute together with all scientific personnel completing a trial test, which was completely successful including the cultures from sterilized bions. In the meantime, a commission of French scientific specialists, among them a leading professor of surgery, were waiting for DuTeil in Paris, who was supposed to bring with him the bions and the cultures produced in his presence in addition to a film showing the work process. DuTeil left on August 7, 1937, following very prolific work, with about 600 m of film and a collection of preparations, which had been produced in his presence.

This is the productive, correct yet discerning and friendly sort of control work. We do not doubt for one second that we have found an objective, critical, yet at the same time productive, helping friend in Roger DuTeil." Signed Wilhelm Reich]

Reich to Eva Reich, Saturday 7 August (*Beyond* p. 109-10) "Barely an hour ago, DuTeil left for Paris, taking with him 700 meters of film, many preparations and cultures of animalcula, and a large microscope that I used here. In Paris, a commission of scientists from various disciplines is awaiting him. On Monday the ninth, he will give a detailed report of my work and present the films. These last two weeks of working with him have been the happiest period of my life. DuTeil is a biologist and a professor of natural philosophy at the University of Nice. Since February he has been performing my experiments and has partially confirmed them. Now, working here, he has learned precisely how to do the experiments, so that he can function in France in coordination with me. Evchen, this is a very big thing. My most important assumptions on the origin of life, which I discussed with you and Lotte Liebeck that time in Sletten, have now been irrefutably confirmed by experiment.

I should tell you that the experiments are very closely linked with my work on sick people. This is because the cornerstones of life, namely currents and electrical charges, are disrupted in modern people, and this makes them neurotic. I have confirmed by experiment Freud's thesis that the sexual drives are the vital drives, and from this I have derived the solution to a basic scientific problem that has existed for hundreds of years. We have been working like the devil here. Our heads are all buzzing after these two very important weeks. We had to do everything very precisely because many people who will not look into the microscope will say that it is all crazy. And for this reason, DuTeil's visit was extremely important..."

Sat. 7 Aug. 37, (bion lab ntbk 2, ms. p. 193) "Bionkultur-Versuche erster Reihe [vuer?] abgeschlossen
Ich gehe jetzt doch über, Versuche zur Feststellung der Maj ersheiter
Veranderlicher Arten ersts höheres Organisation vorzunehmen."

Sun 8 Aug., S. Hoel to Siersted (Sigurd Hoel papers, National Library of Norway, brevs 355***) "DuTeil was fine [free?]."

Weds 11 Aug., DuTeil telegram to Reich (corresp box 10, DuTeil flds) "Commission vunderbahr. brief folgt. Herzlichs = DuTeil." Commission went wonderfully. Letter follows. Heartfelt greetings. RDT.

DuTeil report concerning a lecture on "The Bions" delivered in Paris on Monday 9 Aug. 1937 (correspondence box 9, fld "English translations", taken mostly from long DuTeil letter to Reich of 19 Aug., box 10, DuTeil flds, from which the untranslated French passages come) "...There were, in the audience, ... [eleven people] some of the most representative men of official French science. For example Drs. Allendy and Raoul Charles Monod; Dr. Bonnet, Director of the Laboratory of the Academy of Medicine; Drs. Symbi and Vincent, both highly esteemed biologists; Dr. Barbe, author of well known biological works; Mr. Nordberg, consul general of Finland and an international journalist; and, finally, some high officials, among them the physicist Bourdet, a member of the government.

The meeting began in an atmosphere of diverse attitudes; sympathy on the part of those already acquainted with the work; interested curiosity on the part of those who had not yet heard of it; occasionally also skepticism as, e.g., on the part of the physicist who offered the argument of the Brownian movement and tried to prove at all cost that the proteins were the carriers of unknown and naturally-invisible germs. Dr. Bonnet, whose opinion was by far the most important, showed the understandable surprise of a laboratory man who daily carries out successful sterilizations. Nevertheless, he demonstrated great interest and seemed determined to approach the matter scientifically and to acknowledge the relevant arguments. In brief, the atmosphere was highly favorable and was made more so by my position in the C.E.P.H. (Center for the Study of Human Problems) where I am well known, and also by the microscope which immediately forced one to take seriously the owner of such a magnificent instrument. [et aussi par le microscope qui obligeait aussitôt à prendre au sérieux le possesseur d'un aussi magnifique appareil.]

[Tandis que l'on remontait à l'endroit les morceaux du film qu'il avait fallu couper] While they rewound to the place where it had been necessary to cut the film, I showed the microscopic preparation to the Drs. Monod and Bonnet. The chief of the Laboratory of the Academy showed the highest surprise at the variety of these organisms and the differences in size among them. He [p.2 Reich German transl] admitted 'never to have seen such a thing.' Incidentally, in some of the preparations which, according to the labels, had been autoclaved, movement was still visible.

The projection of film 6, preceded by the film of the laboratory, took place in a most friendly and attentive atmosphere. The different reflections or objections made by the biologists present were refuted by the film itself as it played. One of them who had [French orig p. 4] asked whether the bions formed into segments exclaimed loudly when he saw for himself near the end of the film a bion which shows very clearly a median wall, an indication of segmentation taking place. The culturing part particularly interested all the members present.

Stemming from the film, there was a general conversation from which it seems—as well by what I heard about it myself as by what the members of the C.E.P.H., whom I know reported to me—that the impression produced was very deep. In any case, the concrete result is the following: the laboratory of the Academy of Medicine expressed the desire to perform, from the time of the October rentrée, a series of verification experiments, with or without my presence. Reflecting on this, I think that my presence is necessary, and I expect to organize these experiments myself at the laboratory of the Academy of Medicine in the course of the month of October. On the other hand, I should show the films and give a talk to the General Assembly of the C.E.P.H., which will take place 1-3 October at Pontigny, and Dr. Monod has expressed the desire to be present again for that. I think then I shall take advantage of being near Paris to return to Paris with him and to perform these experiments at this time. I could schedule the lecture at the Sorbonne, which Dr. Allendy asked me to do, with projection of films for the Society of Philosophical and Scientific Studies, a lecture which he promised me all the biologists and professors from the Sorbonne and the Museum would attend. It think it would be hard to obtain a more concrete result on the first try.

The next evening I may give a talk on the state of the question before a pedagogical congress. The next evening, Aug. 20, in Dieulefit, in the Drôme, where my mother lives, I'll run film 6 on the village curate's machine (!), he himself being very interested, for the two doctors, one of whom is a friend of mine [p. 5] and is a specialist in tuberculosis. For a long time, he has maintained by himself that the BK is endogenous and not exogenous. Our works confirm his view, which is moreover the same as Tissot's, and he agrees to collaborate with us from the clinical side."

Thurs 12 Aug., handwritten letter DuTeil to Reich (*)** Seems mostly to recount the same Aug. meeting in Paris that D describes in his long 19 Aug. letter to Reich (see above). [E. Sellers trans.: Dear Friend, Still too tired after 3 days, 900 kilometers, and two film presentations, to write you at length; I telegraphed you. Here are the first details:

In Paris, in spite of great difficulties (the projection apparatus would not take large reels and I had to cut the film into six pieces), I showed the [films of Prep.] 6c and of the laboratory. There were eleven members, including Dr. Allendy who went to see you at Oslo, [?] as in France by his works, Dr. Barbe.

Professor Monod from the Academy of Surgery, who had brought with him,

Dr. Bonnet, head of the Laboratory of the Academy of Medicine,

Dr. Symbi, biologist, known for his work on nutrition,

Dr. Vincent, biologist. Dr. Nordberg, Consul General of Finland at Paris, and journalist; Mr. Janvy [?], attaché at the Consul's residence, etc. dr. Martiny who could not come, wrote a letter from which I am copying a passage. Present also: M. Bourdet, a physicist.

The atmosphere was favorable from the beginning. I had also set up the [large Reichert] microscope, which produced an impression all by itself. Everyone was filled with admiration for such an instrument, and Dr. Bonnet, head of the Laboratory of the Academy of Medicine, worked with it all evening and did not want to stop! [This tends to strongly confirm Reich's claim that the highest quality microscopes really did make a very big difference in what one could see, compared to the standard laboratory microscopes most were used to using.]

1) In the end, Dr. Monod, who did not hide his interest, told me: 'We are very troubled. [sic] I ask you from the time of the rentree in October to send me all that is necessary. The Laboratory of the Academy of Medicine is going to follow methodical procedures on the bions and their cultures under my and Dr. Bonnet's direction.' I add that [illeg] a sympathetic state of mind...[illeg] a support...[illeg] we have two Academies all at once.

2) On the other hand, Dr. Allendy asked me to officially present the film at the Sorbonne in October or November (dates to be arranged myself) to the Group of Philosophical and Scientific Societies which he directs, and he promised me that all the great French biologists of the Museum and the Pasteur Institute would be there, including Tissot, who has already stated that the BK [tuberculosis bacilli] are endogenous and not exogenous.

3) Dr. Martiny, who is on vacation, writes: 'Give M. DuTeil my great esteem for his person and his research. The laboratory of the Leopold Bellan Hospital is at his complete disposal.' And he adds: 'I have just occupied myself with him

regarding personalities from Nice. I want another small...[illeg] on him to transmit to my friend from W[?] (This is on the subject of the post that they award me for my research).’

Another ms letter from DuTeil, date illeg., between 12 Aug and 13 Sept 37, or could be a continuation of the 12 Aug letter: [E. Sellers trans: Doctor Ichaelmann having returned to Brussels excused himself but he put his laboratory at my disposal.

The [only?] objection came from the young dr. Boudet, who ‘made your little Kroll and launched the Brownian movement.’ In the end, he had to surrender to the evidence. Another object is eggwhite, which [illeg] to hot and gray. I [illeg] the vehicle to germs. I replied, 5 the film also.

Wednesday I showed the film 6b at Dieulefit at my house to two of my friends, one of whom Dr. Preance also maintains that the tubercular BK is the product of tuberculosis and not its cause. He was passionate about this, by clinical [evidence?]. He [illeg] much about tuberculosis (it is the crisis of Dieulefit). He showed the film on his own projector!!! He was very interested and convinced.

There you are, dear friends, the first thing that I could do in so few days. I think that it will please you. I knew that with such marvelous weapons [in my hands], your success would be likely. I will write you from Nice on the typewriter with many details. I am now in the country...seated on a corner of my bed.

So thank you again for your total and affectionate welcome. Thank you to Mrs. Elsa Reich for her great kindness and all her attentions. Thanks to all the coworkers and your friends and to you for everything.. RDT]

Fri 13 Aug. 37, (bion lab ntbk 3, ms pp. 18-19***)

thurs 19 Aug. 37, “Auszüge aus den Briefen von Prof. DuTeil” of 10-19 Aug. 37, reporting on the reaction to his presentation in Paris. (corresp box 10, DuTeil flds, 7 pp, German transl 3 pp***pp. 2-3) “...I arrived in Paris at 5:00 and was very tired from the heat. Because it was Monday, I was not able to obtain a transformer for the microscope lamp. Nevertheless, I installed the device hoping for the best. That was well done because it was admired by the doctors and biologists present, who had never before seen anything like it, and, thus, a very advantageous atmosphere was established from the very beginning.

On the other hand, I had greatest possible difficulties with the film. I had announced that the film is 400 m long. Unfortunately the available device was only set up for very small rolls under 100 m, and it was too late to acquire a different one. So we had to cut the main film, No. 6-in which I had inserted the title that arrived at 4 o’clock that afternoon via air mail-into segments. After this time loss, there was no possible way to show film no.8, so I rescheduled it for a later meeting. Those present-eleven people, 6 doctors of medicine and [5] biologists- represented the most prestigious group of official academic French scientists. Assembled together were: Dr. Allendy, author of a book about Paracelsus on the influence of authority-I believe you know him, for he told me that he had seen you in Oslo; as well as Dr. Raoul Ch. Monod, member of the Surgical Academy. [remainder of letter already found elsewhere]

[remainder of this letter already in French and English, above: see DuTeil report concerning a lecture on “The Bions” delivered in Paris on 9 Aug. 1937 (correspondence box 9, fld “English translations”, taken mostly from long DuTeil letter to Reich of 19 Aug., box 10, DuTeil flds, from which the untranslated French passages come)]

Thurs 19 Aug. 37, Reich to DuTeil, “Your news about the success in Paris pleased us very, very much. We wish you much luck in further endeavors!

In the meantime, as you will be glad to know, I succeeded in finding the non-contradictable explanation for Brownian movement in soot particles. I will send you a short description of this very easy to complete experiment as an enclosure. Prerequisite is that one has a good egg medium. I can send you one if you need it.

Although I know I can depend extremely well on your tact and confidence, I feel I must, however, ask you again to be very careful with remarks concerning details and conditions of the experiments. I have completely detailed the reason for this here: I am dependent on the goodwill of the authorities and monetary supporters, and I have to wrestle with tremendous difficulties. Difficulties which the director of an official government institute in no way encounters.

The biggest difficulty now is the impossibility of direct contact to the physically ill. I have no idea how to carry out further experiments that will involve therapeutic outcomes. But I will get through somehow.

I would like to share with you once more how extraordinarily thrilled we all were not only to have you here, but also with the attitude with which you approached both us and this unusual undertaking. It was not empty words when I emphasized on that last evening that we know we have won a true friend in you.

Please keep us in the loop concerning the work, and if you should have any problems, we want to help you in any way we can. We would like to know which impression the film made in Nice and especially which objections were raised.”

Fri 20 Aug., *Dagbladet* article on DuTeil and bion expts (corrsp box 10, DuTeil flds, German transl)

["Does life form under as-yet unknown conditions? In Dr. Reich's laboratory in Oslo, three experiments raise a furor--French Professor visits Oslo for the purpose of collaboration and control. [note that, contra Nilsen Trotsky thesis, *Dagbladet* has not yet turned against Reich]

The renowned analyst and biologist, Dr. Wilhelm Reich, who has lived in Oslo for many years where he treats patients in character analysis, has been occupied for some time with certain biological experiments that are surrounded by great secrecy. They concern the development of life under as yet unknown conditions, so-called bion experiments.

The purpose of Prof. Du Teil's visit was to personally participate in Dr. Reich's experiments performed here in this laboratory. For several months, Prof. Du Teil has performed control experiments on the results of Dr. Reich's biological tests. During his visit in Oslo, he has completed several experimental sequences and could compare his control methods with those of Dr. Reich.

In particular, he had orders to submit a report to the Biologic Commission, which was appointed by the 'Center for Studies of Human Problems' at a meeting on Aug. 9, relating the results of the experiments that he and Dr. Reich had completed. The commission consists of bacteriologist, biologists, and surgeons from the University of Paris. According to reports we received, the commission showed great interest concerning the experiments that were made in Oslo. In France, laboratories and means have been put at the professor's disposal, so that further experiments can be completed. Likewise, he should submit a report on Sept. 18 addressing further natural philosophic consequences of the Oslo experimentations.

From his visit here, Professor DuTeil took a series of extremely interesting microfilms, which, prior to his departure, were previewed by an informed group that included the French ambassador, M. Rene Ristelhueber.]

Mon 23 August, Reich diary entry (*Beyond* p. 110) "Soot, ash, and charcoal can come alive! I have succeeded in that!

The great fame in store for me will cause my total isolation. People already feel honored to be in my company, but it is also a burden for them. I am an unwelcome guest in my own house. I disturb people's peace of mind. They're happy when I'm away for a couple of days. I have become an authority, so to speak. This has been an 'oppressive' effect—people want things to be uncomplicated, comfortable, carefree. They don't want their enjoyment of life disturbed by 'heavy demands.'

Two days ago a notice on both DuTeil's visit and the bion experiments appeared in the *Arbejderbladet* and the *Dagbladet*.

Power increases; it's tempting to misuse it."

Tues 24 Aug., Reich to DuTeil (corresp box 10, DuTeil flds, 3 pp***) [Your letter from Nice on August 19 hit us like a gigantic bombshell; however, in another sense than the one usually associated with this word in today's world. What you achieved in your first attempt is absolutely colossal. I would now like to answer the questions individually:

1. It is now no longer difficult to counter the Brownian movement argument. The experiment with incandescent soot, charcoal, ash, etc., the steps of which I described to you in my last letter to you, is irrefutable. The experiment was at an opportune time, and is very simple to complete without the many preparations and complications of, for example, preparation 6. Now it was revealed that the bion experiments work even with insufficient medium because it cannot apply a high enough temperature and is dependent on the unpredictability of the mixture's success, the autoclaving, and the inoculation. All the same it [Prep. 6] remains one of the major experiments, but the winner in every sense is the incandescent soot experiment. Since your departure, I have completed it roughly eight times in a row and have not had even one cultivation error. Macroscopically, the same type of blue-gray microbe grows again and again. I have demonstrated this several times for friends and on the side. It will ease our overall work very much.

2. The control experiments that Dr. Monod wishes to complete in his laboratory will help us immensely; however, I am asking myself whether it would be beneficial to complete these experiments without your presence. An eventual malfunction of these complicated experiments could easily result in more harm than good. And you, yourself, were of the opinion that you should lead these control experiments, so you would then have the possibility to point out the many small details that must be observed. Please inform me if and when you completed or will have completed the incandescent soot control experiment in conjunction with the others. I am very interested in how it went. If you need egg medium, we can very easily create some here and send them to you.

3. It is such a joy to get so many at one after a decade of isolated work. I mean primarily the many demonstrations which you are requested to perform [bottom line cut off].

4. Concerning the media, we only allowed them to publish a very general notification about your visit according to the report that you left here. Dr. Havrevold has been inundated with journalists; however, we continue to refer to the upcoming

publication again and again. I am working on it full steam ahead and hope we can publish all of the works together by the end of October. I would like to request two pieces of information from you:

First if you would be willing to eventually change your lecture 'Life and Matter' just a bit before we have the works typeset. Second, if you had anything against me publishing part of our letter exchange, as long as it would not be tactically dangerous. I will send you a list of the letters shortly that I wish to either publish in their entirety or parts thereof, provided that they are factually significant, and ask you to please write me whether you agree with this.

5. After I had sent my last letter with the sequence for the incandescent soot experiments, I had the feeling that I had unnecessarily commented on things that are in the best of hands with you. But I also beg of you to understand that I have been taught to be careful based on many, many bad experiences. I am referring to the sentence where I asked you to be very careful when communicating with strangers, for example the following: I am earnestly on the right track regarding sarcoma disease. I was able to grow a bacillus from sarcoma tissue that killed four inoculated mice within 24 hours on the first attempt. At the same time, I could determine that this bacillus was killed by the packet amoeba. Based on these observations, I am going forward full speed ahead with animal trials and will keep you continuously informed. I feel certain (which says nothing as far as science is concerned), that the matter will be successful. I am only extremely worried how to achieve direct contact with infection cases in order to produce cultures from serum and blood to determine the identity of the structures and to begin therapeutic treatments.

I must publically admit that I was fully shaken by the personal part of your letter. I have no yet experienced such a complete understanding of the total issue in such a relatively short period of time. It is not merely a matter of mood on your part, which has been proven not only through your character, which I came to know so well here, but also through the caution with which you approach the consequences of the issues occurring currently beyond the laboratory work. You are completely correct: Only the truth can uniquely and exclusively determine the claims and world view. And if you were able to prove that one or the other of my sociological viewpoints is false in one way or another, I would have no qualms about publically correcting them. In addition: You are also right concerning the question of the "finished product," that someone else completed a century ago. However, I differentiate here between the theories which were based on the facts. Without dialectic materialism barely a crumb of our present work would have been achieved. At the same time, I wish to stress that I see neither the spirit nor the methods of the man to whom we both allude represented by his followers today. Quite the opposite, rarely in the history of scientific inquiries have a method and a scientific opinion been so discredited as right here and now. One of my most important goals today is to separate myself from this desecration and have nothing to do with it. I wish to summarize my fundamental philosophy: I am embarrassed and sickened to eat a richly covered piece of bread if I know that in my closest neighborhood or somewhere in the world millions of people are dying of hunger, not because it has to be, but rather because our world is governed by mad men and politicians. I believe that we, as scientific laborers, have a far greater and more important task to complete than we were previously aware of. Perhaps a very large piece of the tragedy in which our world threatens to fall right now is due to the fact that exact laboratory-and social life have become all too separated. I feel certain that we will be totally in agreement concerning these issues, provided that we leave one another time for reflection and factual persuasions.

I can declare myself totally in agreement with your completion of the task which you took upon yourself; move over, I am even thankful that you have relieved me of the task of trying to carry out the issue against all kinds of protests in addition to my primary job.

I kindly ask that you request whatever you need from us in order to simplify your work over there, and I need not assure you that it would be our great pleasure to assist you.]

Weds 25 Aug., ("Research Notebook 1937-38" in OI box 8) "S-Versuche" the next several pp document the progress of S-injection expts on mice.

Thurs 26 Aug., DuTeil to Reich (corresp box 10, DuTeil flds, German transl 1 p.***) "I received the egg-white shipment and wish to thank you: it will serve me well because I am just now creating an entire culture series. My office in 'Center' changes more into a laboratory each day. I have been working for several days on classification to see which bions are aerobic and which are anaerobic. I believe it is of the utmost significance in regards to the cultures. I will keep you up-to-date about this.

I repeated the incandescent coal trial and it is a complete success. I had just thought of trying it with ashes myself when I discovered in your letter that you already thought of it. Our mutual inspiration was a joy.

At present I am busy drying and dying preparations of all of our productions in order to display them. Everything is completely classified [categorized??-not sure which word is the best here].

I believe that the concern you outlined in your letter is now completely resolved by my correspondence that crossed with yours. The methods I use in my approach in strict scientific stages, for myself as well as in the disclosure of our work to others, make it possible to chase away this fear. The development of the question proceeds precisely and methodically, and there can be no question of either concessions or mutilations. I will tell you again: You have reached a stage where your entire synthesis shines forth from each of your sentences, and you quite remind me of Nietzsche's conceptions. I must, however, in order to bring current scientists to recognize the pure scientific basis of the synthesis itself, first focus on only the purely

scientific issue. Naturally the total problem of the sympathetic system, the complete orgasm-question, and the problem of the tension-charge-formula as a whole, which by itself makes the role played by K[potassium] and C [carbon? Calcium?], from lecithin and cholesterol in the creation of bions understandable, are contained in this. But the type of presentation must be strictly geared towards making the appearance of Life comprehensible. If these issues have social repercussions, then they will appear automatically in their own time.

I am expecting a price quote for a small group of equipment consisting of: autoclave, drying kiln, and reservoir for distilled water. Before that is complete, I cannot begin serious work. I have been promised a design in which everything is compacted together, and I hope that it will not be too expensive.

I hope to have good news from you soon. Please greet our friends for me; for you are correct: it is a true friend who departed from you on the 7th. I am very sincerely your Roger DuTeil.”

Sat 28 August, Vatican effectively recognizes Franco as leader of Spain.

Mon 30 Aug. and Tues 31 Aug., DuTeil to Reich (Box 10, DuTeil flds, German transl pp 1-3***) “I have received the books, the egg-white, and the translations of the newspaper articles, thank you very much. Please, however, send me if possible further glasses with culture media--bouillon, agar, and egg-white-- because I now have begun culture experiments on a large scale and am in danger of running out of medium because I am not equipped to create them myself.

At the same time, I would like to ask you to reveal the pH of your culture mediums, especially for bouillon. I do not have the equipment to do the measurements myself, and in Paris I would like to provide as many exact details about the bion cultures as possible.

I have now also obtained a culture from incandescent coal, potassium chloride, and bouillon that grew yesterday on agar and is gray like yours, perhaps not bluish but with violet reflections. It is certainly the same thing. Under a microscope: slightly egg-shaped organisms.

Inform yourself about the tumor called ‘chimney-sweep cancer,’ which attacks the genitals of chimney-sweeps. It could very well be the case that they develop due to the soot or carbon microbes that we are obtaining and cultivating. As far as sarcoma is concerned, I do not completely understand this part of your letter, because the sarcoma-or cancer microbe is not only unknown, but its existence is even disputed by most of the bacteriologists. One knows of the spirochete [screw bacteria] (or a spirillum), which is present in cancer tumors ((spirochete pseudopallida)), but which has never been considered a carcinogen. It could be possible that your bacilli are a type of spirochete and appear likewise in sarcoma tumors without being their cause. However it may be, we are dealing with an extremely interesting investigation, and I am glad to see that you are pursuing it.

In the same context ((dans le même ordre d’idées)): I am afraid that it could cause confusion if one uses the expression “amoeba” or “amoeboid” for the ordered or unordered cocci-packages that we have seen and show in the film. It is a fact that unordered packages are not pliable like amoebas, rather stiff and at times angular. They quite resemble staphylococci-clusters-with the difference that the staphylococci are immobile, whereas these clusters show motion. The ordered packages, on the other hand, which you call packet-amoeba, are very well-known in bacteriology under the names ‘tetrad’ ((Micrococcus tetragenus)) and ‘sarcine.’ The tetrads characterize the tetragenus bacteria especially well, which, by the way, is the cause of many infections-i.e. lung gangrene. One even knows of barely mobile tetra genus (Tetragenus mobilis ventriculi ((Sarcina ventriculi))). Clearly nothing can change the fact that we have seen them develop from our mixtures; but if one wants to categorize them, it appears to me preferable to use the established classifications and notations.

As great as a relief that the soot experiments provided us, I still believe that the announcement of this discovery would be very dangerous if the bion experiment were no longer mentioned: the bion experiment forms the first stage of the investigations and is indispensable for the explanation of the second stage-the formation of spores-which includes the coal and soot experiments. On the other hand, these last ones are too simple, and, when presented directly, do not give the impression that they could be integrated into an over-all theory. For this reason, I prefer to begin at the beginning: to explain the clinical roles of CaCl₂ und KCl and of cholesterol and lecithin in order to show that the bion mixture is not a product of chance. We live in a century when specialization has made it possible for some people to gain an extremely extensive and deep understanding of specific issues.

They could not-from the beginning, admit that some advances had been made from time to time in these areas without going through all the stages of which they are aware. The bion experiment has the advantage that it goes beyond present biology in that it is still securely anchored in it-which is not the case with the soot and coal experiments. For this reason, I will next introduce the former and reserve the right to show the latter only to those who have understood and accepted the former.

You will understand why, after I accepted the responsibility of announcing the issue concerned to the scientific community, I can only do this by considering the mental standpoint of the people with whom I must contend and then use the appropriate means. One must approach this very methodically. You have already passed through this stage and are playing with the consequences-I

carefully organize all of your fireworks in my drawer, I use them as needed, but I move forward step-by-step when the goal is to reach people.

This letter is already too long for me to now include viewpoints concerning social issues. I find it only important to tell you now that I am very much obliged to you for your absolute honesty concerning everything, and that it would be very surprising if we both touched by the same muse and one as the other determined to stick to the facts, did not reach the point of total understanding someday.

I recently sent you some photographs and ask you to distribute them to the addressees.

Please ask our friends to think of me with fond memories and believe that I am most sincere, RDT.

PS They are refusing to honor the return ticket from Stettin through Berlin to Strasburg both here and in Germany-perhaps you could ask Krölle or your friend, Philipson, in Copenhagen to try to sell them to someone returning through Stettin. They are good for another month. That would be the only way to get the money back. The cost was about 250 francs, or roughly 40 kroner.

Please be so good as to remember my shaver.

Follow-up letter, Aug. 31, 1937 "Dear friend, I am adding a postscript to this letter:

1) I received the shaver, thank you. Now I will no longer be 'grating' your nerves with it.

2) I succeeded with a series of extremely interesting and meaningful experiments:

a) I did an incandescent coal-KCl-experiment on August 19 and had, because there was no bouillon at hand, added a drop of melted blood-agar as nutrient. I, thus, obtained very nice 'gelatin.' Using this gelatin, I inoculated: on the 28th, bouillon-growth; on the 29th, bouillon onto agar-this inoculation produced growth and resulted in a grayish culture with bluish images like the one you have. I was neither completely satisfied with nor sure of the purity of my culture, since I had opened the tube several times to extract microscopic samples.

b) I have, therefore, repeated the experiment on Aug. 27 under absolute unambiguous conditions: a tube with clear bouillon, with about 2 ccm KCl (boiling), and a pinch of incandescent coal. The bouillon presented with colloidal cloudiness and opalescent shimmers. On the 30th, three days later, I simultaneously seeded bouillon and blood agar. Both resulted in growths, and the blood-agar produced the same grayish-blue culture.

The following is very important: when I discovered both of these successful cultures this morning, 24 hours later-and at room temperature-I saw at the same time that the original experiment mixture from the 27th (KCl-bouillon-coal) had subtili colonies on the surface-whereas none of the cultures started from the tube showed any subtili.

From this I draw two conclusions:

a) The *B. subtili* that had contaminated this tube had only contaminated this tube during the procedure.

b) The blood-agar and bouillon cultures with the grayish-blue color are pure cultures.

c) The similarity between both of the grayish-blue and creamy cultures on agar proves that one is dealing with microorganisms won through [created by] the experiments.

Microscopic testing of the dissolved KCl only resulted in insignificant microbes. For greater reliability, I have started a culture from them. I hope that these results, which completely verify yours, will make you happy. In any case, they will show you that I work very methodically and that the comparison between our activities can be very fruitful.

I received a letter from Dr. Monod today in which I was informed that Dr. Bonnet, Laboratory Director of the Medical Academy, will make his laboratory available to me so that I can, as of Oct. 1, personally complete the experiments there. So after my report in Pontigny, I will go to Paris from Oct. 4-10, as I had suggested, to personally complete the bion experiments and the others, if that is possible in the presence of this gentleman. I believe that will please you.

Piece by piece I am building a private laboratory here that is absolutely essential for the preparation of the main experiment in October and will be busy with this all September. I will only demonstrate what I have successfully completed myself ten times and have repeated using my own materials.

So please send me, in several packages via airmail, as many tubes containing fully prepared culture mediums as possible, especially bouillon and agar. I am purchasing small supplies as I go [p.4]; I already have several hundred francs worth of them. Concerning major supplies, I am waiting for the sterilization equipment: this matter is still pending; I am still awaiting the cost estimate from them. My assistant is much better now; she comes each day to help me with the work.]

Weds 1 Sept., Reich to DuTeil (corresp box 10, DuTeil flds) "I send you my heartiest congratulations to your future autoclave and dry sterilizer. How does our microscope like being with you? I made the suggestion to Reichert, Vienna, that they leave us the microscope as a demonstration model so that we can save 2,000 crowns. They will surely earn a lot of money with it and will now have interest in working with us.

In the meantime, the question of the egg-white culture medium has been clarified. With time, some of the previously very sterile culture mediums produce spontaneous growth that is not exogenic, rather endogenous. It is recognizable by its reddish color and hard consistency. The structures are ones specific to the 6th mixture, in particular large amoeba.

There is nothing else to report for the time being with the exception that I am having very, very much difficulty filming the Org-animalcules development in slow-motion and have still not found the technical solution for this.

I am completely, one-hundred-percent in agreement with your way of communicating and working with the scientific fundamentals. Many thanks, too, for the beautiful pictures, which I have passed on. I ask you to please report any interesting new developments to me.”

Reich diary entry (*Beyond* p. 110) “Preliminary work on cancer. It is clear that the transformation of grass into protozoa is the model for the formation of cancer. Therefore, prepare grass for a film.”

DuTeil to Reich, Sat. 4 Sept. 37 (correspondence box 10, DuTeil flds, Reich German transl****) “I am answering several questions you put to me:

1) I will be delighted to use the Reichert z-microscope for eventual customers sent to me for demonstrations. By the way, I have not passed by an opportunity to tell anyone who might be interested what a good opinion I have of it and to please pass that on to others. [see catalog for Reichert Z Mikroskop]

2) I would not consider it disadvantageous to eventually publish our correspondence under the condition that nothing concerning third persons would be included that could result in disagreements. It would be best if you would send me the proofs prior to publication; so I can verify that there is nothing in there that could anger others.

3) I am very lucky to have that microscope because it has allowed me to clear up many unforeseen problems. I work with it on a daily basis.

Now quickly some news: I received a letter from Baron Seillere in which he verified that the announcement to the Academy of Moral Sciences will take place on September 18, and added that he would also share with them the latest update I sent him concerning our experiment.

You can, thus, get everything ready for the publication of the report. What shall we do about the French edition of these booklets?

As far as I remember, you should take care of that in Oslo, and I had left you a similar report and its binding as a model. Please notify me immediately if we will so proceed and, in that case, send me the corrections as soon as possible. It is better when I have the cover made here, and, as soon as I have received the text, I will also take care of having it bound. I will send you, then, the required number of covers for the examples that you wish to retain to distribute yourself. Tell me, too, how many of the French copies you intend to take. I can distribute about 300 copies.

My lecture on the Sorbonne can first take place after vacation. Dr. Allendy will allow me to choose between the 18th and the 25th of November. I will decide in a few days.”

Tues 7 Sept., Reich to DuTeil (corresp box 10, DuTeil flds) [Many thanks for your friendly note. I will now address the factual items. Our laboratory will expedite to you today: 20 each of bouillon-agar-and egg-white culture mediums. The bouillon pH is 7.4. I am asking you to please determine whether or not the culture that you got from the incandescent coal turns out to consist of the smallest, finest amoeboid vesicular clusters when viewed at a 3750-magnification. That was at least the case with the blue-gray soot and ash growths. There is a misunderstanding concerning the sarcomas: I did not write that I have found the sarcoma, or cancer microbes. I was able to grow cultures from sterile cancer and sarcoma metastases, which proved to be pure cultures. In addition I have obtained a pure culture of microbes from two sterile sarcoma-metastases, which are of the same type, and which are completely different from the cancer cultures. I am very wary about any claim that would suggest a carcinogen. It is also contrary to my hypothesis concerning cancer; however, the fact remains that the sarcoma bacilli came from two sterile sarcoma preparations. Now I would like to share some happy news with you. We were able to prove that the package amoebas in the blood of the mice keep several days. We could obtain cultures from the blood of inoculated mice.

As to the question of the legitimacy of referring to amoebas and amoeboid, the following: the terminology I choose are based on terms from formal bacteriology, although they necessarily have a totally different connotation. Two examples: the cocci that we grew from autoclaved lung tissue were diagnosed as white staphylococci by bacteriologists here, but the diagnosis was based solely on the form, while our diagnosis especially included the electrical reaction. It is possible for the form to be the same, yet the electrical reaction different. Our package amoebas can not be identical with the tetragenus bacteria [Sarcina], first because they have totally lost their original shape due to being inoculated onto egg medium, and present as extremely mobile contractiles instead of square structures at a 3000X magnification; second, because they do not cause any pathological phenomenon in organisms.

I would also like to state that one should attempt to correlate obtained structures to other already familiar forms of bacteria, but one should not force the issue because there are too many factors that extremely change the image in bacteria diagnostics.

Concerning primary importance of the bion experiments themselves, you are totally and absolutely correct. Although the soot experiment is striking, it is much too simple for demonstrating the overall problem. Otherwise, as I already mentioned, I am very, very much in agreement with the program that you have proposed for yourself.

The experiments that you describe in your postscript are very clear and gratifying. Only I cannot, emotionally, warm up to the subtili question for the time being. When a film appears, it must not necessarily indicate subtili, for it could also be that the bacteria consolidated into a membrane. Besides that, we had agreed here that fundamentally the subtili-bacilli themselves cannot be anything else than some of the forms we obtained through artificial means.

I understand very well that you will proceed along a different track than I. I only wish to add this: both tracks are branches of one railway line, which will, at a later point in time, unite to form a single railroad track. I believe this analogy characterizes my full understanding of our two positions that exist out of necessity.

I have succeeded here with the soot experiment eleven times with only a single failure.

I don't know whether you are aware how much your work frees us here, for it makes it possible for us to completely concentrate on animal trials and therapy issues.

I am sending you a very successful picture in the enclosure, which will certainly please you.

We have sent your return ticket, Stettin to Strasburg, onwards to Copenhagen for processing.

So, good luck, dear friend, for we here are also working very diligently and meticulously ahead.

PS I just heard from Reichert, Vienna, that they accepted my suggestion to make the microscope there available to you at no cost as a demonstration model. I will save 2000 crowns this way, and you have usage of the microscope from Reichert at no cost. You see, the world is not always as bad as one thinks.]

9 Sept., Reich entry in bion lab notebook 3 ("Basic Research III" OI box 7, ms pp. 34-42, ***9 pp) describes S-bacilli "Es gibt wieder ...1) Paket-gespritzt...Blut Pakett ...2) S-bakterien mit ein wenig...[top p. 37] Die S-bakterien sind elektrisch indifferent..." ms p. 38 "Ein Knopf!!!!!" Here Reich is beginning to guess at the role of excess carbon in blood bion expts, by p. 40, CO₂, p. 41 cancer.

Thurs 9 Sept., Reich to DuTeil (box 10, DuTeil flds) "1. I have already written that Reichert agreed you may retain the microscope you have at your disposal as a demonstration piece at no cost. I hope you were as happy about this as was I.

2. In the question of publishing our correspondence, I propose the following: I will review the letters once more in detail and then first decide whether we want to do it at all. In any case, I want to publish the most important of those letters between us that contain factual discussion and details about the bion experiments.

We are extremely happy about the many presentations and demonstrations you will be performing. I only have one major concern: whether three days are really enough, for example at Bonnes, to give a sufficient overview of the many different facts so essential to understanding if one wishes to actually understand the bion experiments and not just make a one-time or even repeated cultivation failure grounds for total dependence [total rejection]. {*Anlehnung = dependence, Ablehnung = rejection*} I ask you not to forget the important fact that only the bions from preparation 6, which have a strong negative charge, result in cultures. I do not inoculate neutral bions at all anymore because it results in an unnecessary statistical change for the worse.

I would like to ask you to send us very detailed reports about all of the demonstrations and presentations you have given as well as the reactions to them for the big report that we are now beginning to compile for printing. I believe that you will agree with me that it is very wise to announce the initial reactions of the scientific world in the first publication and will have nothing against it.

I can hardly comment concerning your suggestion to simplify the bion experiments in the manner you described because I have never done the experiment that way and cannot guarantee that it will be successful. I do, though, believe that it will work. I would like to point out one thing, though. When one sterilizes the second mixture, first autoclaving and then combining, there will be no more lecithin tubes, and it is then questionable whether the large, pretty 'amoeba' will grow on the egg-medium.

I am expecting your shipment containing the subtili that grew on the gray blood-agar culture. My gut feeling is to reject the analysis. I have observed here several times that the surface of a creamy-type growth begins to change after several weeks. Examining the different layers under a microscope, one sees formations that are different. It looks like a mixed infection, but I must inevitably bear in mind that forms change under circumstances that are not yet understood. And the formations, which you described as 'real sausages,' I saw in absolutely pure cultures a long time ago.

Now concerning printing the brochures:

1. Please write me as soon as possible whether you wish to have the complete report published in French or only your work 'Life and Matter.' There are advantages to the first [suggestion]. From the complete report, one could, eventually, if you agree, publish only my detailed announcement and your theoretical work in French. We will have 500 copies printed here, and that in the format *Experimentellen Berichte über die elektrische Funktion von Sexualität und Angst* [Experimental Reports about the Electrical Function of Sexuality and Anxiety]. Please answer this last question as soon as possible to avoid a delay.

The following from my work: I find myself in a blind alley again and cannot get out, but I know, based on certain signs, that in short a piece of the puzzle will fall into place. I don't know whether I have already written that we were able to grow package amoeba from the blood of inoculated mice. Strangely enough, blood drawn under sterile conditions from untreated mice also produced agar growths.

I have a favor to ask of you: it is completely impossible to reference all of the bacteriological subject matter. I will have to send structures that we got here and are questionable to bacteriologists for diagnosis, although I can only accept that diagnosis with reserve. I can send you some cultures to have diagnosed by bacteriologists there. We will have the same cultures analyzed here, too. Most of all, I want to know whether the green, growing, pure-culture of short, small rods which are electrically neutral and came from sterile sarcoma fluid, is really identical to the *Staphylokokken pyozeanus* [now *Streptococcus pyogenes?*].

We are very happy here with all of your reports..."

Mohr to Dunn, Thurs 9 Sept. 37 (Dunn papers, APS, Mohr 37-9 file) Describes *fantastic* summer weather in Oslo, "Great sun every day, almost too much of it." "Faussen who just returned from a month's stay in Berlin was perfectly horrified at the wild and barbarous spirit down there. It was much worse than anything he had expected...The end result has been much worse than I could dream of. You should hear the Nürnberger [Nazi Party Congress] addresses on the radio, it is simply unbelievable!" M has spoken to a British neurologist interested in genetics who just returned from Moscow. He told that it had been impossible to talk to Gershenson, Levit, and others, without an "interpreter" being present all the time. "Suspicion everywhere, and all conversations started with cliché declarations about the Marxist science being the only science, etc. etc." Venke is back from staying with Dunns the summer in USA

Thurs 9 Sept., (bion lab ntbk 3, ms pp. 35-37***)

10 Sept., (bion lab ntbk 3, ms pp. 34, 37-42***)

Sat 11 Sept. 37, DuTeil to Reich, [" I am replying to your letter from the 7th, which I received yesterday and which made me very happy. I am thrilled about your explanation, which not only fully satisfied me, but in addition was also very interesting. The question of terminology, i.e. the word 'amoeboid,' has a specific meaning to me. It is correct that when we use an expression that usually identifies a specific thing in present day bacteriology and apply it to something with completely different characteristics, we are provoking additional objections. I am just now working on this in order to possibly eliminate dumb objections, so that only the intelligent-useful-and, yes, even indispensable objections remain. Objections that target words waste time that is all. That was the exact purpose of the objections I made to you-which were, by the way, just as dumb.

I would also like to present my viewpoint concerning the *B. subtilis* question. We cannot deny that there are a large number of microorganisms in the atmosphere. We, ourselves, have collected and cultivated such. And, furthermore, we cannot hope to guarantee that never, ever did one of these microbes fall into our bouillon during an experiment. Now, with the impossibility of completely preventing germs in the atmosphere from contaminating our experiments, I believe it would be better for us to stand together and all agree that here and there a germ could be present. For this purpose, it is vital that we understand how to recognize and identify them. It is the same thing with the *B. subtilis*, which are very common and at the same time very easy to recognize. Take note that when I determine *B. subtilis* in a culture over the initial, authentic, pure culture, I thus verify the purity of last one made.

After this, the case that I reported to you is absolutely clear. The moment I use my strain from Aug. 27, it is already three days old-in the meantime, we have the contamination. I inoculate simultaneously onto bouillon and blood agar. The next morning, I find a small film on my strain that teems with long, sausage-shaped rods, whereas the rest of the culture only contains cocci. Only cocci in the bouillon-the agar, after it had shown a creamy, gray cocci culture the morning following the inoculation, is suddenly infested with a dry culture, which is ripped like a wilted leaf characteristic to *subtilis* and which covers the other thoroughly aerobic culture. The next morning there were rods in the strain as well as on the blood agar, while the bouillon, which had been inoculated at the same moment, presented only with cocci, no pellicules. These circumstances make it evident that the new culture, which shows up on the strain and on the agar, has been caused by an event that does not occur on the bouillon. I have found this justification: The bouillon is initially inoculated using a pipette that was thoroughly flamed with alcohol, while I dipped a platinum wire that was probably insufficiently heated to incandescence in the strain to inoculate the agar, which then infected the strain and subsequently the blood agar. In the case we are dealing with now, there can be no doubt left about the exogenic origin of the culture that suddenly covered the first one. But clearly understand: the fact that *subtilis* were identified in two of the three tubes, guarantees the different and probably endogenic origin of the pure cocci cultures now found in the bouillon and which were also present on the strain the first day.

It is a scientific fact that those who seldom give us a chance are usually very confident concerning the reason behind any phenomenon. And it appears to me that when I wish to convince biologists, I should prefer to take a stand for the *subtilis* infection, which the other researchers will oppose without fail without me being capable of explaining in one way or another why, after three days, a sudden change took place in two test tubes but not in the third one, whereby especially the third one had not been treated under the same instrumental and experimental conditions as the other two (pipette and platinum wire). By the way, *subtilis* is very distinguishable under a microscope, and I am sending you the tube of blood agar upon which it developed in order to further simplify its macroscopic and microscopic identification. I would like to ask you to return this culture to me because I specifically use it very often to prove that, when airborne germs coincidentally do appear, I understand how to identify them and distinguish between them.

As you can see, these efforts to eliminate useless objections prompted me to design a device and have it built, a photo of which I will send you in two to three days. With the help of this apparatus, the complete bion production and seeding onto bouillon a few days later will take place in a closed system impenetrable by germs and which only needs to be touched and operated externally because the transfer of liquids from one container to the next occurs either through gravity or through expansion from heating air or from steam contained in the device. Yesterday I re-inoculated bions that were created using this method on the 6th after autoclaving the necessary materials for three consecutive days in 24-hour intervals at 130°. My impression this morning is that the bouillon is already cloudy. I had a tube in the form of an h constructed [see marginal diagram, bion experiments, p.] to achieve the same result in order to inoculate in a closed system without external intrusions, and so I could, at the same time, preserve a pure bouillon sample in addition to an inoculated one.

I would like to request that you immediately report back regarding the experiment with the soot heated to incandescence and let me know whether this soot was from a wood or a charcoal fire. It is very important for the interpretation of a series of experiments that I am currently completing with KCl and that I will relate to you in a few weeks after all results are verified.

It pleases me to think that my current work removes a load from your shoulders by allowing you to concentrate on immediate, important concerns. I thank you for the shipment of bouillon and agar tubes, which I hope to receive today. In the meantime, I could produce thirty of them with the help of the municipal laboratory and sterilize them in my autoclave. In addition to myself, the laboratory now employs two half-time helpers, one for the actual lab work (sterilization, etc. and to watch over the drying kiln that I could obtain but which has no thermostat control)-the other for writing the daily protocols, technically clean copies of the experiments, and for correspondence and the microscopic preparations. In conjunction with this letter, I am sending the tubes mentioned above as well as an equivalent tube of pure culture for comparison purposes and a gram-staining of the pure culture that I personally completed yesterday. After using them to re-inoculate, I would like to ask you to return both tubes to me as they are useful for my demonstrations here.

I have enjoyed hearing the news about Reichert that you shared with me very much. You have a great idea there, and it would make me personally very happy to include the Reichert Company in any connections that may result from my position and the distribution of our work. I do not miss an opportunity to announce and ascertain my good opinion of this wonderful device [p.3]. I have had a small transformer constructed for the lamp, which functions very well. Ask all of our friends and Frau Reich to remember me fondly...”]

13 Sept., Reich to DuTeil (box 10, DuTeil flds) “It already shines on the horizon of pathology. A mouse that was inoculated with extremely deadly bacteria remained alive after an injection with bions as opposed to other mice. All of them were dead as a doornail. [literally: as dead as a mouse]. In short, I write you in detail about this highly gratifying matter. We are repeating this experiment until total certainty.

I am sending you in the same mailing a tube with very deadly bacteria, fatal for mice. Please be very careful with it. Tell no one this is dealing with the S-bacteria, which I got from the fluids in a sterile liver sarcoma-metastasis. I would like to have the bacteria identified by a bacteriologist anyway. He should not know where they originated and what was done with them. I am having Dr. Havrevold do the same thing here. It should be *Staphylococcus pyocaneus*.

Please tell me the assessment as soon as possible. Once more, very, very careful.
We will be typesetting your French textual work shortly.”

Monday 13 Sept., DuTeil to Reich (re Sy-Clos arrangement, French original plus German transl***) I respond with urgency to your last letter: 1) I am sending you via airmail today the first bions produced

a) by the procedure of ‘sterilization of the mixtures made,’ b) with the closed apparatus which I imagined, designed and had built [see diagram in margin of letter]. I shall send you shortly the photo of this apparatus. It consists of two glass vessels communicating with each other via a stopcock. The two mixtures are placed, one in each of the two vessels with the stopcock closed. The whole thing is placed in the autoclave three times, three days in succession, at 24 hour intervals, thus adding Tyndall’s [fractional sterilization] procedure to the usual procedure, necessary in order to kill the *B. subtilis*. Then, I say, three autoclavations at 130° for half an hour, one of which for three quarters of an hour. The tube containing the bouillon is also cooked in the autoclave three times, for it is also joined to the apparatus by a sealed tube and a rubber stopper. The culture was made after five days. The stopcock separating the two vessels was, of course, opened up and the final mixture effectuated after the third autoclavation.

The culture in bouillon succeeded and was already subcultured yesterday on bouillon and on agar with success.

I called these bions Sy-Clos bions (which means made in a closed system), and I called the preparation:

Prep. 6D-T, indicating that this is my new method of sterilization of the mixtures before the final mixture, which avoids the 'after' sterilization.

I joined to the sealed tube one gram of these bions. The culture on agar that I will send you is bluish gray, the same as the others. The gram shows very clearly the same ovoid and round shape. There were particularly beautiful amoeboid contractile formations and also rings of segmented lecithin.

I think that this news and this shipment will please you. I am starting up the experiment again. I insist on the fact that all the operations after as during the sterilization are in a closed system including the culture, made without touching anything by dilating the air contained in the apparatus by heating it. So there is no longer any chance possible, with this system, of an external infection, any more than thanks to the three sterilizations of an infection which had existed in the ingredients.

2) I wanted to speak to you about my paper, to the Academy only, at least for the time being. It seems to me essential to publish it from now on 'apart' [as a reprint?] in order to benefit from the authority of the Academy. The format should be the same as the other paper (blue) that I left you as a model, and I will have a cover similar in color and design made here so that it will resemble the editions that the Academy itself makes.

Concerning the other papers [communications], it seems very useful to me to wait until I have the final text, at the moment in press, of the paper from Pontigny at Pentecost and also the text—a very detailed one—which I am going to present at Pontigny on 2 Oct. and at the Sorbonne on 18 and 25 Nov. The last text was very technical and scientific, where I summarized your 'Urgegensatz' giving the essentials of your synthesis, and is much more interesting than the previous papers. Thirty pages sum up the whole question as well as previous works, the Kraus theory, ... etc.

Toward the middle of the week I will send you a note on my present experiments, which are written down day by day, in particular three experiments on blood charcoal and the bions experiment. The experiments with the KCl, although very noteworthy, still require several more days of confirmation. I think that they will bring us an immense simplification, in a sense much more than soot.

Thank you for the beautiful photo, which as you guessed caused me great joy. I recognized therein all, or almost all our friends. I am putting it on my desk so it is always in my sight and to encourage me in my work.

Again about the printed paper: In my opinion this communication to the Academy addresses a special category of scholar, who is interested more in matters of pure thought than in science. It should contribute to rendering favorable the general atmosphere of the intellectual world in France and in Europe. That's why it's important to publish it separately right now to prepare the others. But you might save the paper and present it again then, with all the others, when we have the Academy of Medicine, which will not, I think, be long in coming.

For biological comparisons, I'm taking charge of this and will make myself helpful in this way. They are, you understand, essential in preparing the practical consequences of our works. I sent you yesterday for comparison two tubes of our culture of *B. subtilis*. In this case, I consider my explanation of infection simpler the other, since it can link the appearance in two of the tubes to a known cause that has not acted on the third. Moreover, if the appearance was spontaneous it would have taken place on the second bouillon. And I deem it more 'diplomatic' anyway, to make allowances for infection to avoid the objections that would otherwise minimize all the rest of our work.

On the other hand, the transformations of microbes, their polymorphism [pleomorphism] are known phenomena, which I accept completely, and I have examples in my gray cultures themselves where the organisms appear sometimes round, sometimes ovoid, and often become round again when freshly subcultured. But the macroscopic aspect scarcely ever changes.

In any case, I prefer in regard to the bacteriologists to admit the exogeneity of the rod bacteria [*B. subtilis*], to authenticate at the same time the endogeneity of the cocci, which is unquestionable. There will be time later to verify if they [the rods] also can arise spontaneously. RDT]

Tues 14 Sept., DuTeil to Reich (box 10, DuTeil flds, original in German (no French letter) "My assistant is not here today, so I can write you in German!

[Strick trans.: 1. Cultivation of the first bions, Prep. 6 D-T was successful. I have already made the third inoculation on bouillon and on agar. Only once so far, have I attempted a control inoculation for the second time, when *B. subtilis* suddenly grew from my strain. Naturally, I had three or four times opened my tube, for inoculations and for samples to observe under the microscope. In two or three days I will send you agar and bouillon [cultures] for comparison with what you yourself will grow up [in culture] with [from] my bions—sent yesterday. I'm moving along towards it.

2. Enclosed you'll find photos of the two that I designed and constructed myself. In the "h-tube" I can conduct totally sterile inoculations, without doing a thing. In the "Sy-Clo" I have succeeded after three autoclavations (three days apart) in the first cultivation in a totally closed system. Notice, please, in the gram stained preps the beautiful and here 'really' blue amoebae.

At 3250x magnification in the living preparation, one sees the contractile movements very clearly. The structures seem to me to have no membrane. They come one after another without difficulty. I must tell you it [settled down in the ampules for a long, slow time?], and that today I saw in the tubes some rods that looked suspicious to me [i.e., that might be *B. subtilis*?]. But all round and uniform structures are very certain and true bions. All growths are blue-grey and show the same basic form that I've always gotten here using KCl.

3. This morning I received the culture medium. It was just in time; I had no more in store. Yesterday I attempted nine inoculations. All went very well. Only with KCl have I found special difficulties, about which I'll soon tell you, when I become certain of the outcome of some ten experiments.

4. I have translated about half (the second half) of your "Urgegensatz [des vegetativen Lebens]." Soon I'll send you the French text. I've found therein the basic argument to use in my reports in Pontigny (October) and in Paris (November). I analyzed this work, with your synthesis, before I had clarified for myself the researches. It will be very clear.

5. Now I have obtained an autoclave and an incubator. In two or three days, I'm getting a small dry-heat sterilizer. So I hope you will forgive that I've not yet paid back any of the money you lent me, since I thought it more important to first buy this needed equipment, in order to carry out the experiments properly. Tell me if this was correct. Since my trip home, I have already laid out [bezahlen] 4 to 500 Kroner. You know yourself in such a case how quickly it [money] goes and [how the amount] increases! Until now it was impossible to do the work correctly in my Nice laboratory. I must be alone and quiet. I work about three hours daily, sometimes four or five. And thus I have already helped you clarify [things].

It's late, and I must get back to work.]

Weds 15 Sept., Reich diary entry (*Beyond* p. 110) "Mice diseased with tumor bacteria do not die when injected with bions.

Informed all coworkers at the laboratory this evening that we're after the cancer plague."

c. 15 Sept., Reich contacts Kreyberg b/c of his desire to pursue the cancer problem. Kreyberg "entered the controversy when Dr. Reich asked the Radium Hospital, which is the principal cancer hospital in Oslo, if it would furnish him with samples of the blood of patients suffering from cancer for use in his experiments.

Thurs 16 Sep., Reich to DuTeil (corresp box 10, DuTeil flds) I am writing you today concerning the culture that I have, in the meantime, thoroughly examined. As soon as I have more time, I will address the questions you sent me in your last two letters in detail. I have included three test tubes in this mailing. Two of them are the ones that you sent me, while the third originates from a series of four incandescent soot inoculations. You can observe, both macroscopically and microscopically, the exact same growth as the one appearing in your experiments. This growth appears in all four of my culture mediums that were made on the same day, and contains cocci that definitely originated from the soot and rod-shaped organisms that one does not normally find in soot cultures. I am requesting that you carefully re-examine the two culture mediums that I am returning because I found cocci as well as the *bacillus subtilis* rods you diagnosed in both of them.

The report of this culture, of which you have a sample, is as follows: I completed a total of fourteen incandescent soot experiments, three showing no results, and seven producing a bluish-gray growth like the sample I sent to you. Another time I made four incandescent soot specimens, all at once. Instead of the bluish-gray growth, each one of them produced a brownish growth, which was partially hardened. I wish to emphasize that all four of the samples that I made on the same day contained rods. I cannot say for certain, but I believe that the rods are endogenous.

For the time being, do not let this prevent you from asserting the subtilis nature of the rods when discussing the trial results. That could, after all, be refuted at some point. Please report to me if the rods did not show very rapid movements when they were inoculated directly with airborne germs. The rods in your preparation showed almost no motion. There is no difference between the rods in your preparation and those in mine. Comparing the homogenous cocci, your rods show no structure, rather resemble small sausage links. Mine, on the other hand, show either a strepto-arrangement of vesicles or are cudgel formed when viewed at a 3000 magnification. I obtained similar rods when I autoclaved moss, as I once demonstrated to you.

I consider the question of airborne infection extremely important, not so much from a practical standpoint, rather from a theoretical one. Recall that we experimented with eggs in order to determine whether the putrefactive bacteria in rotten eggs were a result of the self-accelerating egg white decomposition or a result of airborne bacteria. Yesterday we opened the first egg that had been coated with tincture of bronze. It was completely rotten. We made a culture of it and will send you a sample. Based on my experiences, the decomposition must be endogenous. This does not, however, totally exclude the possibility that air contributes to the decomposition of eggs. That's it for today.

I am happy to report that we now have a fourth mouse that survived a serious infection after being treated with bions. I will send you a detailed report when the experiment is completed. The fact that you were able to produce bion cultures is wonderful and will certainly make a good impression on your colleagues. I cannot locate your French manuscript titled "Of Life

and Matter.” Do you, perhaps, have an extra copy that you could send me as soon as possible? It will immediately be sent to the printer. I also cannot find the envelopes that you claim to have left here. Please send duplicates to the publisher if you have them. Your brochures can be printed within ten days following the first day of typesetting.]

Sat. 18 Sept., DuTeil presents “Life and Matter” at Academie des Sciences Morales; release of ZPPS issue with “D-M in der Lebensforschung” v. 4 (3): 137-148 [**Reich sources 2 bdl**]; Reich to DuTeil (box 10, DuTeil flds) “I would like to answer your last two letters from Sept. 11 and 14th. I hope that you have received my incandescent soot cultures in the meantime and can see for yourself that the macroscopic growths and the microscopic structures are the same as you got. Even the rods are in there. The bions that you sent me were unfortunately ruined, for the ampoules were broken. I cannot do a thing with them. I have not yet received dyed specimens. I would be very appreciative if you would send me your mixtures once again. It is possible that different structures form when different procedures are used.

I have analyzed all of my bion cultures for an eventual second growth over the first one, which you consider to be an infection. An infection must occur within 24 or 48 hours. In my case, several bion cultures that are a couple of weeks old are now showing the first signs of a differently shaped growth on top of the original growth. There is absolutely no doubt that the structures change with time; however, I couldn't tell you today how that happens or what it means.

And now some good news:

- 1.) several mice that I inoculated with bions after they were severely infected with S remained alive.
- 2.) Mice inoculated with weak S developed tumors. This last fact is of unheard-of importance for the entire project.

I beg you once again to have the S-bacteria in the tube I sent you identified by a local bacteriologist as quickly as possible. But I urgently implore you to say nothing about the purpose of this entire issue.

This coming Monday I am presenting the films to a local cancer researcher. I am very curious what he will say to all of this. He will probably only react with awe and mistrust. I am doing this in order to get direct access to cancer stricken patients. I believe that we are further than we think.

Concerning your letter from Sept. 14, I wish to thank you for the pictures. If it is alright with you, we will publish your device together with our devices here, but as constructed by you. I also ask you to send a description of your device and its operating instructions as soon as possible. The entire work goes to the typesetter today and your report to the French Academy in French is already typeset and will be available in about one week. You will get it afterwards.

I thank you very much for translating the “Ancient Contradictions.” Did you omit the first half of it? I am afraid that the second half will not be completely understandable then.

Regarding the money that you owe me, please do not worry about it. You can wait to pay me back when it is easier to do so.

All of us here are thrilled over the immense friendly and objective enthusiasm you are developing, and I can only wish you quite a number of discoveries. But you will write us about important observations, right? I will give you a detailed report as soon as I have completed a large series of experiments with the S-bacteria and the remedy against them to the point of certainty. I might then ask you to put me in contact with the Belgian cancer researcher. The issue astounds me and is almost unbelievable.”

Mon 20 Sept. 37 DuTeil to Reich (box 10, DuTeil flds) “Just a word to say that the paper to the Academy of Moral and Political Sciences was read Saturday and that several press articles have already appeared, which I include in this letter. [**sheet of articles included in letter**] I am waiting for the printed text, but don't have any covers made, as I will make them here so they conform to the Academy's style.

I shall write you at greater length tomorrow...

PS The only journal that published a very long and complete article on my talk (*The Journal of Debates*) unfortunately overlooked a misprint, omitting the twelfth line, in which I think they indicated that the elements were purely chemical and strictly sterilized. This makes it hard to understand the remainder of the article. I am writing today to the *Journal of Debates* for a correction.

I did not receive the tubes you told me about the other day (sarkom metastasis). In fact I have an offer on hand to identify the bacilli. Send them to me as soon as possible.”]

Tues 21 Sept., DuTeil to Reich (box 10, DuTeil flds, German transl 3pp*** French original, mentioned in Reich's reply, most of this translated as pp181-192 of *The Bion Expts*) My dear Friend, First of all, I thank you for your telegram. It made me very happy to hear about this unexpected result for which I congratulate you. Likewise, I received the S-tube, and I thank you for it. I shall see if I can experiment here myself; in any case I shall without saying anything have a bacteriologist from Nice make an identification. I have had a new apparatus built also in closed system, composed of four communicating tubes [Bion Expts p. 187] and which I call for this reason “Syclo-tube.” On my first try I obtained a very clear culture in 48 hours. And I'll add

that I had made bions without milk and that I replaced the red color of the gelatin with methylene blue dye (the gelatin I had was colorless). This preparation, called 6DTB, has quite the appearance of absinth and was very strongly turbid [troublé] with bouillon after several hours of incubation.

Meanwhile, I made a second preparation of bions in my first apparatus and I obtained a culture in three days. My next culturing will take place after 24 hours and the following one, I hope, right after making the mixture. It would be perfect if I could do the entire operation and obtain the result in a limited number of hours, which would allow me to more easily convince the observers.

The famous French bacteriologist Dr. Colin has just asked me for a documentation. I intend to go see him where he is now in Nancy, and right afterwards in Paris, i.e., early in October. I also received a visit from the biologist Vincent and from his father-in-law Marie who is one of the most well-known French chemists. They stayed two hours and returned convinced. The affair is making considerable progress at this time.

Finally, my lecture at the Sorbonne is planned for 18 November at 9 o'clock in the evening, and around 1000 people will be attending. They are already asking me for a resumé, for a new scientific review to be issued in January by *La Nouvelle Revue Française* (N.R.F.), which is one of the top literary magazines in France.

I'll reply shortly to Lotte Liebeck to tell her that the Dr. Allendy whom she's going to meet in Copenhagen is really the same one whom I met in Paris and who knows you. It is he who is organizing my lecture at the Sorbonne. This is to tell you that he is a true friend and Lotte would certainly be well advised to meet with him during the Congress.

Moreover, Hanson asks me for a documentation on the French circles who are interested in the affair, saying that it is for a young biologist from Copenhagen who is himself working on the affair. Please tell me if I may or may not send this information.

The press continues to speak about my paper. More would have been written, had it not been for the death of President Mazaryk as the meeting went on, and for a paper from Mr. Flandin, former Minister, delivered the same day, which absorbed the press's attention and took up the space in their columns. Anyway, the most important point is that they spoke about it, that the date was taken in this way, and as I told you one day, any danger for the affair in France be henceforth "ausgeschlossen."

It is now necessary, my dear friend, for me to speak to you about a very important matter I did not tell you about earlier because I wanted to be sure about my experiments first. I am now going to objectively explain to you the series of three experiments; I'll save my interpretation of them for later.

1) 19 August: I was making a mixture of KCl and charbon au rouge [blood charcoal] (heated to incandescence) with a drop of sterile blood agar that I melted, lacking bouillon. (This blood agar, or at least that which remains in the tube, is still sterile as on the day of the experiment. 28 August, possibly ten days later, I inoculated a bouillon culture which took on the 29th, and agar which also took—the usual gray culture.

The same day, on the 29th, I twice reheated the first tube at 100 C for half an hour, then I reinoculated a bouillon, which took. On 3 September I autoclaved the first tube again (in the autoclave at 134 C) and reinoculated a bouillon, which took, and on 4 Sept. an agar, which also took.

On 5 September I autoclaved once more at 130 C and reinoculated a bouillon, which took, and on 6 Sept. an agar, which also took. This is a first experiment which should not amaze you but which acquires considerable importance by repetition.

Since there was only charbon au rouge (incandescent blood charcoal) whose sterility cannot be doubted, and the bouillon whose limpidity indicated its sterility, it is certain that the KCl solution was probably not sterile in the beginning. It is why on 3 August I undertook a series of experiments on the pure KCl, about which I will now tell you.

[note in upper corner of page:] Please, if you redo the KCl, telegraph me the result.

ATTENTION: I am calling for your serious attention. I should add before I start, that I now have an autoclave, an oven for sterilizing to 190 C, and an incubator.

2) On 30 August, I took a solution of KCl made with pure KCl from Poulenc and water distilled by my pharmacist by boiling three times to 100 C. I inoculated this KCl the next day with a bouillon that was turbid and had taken. On 2 Sept. (three days later) this bouillon produced for me a very strong culture on agar. Fearing that this experiment only meant that my KCl was unsterile, on 3 Sept. I placed the KCl in a dropper bottle stoppered with filtered cotton in the autoclave at 130 C for three-quarters of an hour, and I inoculated it immediately on bouillon, which took.

On 5 Sept. I again placed my KCl in the autoclave, placing it in a dropper tube that was sterilized itself placed on the tube of bouillon. The inoculation was obtained by the pressure of the steam inside the autoclave. This bouillon of 5 Sept. took in turn and gave me again on the 8th on agar a very beautiful gray culture, though this culture indeed took longer to grow and was clearer than those preceding it.

On 8 Sept. I autoclaved the KCl from the same bottle for the third time, and I reinoculated it on agar directly. It took four days afterwards, producing a grayish culture for me, but whiter still and very long in setting. A reinoculation of this culture on another agar made on 11 Sept. took in turn and produced a very beautiful whitish gray culture.

Then wanting to have it clear in my mind, I constructed the two h-tubes whose design I sent you. The KCl was placed (already sterilized four times) in the right branch of the apparatus, the bouillon in the curved branch. The entire thing, stoppered with rubber, was placed in the autoclave, making a fifth sterilization for the KCl. Coming out of the autoclave, the tilting of the apparatus sends the bouillon over the KCl and keeps a little of the bouillon in as a sample in the curved branch. I made two experiments with the h-tubes, 8 Sept. and 9 Sept., the KCl of the 9th having in this way been autoclaved one more time than that of 8 Sept. Now, these two experiments succeeded and although the bouillon inoculated by the KCl only presents a barely perceptible turbidity, the two reinoculations on agar produced cultures teeming with cocci. I sent you one of these, gram stained, without telling you that we were dealing with pure KCl and which you could correct on the label.

I maintain then that it is an established fact that pure KCl sterilized 5 and 6 times produces living cultures directly in the bouillon that reproduce themselves, even when the mixture, which is at the same time the inoculant, is made coming out of the autoclave and in a system that is rigorously closed. I should, nevertheless, draw your attention to the fact that the organisms obtained absolutely have the color and the transparency of the medium in which and on which they find themselves. This explains why the bouillon may appear perfectly clear and only reveal an imperceptible turbidity in comparison with a sample placed in rigorously identical conditions. As for the agar, it only presents very small, colorless and transparent granulations, visible by their refringences and which require much attention to find. You yourself observed with me such fine, scarcely visible granulations when we were experimenting on charcoal and soot.

It is then perfectly explainable why you never found this property of the KCl when we were making the control inoculations in the course of preparing the bions. The slight turbidity in fact only appears often at the end of three or four days, and it seems to me that it appears more easily at room temperature, around 23 C [25 C?] than at the temperature of the incubator.

There exist two possible interpretations of these experiments and of their results: 1) The Pasteurian interpretation would say that KCl contains microbes resistant to all sterilization, a phenomenon that it would be necessary to explain. Or 2) the interpretation I propose is that KCl has the property either to awaken or revive life amongst elements having formerly constituted living matter. More simply, and in line with Reich, I would say that the bouillon naturally contains, in natural amounts, the indispensable elements we already combine to make a bion preparation, i.e., lecithin-cholesterin, potassium-calcium; but especially calcium. The supplementary introduction of potassium in the mixture produces the same effect as its introduction in the mixture of the bions.

If one opposed the results you obtained with incandescent soot in the bouillon alone to this interpretation, I would reply that the soot coming from wood fires certainly contains potassium and that your experiment only confirms mine, or the inverse.

Such is the result, my dear friend, toward which my recent experiments have led me—led me with an absolutely rigorous method, complete material means and a critical spirit, constructive, it is true, and constantly alert. Perhaps it won't astonish you over much since one can and one must interpret it in the sense of your theory. I ask you then to inform me as a matter of top priority of your feelings on this subject. For the time comes when I must explicate the whole question before the French scientists, and it seems to me that this new fact, if it is confirmed, throws light on the whole question which it is impossible to hide.

You will doubtless have an interest in re-doing the experiment yourself, which is very easy, and *even if you see nothing on the bouillon in cultivating on agar again*, then using the microscope. I forgot to tell you that, moreover, I redid the experiment three more times with some KCl crystals heated red hot and melted, and I obtained the same results. RDT

Tues 21 Sept. 37, Kreyberg at Reich's... Kreyberg looked through the microscope and was taken aback. 'I would like to see your broth,' he said, implying that he believed it to be a contaminated solution. This astonished me because he had seen the clear solution himself. Furthermore, it is impossible to confuse a coal bion with any known particulate matter obtained from the air. Still, I yielded to his demands and put a drop of the solution under the microscope at the same magnification. Naturally there was nothing to be seen. Kreyberg walked away obviously shaken. Previously he had asked me for a coal-bion culture to examine at home. I hesitated slightly, knowing he would have no idea of what to do with it. I did, however, give him a sample of the culture [3e] grown on agar..." [compare with description in letter to DuTeil of 23 Sept. "took his breath away..."]

Weds 22 Sept. 37, *Aftenposten*, the main conservative paper, follows up 9/21 TT article with a warning about Brownian movement; *Tidens Tegn* carries Vistemann på Kampen cartoon [Øverland, S. Hoel in front row of worshippers, Wenesland right behind Hoel]; Reich diary entry describes a news article [from 21 Sept. *Tidens Tegn?*] on Mohr's objection to the bion expts.: "Mohr, a socialist genetic biologist" has scornful opinions "without having seen anything."

23 Sept., *Tidens Tegn* carries Reich's letter in response to its 9/21 article, asking that the papers contact him first before reporting on scientific research..

Reich to DuTeil, Thurs 23 Sept. (*Beyond*, p. 111-13) 2. I am enclosing the translation of some articles that recently appeared in the Scandinavian press, unfortunately somewhat prematurely, as if due to an oversight. I would like to give you some more details about this matter, because it could be important for planning our future tactics.

On Sunday the 18th of this month, our *Journal for Political Psychology and Sex Economy* printed the article "Dialectical Materialism in Biological Research." Barely had our friends received the issue when an extreme right-wing newspaper, acting with veiled malice, seized on the material and at first published a factual article on the subject. But at the same time the newspaper arranged for an inquiry by some highly reactionary university people who are not held in very high regard here by the scientific world. [He's mistaken here, at least about Mohr.] One of them is a physiologist and a fascist [Hansen], the other is a fascist and a genetic scientist. [Mohr? Using the term fascist broadly?] They have committed a major blunder because they have commented on a matter about which they cannot possibly have any knowledge at all. They have not even read the article to which the newspaper referred. Their actions created a very bad impression with everybody here, from scientists to laymen. Their actions helped us.

A highly personal issue between one of my patients and the author of this article is hidden behind this journalistic trick. For neurotic reasons and with a great fondness for work my patient developed the idea that it would be dangerous to be together with me because I am an intense individual and would captivate everyone's attention. As I just found out today, he has been bragging to his friends, amongst them the mentioned journalist, that even though Sigurd Hoel, Överland, and Havrevold were influenced by me, he was not. And what happened? He naturally swallowed the bait, because on the next day in the same newspaper there was a caricature showing me cooking at the stove and the four above-mentioned people, including him, were worshipping me. That's how these things come back to roost. We found out about it because together with factual items in the article that was published, issues were mentioned that only a person working in close proximity could know. Additionally, everyone believed that I had launched the article. The next day I published a short rectification and a reference to the detailed publication to follow. I have also specifically requested my friends to avoid any type of discussion with people who do not make the effort to properly review and follow the issue.

It is scary how in this case a burning interest for Life's problems was connected to lowest yellow journalism. But, we are in a very good position.

It's uncanny, how burning interest in the life-problem and low, sensationalist journals become allies [strange bedfellows] in this cause. But we're doing very well.]

3. Let me report a new experimental matter that I do not want to keep from you, because it provides enormous encouragement and confidence with regard to the earlier experiments. I recently telegraphed to inform you that the mice injected with the S-bacillus [earlier name for t-bacillus] either died after twenty-four hours or, if they did not die, after about ten days developed a very large infiltrating cancer tumor under the skin on their backs. The S-bacillus is thus in all probability one of the causative agents of cancer, or perhaps it is the only one. It is cultured from sarcoma liver metastasis. Now, the following thing happened. To carry out controls on the preparations, we injected my 3e incandescent soot bions, and after ten days the mice had developed tumors. I do not understand this at all. I am waiting for the medical pathology examination to be performed and also for further experiments of the same kind to be carried out. But if it is correct, then it solves a major mystery connected with cancer.

I would now like to share the following thoughts with you in confidence: 1. When tar is applied to the skin, it leads to the development of cancer within about six months.

2. Pipe smokers are prone to cancer of the lips.

3. Soot bions cause cancer.

Conclusion: When an excess of carbon is supplied to the organism, it is a vehicle of cancer. But how? Cancer pathology has long established that cancer tissue exhibits an asphyxiation metabolism—i.e., an excess of CO₂.

This fits in perfectly with the production of cancer by soot bions and tar and tobacco residues. [K must feel threatened, b/c his main work is on tar-painted mice. And he already had the 1932 episode where he thought his work there was being scooped or stolen by somebody else.] If this is confirmed, then the possibilities it opens up are immense. Because of these recent observations, I got in touch with a Dr. Lejv Kreyberg, a cancer researcher here in Oslo, and he visited me the day before yesterday. I showed him the Preparation 6 film, and he reacted very decently. I then showed him the incandescent-soot test, which completely took his breath away. But he is behaving objectively and has promised me he will identify the 3e bions in his department at the university, will supply us with blood taken from cancer patients, and will help us determine the mouse tumors obtained. This is a great deal of help and I think we will make rapid progress.

I would like to ask you to write me whether it would likewise be possible for us to have such provisions implemented in Nice or Paris, parallel to these examinations here. That would have the advantage of attracting interested researchers, that these researchers would be more cautious with an eventual refusal, and, third, that we would absolutely be playing it safe. I am wondering now if it would be practical to keep this scientific secret and if it wouldn't be better to initiate a broad campaign among researchers in a united front against cancer. Please consider this question and tell me your opinion.

I am surprised that you have not yet received the S-tube. In any cases, you will get a second one sent on. I am waiting anxiously for the results. The main point is, does one recognize these bacilli and does one know that they have something to do with cancer tissue?

Should the soot experiments occasionally not work out, let the egg-culture medium stand for a longer time. I have observed that on culture mediums that have not shown anything after 48 hours, delicate mounds of the same type as usual appeared after about 2-3 weeks.

I forgot to ask you to send us a short, clear synopsis over what is currently happening in France, especially comments from bacteriologists and biologists. One is already attacking you. There are groups that understand nothing about the issue and attempt to discredit you by attacking your competence as a biologist. Naturally, that is supposed to offend me and not you. In contrast, remarks made by Bonnet or similar people, which demand genuflection from cowards and the faint hearted, will be very beneficial.]

That's all for today. I could tell you much more, but I don't want to overload you. We are delighted every time we receive a letter from you. Please go on delighting us as often as you can."

23 Sept., Reich diary entry (*Beyond* p. 113) "Why do the 3e bions cause cancer?"

1. 3e are living organisms of pure carbon.
2. Cancer tissue has a developmental metabolism.
3. 3e produces excess of C which cannot be combusted.
4. Tar = C → cancer
pipe smoke = C → cancer
3e = C → cancer

Causes of cancer: a. poor respiration = lack of oxygen in tissues.

- b. Stimuli arising from excess of C promote local formation.

The inhaled O combines with bion C to form CO₂, which is exhaled.

When there is an excess of C or deficiency of O the tissues suffocate.

Cancer is a pathological attempt at regeneration."

Thurs 23 Sept., Kreyberg (from Radium Hospital) to Reich (correspondence box 9, "1936-39" flds) [H. Nilsen transl.:

I have carried out a stain of the culture from egg medium in a smear preparation that was made when I visited you the day before yesterday. The result is completely consistent with gram positive staphylococci, which I will not hide from you, look just like the result of common infection of the medium [from the air.]

25 Sept- 16 Oct., Blood drawn (?) from 10 of Reich's colleagues, to produce bions: Tabelle der 10 Bione (OI box 7, "Basic Research III" notebook, ms p. 59***), gives age of each, including Hoel, Lange, Berle, Gert. Brand, Gerda Ring, Mildred Schjelderup, etc. plus details about the bions produced. Lange has not yet withdrawn from Reich as Helge Waal says he did before Reich's departure from Norway.

Reich to DuTeil, 25 Sept. 37 (*Beyond*, p. 114-5, draft p. 229) "Today, just a few lines in reply to your letter of 21 Sept. 37. [The business with Colin, Vincent, and Marie is wonderful. I hope it continues that way.](#) Kreyberg, the cancer researcher here in Oslo to whom I showed the three cultures of soot heated to incandescence and also gave him a sample for Gram staining purposes, sent me a short letter in which he states that, on the basis of this exclusive staining experiment, he has diagnosed staphylococci. He also writes: 'I believe that contamination from the air must have taken place.' I do not understand how these colleagues are capable, first, of giving so little thought to the matter and, second, of assuming that I did not have the same idea myself long ago, at which time I established for sure that airborne contamination is not involved. In view of this experience, I have decided that from now on I will not casually reveal details to anybody, because it merely causes confusion, possibly creates enemies, and certainly does nothing to advance the cause. I believe the correct path to follow is to be willing, as before, to demonstrate the phenomena but to require that my colleagues either deal seriously and in detail with the matter or leave it entirely alone.

I am not in the least surprised concerning your first experiment with incandescent coal and KCl, because I had attempted some experiments to cultivate cooked and autoclaved cultures myself some months ago that were partially successful, partially unsuccessful. Then I left it alone. I am, naturally, very appreciative when you definitely clarify this question.

It was different with the KCl experiment. It seems, even to me, to be totally unbelievable, but I must agree with your interpretation. It is completely possible that the KCl has a stimulating influence on the organic substances in the bouillon as far as organization is concerned. I followed this interpretation completely subconsciously when I completed the control experiments as well as the bion preparation 6 trials in addition to all other culture experiments only with water or under dry conditions, thus

avoiding KCl. The swelling effect of KCl is without doubt the deciding factor. We will now complete the second experiment two or three times here and write you immediately as soon as we have definite results. From now on, I will also do the incandescent coal trials in that way, no longer directly in bouillon, rather first by putting the incandescent coal in KCl and then transferring it to bouillon. You have no idea how happy I am with the results you reported to me. You are really brilliant, likewise the idea of inoculating in a closed system. Now I have an important favor to ask of you: Could you have samples of the Cyclotubes made and then send them to us? It is easier than if we have all of it built again here. I will write you immediately as soon as further news is available.

And now another piece of good news: the cultures from cardiac blood, from tar mice that had tumors proved to be pure cultures of S-formations. It appears that carcinoma and sarcoma are not far removed from one another in regards to the S-bacilli. From now on, I am calling the bacilli grown from S that resulted in tumors SI, and the bacilli grown from the tar mice, which also produced tumors, SII.

I don't know what I should advise you concerning the inquiry from Copenhagen. You are dealing with the plant pathologist, Paul Neergaard, who saw the coal and bion experiments some months ago and offered to do experiments with plants. I sent him two cultures, however, he let months go by without replying. I find it curious that he should take this course. It doesn't look too good from his side. I think it would be best, as I also wrote to Hansen, if you contact Paul Hansen that Paul Neergaard should write to me. You understand this caution, my dear friend. Man's subconscious is very deep and peculiar/remarkable.]

29 Sept., DuTeil report to Reich on his control expts of 19 Aug.-28 Sept. (*Bion Expts.*, p. 179-92, corresp box 10, DuTeil flds, first 2 pp of German transl***, French original, I am replying successively to the various questions in your last two letters:

1) Concerning the press relating to my lecture at the Academy, and it is relatively important. One must not compare the newspapers of a city like Oslo or like Nice, which have little interesting material, with those of Paris where there are several million inhabitants and which is the capital of a very large country. There are many papers which the popular press does not even report. Moreover, it concerns the Academy of Moral Sciences, where the subjects treated are, for the most part, inaccessible to the general public, and the paper in question was in the metaphysical domain, which is very difficult and reserved for specialists.

Do you remember, moreover, that this paper especially had the purpose of setting a date to ask the question as established and stop any thought of plagiarism.

While on this subject, I am sending you a second clipping from the *Journal des Débats* which on 25 September corrected the false impression which had disfigured the article of the 18th. It made another mistake when talking about the University Centre as something being out of line; but that is in good shape since it conforms exactly with the terms of your reply to the Norwegian press, where you say that the works are officially conducted by the University Centre of Nice. You'll see, furthermore, that I took advantage of the occasion to already announce the 18 November lecture at the Sorbonne. On this side, everything is going well. (The article speaks erroneously about the University Center, but you know that the works were officially conducted by the University Center of Nice according to your reply to the Norwegian Press.

2) I think it was a mistake to have wanted to go too quickly to Oslo. It is this excessive haste in a very delicate matter, which needs to have a long development, which set off an incoherent press campaign whose coverage you are sending me. I think it is preferable to wait for the establishment of these works to be made officially in France; at this time all the journalists in Oslo will be quiet. I operated in the same way in France at Nice. I only spoke about the question with a few sane people or to scientists who had specifically come from elsewhere to see me. It is at Paris and Paris alone that I want to devote myself. After which, Nice will follow quite naturally, **without having to fear arguments similar to those which your personality incites at Oslo.**

3) Concerning, cancer, about which you ask my opinion, the case, although in a different way, presents itself with this same aggravation that the cancer problem has provoked and provokes still more numerous researches in various different directions, and that a too hasty popularization will produce a general outcry that must be avoided at all costs. Here again, I think that we must proceed very slowly. The problem is so delicate, and it would be an extraordinary event if a discovery were made at the same time as yours. Once your name is accepted as the inventor of the bions, we have, then, time to base the works that follow on this authority. If we again give too much at once to science, they won't take us seriously any longer. Therefore, I think that the best procedure is the following: at the time of my stay at Pontigny and at Paris, and once the results of the bions have been officially recorded and confirmed, I will have Drs. Monod and Bonnet introduce me to one or two doctors specializing in cancer, or at the Pierre Curie Institute, so as to be certain that the atmosphere of my revelation will be acceptable. There I will be certain, as I am with Bonnet, to be listened to with a truly impartial scientific attention. At this time, but only at this time, I will put these doctors directly in touch with you.

4) Concerning the scientific question of cancer and its relation with incandescent soot, I believe as you do, that there is more there than just a connection, but rather a very close relationship. I remind you that in an earlier letter I pointed out to you

that they knew of an epithelioma called “cancer of the chimney sweeps” which appears on the genitals. They take care of it by exstertion. You should find out about it from your colleague [Kreyberg?] and ask a surgeon for a sample. You could then compare it to what you have obtained.

5) I am sending you and enclosing the proofs of “Life and Matter,” which are very well printed and exactly of the same character as my preceding paper. Don’t make any cover. I will have them printed here, as similar as possible to the preceding one. Send me that in the mail and if possible in small packages so that the customs does not end up stopping them.

Concerning customs: I learned that they often opened and broke tubes at customs, keeping watch over drug trafficking of cocaine or morphine. That explains how my tube, although packaged ver well, arrived broken and that they stole the colored preparation which was in the same box. If I have time, I’ll make you a new shipment today or tomorrow before my departure for Pontigny and Paris.

In case you need to reach me, I’ll be at the Pontigny Abbey Saturday the 2nd [Oct.] and Sunday the 3rd, and in principle in Paris at Mme. Morel’s, 11 Arcade St. (8^e) the 4th and 5th.

6) Tomorrow I’ll send you some copies of the small printed paper that I had made up, in which I relate my last three experiments [eventually included in *Die Bione*], preceded by an explanation of your synthesis. I intend to distribute them at Pontigny and to insert it in the copy of the paper, to send it at the same time to the scientists who ought to receive it. Then you’ll be able to draw from all the technical information in these few pages the precise things you ask me about these experiments, in order to publish them in your great work. I’ll also send you the text in extenso of the paper that I’ll deliver Saturday at Pontigny and which I’ll pretty much reeat at the Sorbonne on 18 November. You’ll find the explanation of your synthesis in it.

As it explains the discovery of biogenesis, you’ll understand then why I translated—for my personal use—only the second part of your work on the “Basic Antithesis of Vegetative Life.” In fact I only saved the parts that would clarify the process by which you pssed from psychoanalysis to biogenesis. You will see that I even alluded to the social complex of your system. You’ll see also that I said a few words about my own works, with the aim of justifying my competence regarding yours. All right, so much for Copenhagen. If I spoke to you about it, it is just because that seemed suspicious to me and I don’t want to do anything in this domain without your consent.

As you ask me, I am also going to have two SyClos tubes made up and sent to you; but that will be quite long in coming because there is only one worker here. I don’t expect to have them before my return from Paris.

At Paris, even you have nothing to fear about the experiments. Thanks to the SyClos they are much quicker and I have succeeded in inoculating the culture completed even by the evening [of the same day.] In any case, my purpose is especially to show the experimental procedure to a man of good will. It is possible that before returning to Nice I’ll pass by Nancy to operate in the same way with Dr. Colin.

I am not writing to you at any greater length today because of my departure preparations. I am taking all my cultures and at Pontigny I will find a film projector. I am also taking the two SyClos and the h-tube. I’m very pleased that you accept my interpretation of the pure KCl, and I ask you to tell me the result of your experiments by a letter or a telegram that I will have at Paris on the 4th or 5th of October.]

29 Sept.-18 Oct., (OI box 7, “Journal, Odd Wenesland” fld) Wenesland conducts controls of DuTeil’s culture expts: “Kontroll over Professor duTeil’s kulturforsök med KCl og Glödet koks i KCl og Blodagar. [DuTeil’s culture experiment with KCl and incandescent coal and KCl with blood agar] [Karl Fossum trans: From the diary of Odd Wenesland: Control of Prof. DuTeil’s culture experiments with KCl and incandescent coal in KCl with blood agar. 9 Sept. 37: 4 experiments with incandescent coal, immediately added to sterile mixture of 0.1 n KCl and a drop of melted blood agar. These 4 expts referred to as 3eI-IV. Was placed in incubator at 37 C and incubated there for 10 days.

9 Oct 37: all the mixtures were inoculated in bouillon. After c. two days, no growth could be seen. There was, however, inoculated over agar, which also didn’t result in any growth.

12 Oct.: The original mixture of incandescent coal autoclaved for an hour with steam under pressure. The same day, there was inoculation in bouillon. On 14 Oct. inoculated on agar. None of these resulted in growth.]

Thurs 30 Sept., Reich diary entry (*Beyond* p. 115) “I have found the bacillus that causes cancer!! T-bacillus as a degeneration.” Cont’d 4 Oct.: “It is the death bacillus. My isolation seems inevitable...”

1 October, DuTeil to Reich (box 10, DuTeil flds***, [At the moment, leaving for Pontigny and Paris, to play this big role, sending you the items which I told you about yesterday:

1) The text of the announcement that I will make tomorrow in Pontigny.

2) Some samples of the small brochures that I had printed and am thinking about including in samples of the report to the Academy of Moral Sciences, so that each scientist that gets one can clearly oversee purpose and consequence of the relevant experiment.

I am prefixing your synthetic theories with a description of the experiments for a few moments. I believe that I have achieved a more condensed wording that expresses the fundamentals. In this report, you will notice that I have included a lot from your work-of course by quoting you-and that I have also tried to allow the general lines of your synthesis to emerge. I would like to direct your attention to the explanation about Brownian movement that I will give for my part; I believe it is better to take a position of this type, whereby no further discussion of it is necessary. The Brownian movements are included with aim and intention to create limiting membranes and to bring movement into the system. Both of the Sy-Clos-tubes will be ready in a few days. I have said that they should send them directly to you and not wait until I have returned from Paris.

You use the device like this:

The lecithin mixture comes in tube a.

The mixture from KCl, Ringer, coal, etc. in b.

Use only very small amounts.

The bouillon is filled in the tube d. beforehand.

One pushes the stopper, which I had built specifically for this purpose, very far down, so that an inoculation cannot take place while autoclaving, and so that the first operations can take place without risk of a premature inoculation.

Then everything is put into the autoclave.

Afterwards the contents from a and b are mixed in either one tube or the other (a or b), preferably, though, in a. Should an inoculation now take place, one puts a drop of this mixture into tube c, which has remained empty up to now. After this, the stopper in tube d is withdrawn a bit, so that the connection tube is open and the bouillon can flow from d into c. A little bouillon remains in d. for control purposes. A wad of cotton can now be placed in tube d to prevent an infection of the control bouillon through air in the other tubes.

I will close for today to complete my travel arrangements and assure you my sincere friendship.

PS The last bions were created without milk and with methylene-blue instead of red.]

4 Oct., Reich to DuTeil (box 10, DuTeil flds) "In your letter of Sep 29 you touch upon a few questions about our work relationship. I totally agree with your statements. I believe it is best if the Centre Universitaire of Nizza presents itself to the world in two different ways: First as the prime decisive Control Station for the experimental work of my institute, but second, at all costs, much farther, if you agree, as a parallel working research station. I believe it is good to already envision the development of this work relationship. With the above statements, I believe, I have best described the current state of the situation. I am asking you to please write to me, whether you agree with these statements. For the requirements of my work it is, for the time being, sufficient, that the world recognizes my institute as the source of the original theory and the source of the fundamental experiments. In the future we can talk about further possibilities of a more detailed order in our work relationship. I am convinced that I will never have difficulties with a friend of your nature, but you know, dear friend, that I never quite can get rid of my concern which probably stems from the complicated connection of all the parts of my scientific perspectives. The demands are truly not small. I know that very well and you can be assured. I never cause unnecessary troubles. That leads me to the Oslo racket in the newspapers. I could not quite understand from your letter, whether you believed, that our working circle had caused the start of this press hoopla. That was definitely not the case, on the contrary, we were upset about it. It was simply the outburst of a very private and insignificant group, which fortunately hurt themselves and not us, as it now becomes more and more obvious. For a short time I was afraid that this incident might cause damage to the quiet development of your work in France. But I do not believe that this is the case. I do have like yourself every interest to let the development of this affair take its course in quiet and with patience.

I am extremely happy with your decisive remark, that the confirmation (acknowledgment) definitely must and should be obtained in Paris. I am also totally in agreement with you that one should not present too much all at once to the world of science. My inquiry regarding the cancer case was not referring to a possible public statement, but only the sounding out of future possibilities, without anything being announced in any way. In that respect I would like to ask you an open friendly question. I would like to tell you that I have an urgent need to share with you an abundance of partly very positive, but partly almost confusing new facts, which I am learning about here. It is great and productive to have a discussion with you because I feel that you have a direct and "vegetative" interest and understanding for it. But I can also imagine that I am distracting you from your own work in this field with my interventions and observations and questions of a different kind. As great as my desire is for discussion with you, my willingness to hold back is even greater, if that is the right thing, as my intuition above tells me. I am sure you have noticed that in our circle, instead of superficiality, insignificance and uncalled for politeness, a direct openness steps in. The following one only asks his very best friends. I am asking you then to please tell me very openly, if it is alright with you, that I keep you up-to-date on the cancer research, or whether it would be better, from your workstand point, to just notify you with the final results of the major experiments. This would have its advantages but also its disadvantages, because the whole work approach is so strange and unique, that easy contact is lost if the methods are not continually and mutually

discussed. **I would like to use an analogy: I have landed in a jungle and I have built a wide road, but from time to time I am tempted instead of continuing to build the road straight ahead, on the basis of a vague intuition, to penetrate sideways into the jungle. I can hardly give account to myself for why I am doing this. The way this work is developing here on the cancer problem, that is such a new jump sideways. But I am assured that it absolutely proceeds in a correct fashion. The logic which unfolds itself in front of us---outside of the working people---is sometimes almost creepy.** I know that you understand me. To publish something like this officially could mean scientific suicide. Please write to me about this.

You are of course right. We will not share anything about the cancer work, we will not publish one sentence, as long as the first part which you are working on now, is not finalized and safeguarded. When you meet with cancer researchers I ask you to get more information and to find out if anywhere in the literature there is knowledge about cancer having anything to do with self-infection.

Next week I will start a great experiment with 80 mice, which will repeat and encompass all smaller experiments up to this point. I am eagerly anticipating it.

You will receive the unabridged versions of the printed "Vie et Matiere" in small parcels as requested.

I still have not heard anything from Kopenhagen (i.e., Neergard). I simply do not understand it. About the working through of your KCL experiments here with us, I cannot give you any results yet unfortunately. They probably will not be available until October 9th. We have calculated that the whole repetition of your experiment will take about 12 days, exactly what you indicated.

All of us here are very curious about the outcome of your Paris trip and the success you will have. Please do not keep us waiting.

The preliminary publication of the bion experiments in our journal has made great impressions in several places in Europe. I have received enthusiastic responses from several people. Local [Norway] young biologists from different university labs have looked me up and have gotten all experimental and biology work material. They want to study it and be part of it. This only on the basis of the one article. In Stockholm, a physicist whom I don't know is interested and also one in Kopenhagen. From Switzerland I received 20 francs from a woman for my lab, on the basis of this article. So, that is all for today.]

4 Oct., DuTeil secretary Pomproy(?) to Reich (box 10, DuTeil flds), I am sending you today via separate mail, and following the instructions given by Prof. DuTeil before his departure for Pontigny:

1) A colored preparation of the Sy-Clos tube of the third experiment, prepared on the 18th and recultured in bouillon on the 20th of the month by closed system.

2) A tube of the bions of the first Sy-Clos.

For these last experiments, there is good reason to take into account that, due to the difficulty of placing them in the vials, possibly we may find foreign infections there.

Reich to DuTeil, 5 Oct. (box 10, DuTeil flds) **I just received your shipment containing the printed materials as well as the presentation for Pontigny and read them immediately. I must admit that I am exhausted from unfamiliar emotions. I would not like to say much. Only this: In my event filled life, I have never as yet encountered such direct and courageous humanitarianism. Just as little such understanding, such help, such true, purposeful friendship. I hope to be able to thank you appropriately for this someday. The best reward will be the concrete help that our joint work will probably bring to this down-trodden human race. Please accept my sincere and complete thanks!**

I wish you and all of us much success in your endeavors. You know that simple, working people are following each of your steps with anticipation and great delight.

I will write you again tomorrow in regards to business.]

DuTeil to Reich, Fri 8 Oct. 37 (box 10, DuTeil flds, German transl 3 pp****, French original [compare Sellers translate] **photos 1687-1695**) I write you quickly now to give you an account of my trip and my experiences in Paris, from which I just returned last night.

My paper at Pontigny was to an audience of 65 people from France and abroad, among whom were several doctors and bacteriologists. If it was true in principle to the text which I sent you, in reality I presented the question a little differently, as a way to separate on the one hand the synthesis, which is based on previous works and theories, and on the other hand the present works, which only consist of a verification of this working hypothesis on the particular point of biogenesis. I thus took a very strong position, noting that even if it revealed an experimental error in our last works, the synthesis of it remained no less a new and very grand thing, able to serve as a prolegomena to any test of future biogenesis.

The impression [I made] was very deep and one day you will have the printed account of the discussion which followed. Moreover, the result was immediate. Dr. Philip Decourt—very well known in France for his remarkable works on malaria—and

who is the clinical head of the Faculty of Paris, invited me to come later for two days to redo the experiments at the laboratory of the Claude Bernard Hospital in Paris, which the famous bacteriologist Reilly directs for the laboratory and bacteriology.

On the other hand Dr. Martiny placed at my disposition the laboratories of the Leopold Bellan Hospital and made a date with me for 18 November, a day when I should also have lunch at his home and give the Sorbonne lecture.

Monday morning I was in Paris and immediately contacted Dr. Henri Bonnet who met me at 4 o'clock at the end of his course on practical works in the service of bacteriology at the Faculty of Medicine, where he teaches under the orders of Professor Debre who is likewise one of the greatest French bacteriologists. That evening we started microscopic examinations as well as [inoculating] a culture from one of my tubes, a culture which on agar grew up as soon as the next morning. [1688] The experiments resumed at 2 o'clock Tuesday afternoon at the Faculty of Medicine. With Bonnet I created bions following your formula [Prep 6], although without milk, such as I had done successfully myself several times. We used the SyClos tube, which stayed over there and which will be used to inoculate bouillon for a culture several days from now. Moreover, Dr. Bonnet decided to redo the experiment several times and this test did not yield anything. Also Professor Debre whom numerous professors and assistants, who had heard about the affair and gathered in great numbers around me, all listened with great scientific curiosity and with an attitude that was perhaps astonished but certainly sympathetic. We can be certain that the matter is going to be studied over there with all necessary seriousness and conscientiousness and if, as we must hope, the results are positive, the thing will be officially accepted and known about a few weeks henceforth.

Wednesday morning, I started the experiments again at Professor Reilly's laboratory at the Claude Bernard Hospital, which is the largest hospital for infectious diseases in Paris and in France. There, the experiments were redone with the collaboration of Doctor Philippe Decourt, who had already learned about the matter from a representative of the Rockefeller Institute [sic] after a trip to Oslo, and who had become very interested in these experiments. There also, the appearance of organisms was noted by Reilly himself, who having put his eyes to the microscope simply said "It is exact," and continued his works. There also, I left the tubes containing the preparation, for a culture which will be attempted in a few days. There also, finally, Doctor Decourt declared to me that he would redo the experiments as many times as necessary to arrive at a result. Finally, I also left a tube of cultures there, to submit the organisms to their examination. The atmosphere at Claude Bernard [Hospital] is even more sympathetic, if that's possible, than at the Faculty of Medicine.

There, my dear friend, is my work for the beginning of the month. I think it will satisfy you. Concerning what you tell me in your letter—which I received in Paris—I don't want for anything in the world for you to stop keeping me informed about the development of your works. I need to know about it even if only to clarify the beginning. All I wanted to say is that I consider it necessary to leave people time to digest the new nourishment we are giving them. I keep what you report to me in my head and in my drawer but have decided not to talk about it until later, once the first stage is accepted and also when the new discoveries are confirmed. In fact, I realized in contact with these professional bacteriologists that we are arriving at a very dangerous period.

As soon as we arrive at a general synthesis, based on our great, well-known and popularized theories, such as the one I presented at Pontigny, we can discuss [our ideas] with anybody, fully as equals. But when we enter a domain as vast, complex and deep as that of cancer, which has already given rise to hundreds and millions of remarkable studies to which people have devoted their entire lives, and where the greatest bacteriologists break down before problems of detail accessible only to a few initiated people, it is absolutely impossible to discuss these questions with them unless we have a culture as deep and complex as theirs. All questions of bodies [i.e., antigens] and antibodies, of genes and antigens, of toxins and antitoxins—which can act even after sterilization, and sometimes *only* after sterilization—render these questions appallingly complex and require us before announcing a new find or discovery in the order of things, to have obtained the agreement of a bacteriologist since we are lacking in personal and specialized knowledge—a bacteriologist who could speak the same language as the others and discuss the objections with the others on the same level, taking into account all the latest discoveries and the present state of the question. In other words, just as a general synthesis may be brilliant, staying not only on the surface of things but on the level of the profound meaning of each thing, so from the time that you want to proceed to the *application*, it is essential to do it only with *all the weapons of the technician and the specialist*. Moreover, the difficulty I note is that when you try to approach all the specialists you'll find yourself in the same situation as when you want to approach just one of them to get him in your camp. With even this one, only with much patience and circumspection and an extremely progressive program are you assured of his support. Furthermore, you should aim for a technician who is a modest executant, having never made [important] works of his own and who, consequently could attach himself to yours without reservation.

I advise you then, in the interest of the development of these works, to attach a young bacteriologist to your laboratory, who, just coming fresh out of university, would be perfectly au courant with all the latest works on cancer and tuberculosis, in order that he may translate what you have found in technical terms accessible to all the technicians. In this way, you'll avoid replies like that of the cancer specialist [Kreyberg] you quote to me in your letter before last. Or if such a reply has occurred, you'll have at hand someone who can provide you right away with all the technical arguments to reply to it. What concerns me, I

am often in contact with the Director of the Nice laboratory who gives me precisely, from a technical point of view, precise information and essential orientation. He provides me with arguments that sometimes I lack. For the rest, I worked and worked very seriously every day on a manual of bacteriology, where I found fundamental information without which I could have done nothing. This allows me to make in a few weeks an approximate identification of the bions and to reply with precision to *very detailed* and *very technical* questions which were asked of me at Pontigny and at Paris. To the point that Dr. Bonnet was very astonished to learn by chance, when I was leaving him, that I wasn't a doctor. Bacteriology has become a science of extreme complexity and the question of the culture media alone, for example, demands months of study. It is even possible that methodological changes in the pH by two doctors with whom I had to discuss more than twenty-five minutes; if I had not been able to reply to them, I felt very clearly that they would no longer have taken my experiments seriously. It is necessary to speak to people in their language and to be with them on equal terms at all levels.

I'm all the better able to tell you this since I know that you know my friendship toward you and will take this as a mark of my friendship. I have in fact just received your short letter of the 5th from Oslo and am myself very moved to have hit so close and that you felt this. Tell all our friends that I am with you... RDT

11 Oct., Reich to DuTeil (box 10, DuTeil flds****1 p) I am writing you just a short message to very that the Syclo-tubes have arrived. They are still at the customs office, and I am thinking over a convincing explanation why they should be released without import fees.

I would like to take this opportunity to draw your attention to an error that was found in your short, self-published report.

In the preliminary remarks concerning my work, you write that I modeled my biological works after the Freudian differentiation between "towards the world" and "away from the world." I just proved in the first part of my work about the primal anti-thesis of vegetative life that the Freudian concept of fear as a mental signal does not help the problem of anxiety in dangerous situations, rather complicates it. I arrived at my account of the two opposing electrical currents in biological organisms especially through criticism of this Freudian theory. You have no idea of the fight that swept through the Society of Psychoanalysis centered on this one question. Freud claimed mainly that fear is an agent of impulse suppression and that the energy of the suppressed impulse had nothing to do with fear. I adhered to his original formulation from the year 1900 stating that topical anxiety corresponds with continuous sexual desire. I first arrived at my independent physiological explanations concerning the identity of direction of flow, emotional character, and the dialectic contrasting unity between breath and anxiety in continuation of this original Freudian perspective, which then lead to criticism of the later Freudian formulations about anxiety.

The first control of your KCl experiment is approaching its end; however, I will complete another experiment with the Cyclo-tube [sic, i.e., Syclo] that you sent and then send you all the results. We here are very anxiously awaiting the results of your trip to Paris.]

Reich to Kreyberg, 12 Oct. "In the following, I would like to provide a short summary to clarify and address the situation of the results from our discussion on October 8. I have subsequently recorded the essential points here.

At this point your counterarguments mean a challenge to my work similar to the argument of pre-existing spores, which, at that time, was the response to my first report concerning the cultures of autoclaved substances. This objection then led to the soot-incandescence-experiment that I showed you, and that led to cultivation of the incandescent-soot-bions. A similar position now begs the question of whether the diagnosis of the granulation tissue, which you and another department made, is correct. You make the strict and clear-cut differentiation between inflamed granulation tissue and cancerous growth tissue, justifiable from your methodical point of view. I encounter that a totally distinct and principle separation between both types of growths is, for several reasons, not possible.

1.) I have examined the tissue in question at an immersion magnification of 3000X and found the outer pieces to be moving, likewise cells in the liquid that moved like cancer cells tend to move. They were only a bit elongated.

2). the day after our conversation, I examined the hibernation gland, about which I previously knew nothing, in a healthy mouse and found the tissue dormant at the same magnification.

3). When I microscopically examine cancer tissue under the same conditions and find similar phenomenon, then I can no longer draw such a sharp boundary between granulation tissue and cancer tissue. It appears to me that an entire list of different qualities appears in the phenomenon, which turn from benign to malignant growths at some specific point.

4). you claim that one cannot diagnose cancer cells based on microscopic observations alone. I encountered that I have learned to exactly differentiate between cancer cells and the more bacteria-like structures in fresh cancer tissue.

5). concerning my basic standpoints, I shared with you my observations involving charcoal and soil crystals where I watched and filmed motionless systems develop movement. Because it is generally known that in the case of cancer dormant tissue changes to moving tissue, I found this discovery from outside the area of cancer disease to be significant.

6). likewise, I explained that I consider the vesicles and rod-shaped structures as well as the vesicular clusters that one finds in sterile cancer preparations to be an internal sign of the disease instead of a tissue infection.

7). the difference between granulation tissue and cancer tissue would, thus, be not in principle, but rather merely gradual. The difference would be, from a purely clinical pathological stand, the destructive growth of cancer tissue and the formation of metastasis.

I would like have an in-depth discussion with you specifically about the issue of metastasis formation. I shared with you that I arrived at the cancer issue through my studies involving malfunctions in the vegetative system, and that I wished to seek your help and guidance as a cancer specialist as I position myself in this field. Above all, I asked you to make blood from cancer patients available to me. We remained in agreement that we would both wait for the time being until the issue in my experiments was further clarified.

I stand by my plea to make the blood from the cancer-stricken available to me. I believe I have made some observations that could perhaps be vitally important, and I am obligated to continue with the investigation until it is determined to be either correct or false.

I ask you again to please tell me whether or not it would be possible for me to obtain the blood from cancer patients needed for my investigations.”

14 Oct., Reich to DuTeil (corresp box 10, DuTeil flds***) All of us wish to thank you most sincerely for the most welcome results from your trip to Paris. If the cultures with Bonnet succeed, then we are relieved of our greatest problems. If they do not succeed, we will have a lot of difficulties. I am in awe of your ability to confer with specialists. I wish I had the possibility to learn from you. Unfortunately, I am bound to very dangerous angles with many types of rigging.

Regarding your remark addressing the importance of being appropriately well-versed, I can only agree. I find it to be a great advantage to be a medical doctor and be familiar with the basics of biology, even when I am not a specialist in the area and am lacking completion in each department. But that is easy to learn as I go along, along from the necessity to refute objections. I have learned very much about special techniques these last four years. You do not have to fret. Theoretically, I have mastered the cancer issue. Your suggestion that I hire a bacteriologist will be immediately implemented, as I have been thinking about it, too, for quite some time, as soon as I have the material means to do so. Unfortunately, at this time a breakdown in my praxis has left a huge hole in the budget.

To prevent bothering you until verification of the bion experiments, I will wait patiently and not proceed any further. I am considering whether to postpone the big publication until then. If it fails in Paris, then I can still include those arguments. If it is a success, so much the better. I will publish the verification with it. The protocols, though, are necessary to do this. Could I get them in any case? I am ready for the results, positive or negative. My conviction is all too freely based. Yesterday I wrote you a five-page letter, but I decided not to send it to you should you have unpleasant experiences with the biology specialists. I do not wish to disrupt you.

Should I send you a complete galley proof of the publication? I think that would be good.

I do not have the slyco-tubes yet. I hope they arrive soon. On the other hand, I have examined your bions; they are phenomenal, even electrically negative as they should be. The seeding here took place yesterday.

I will write you a detailed report about my work with cancer as soon as I have somewhat collected the essential facts. Right now I am still dealing with possible objections along the line of the spore objections. Local cancer people claim there is a distinct boundary between cancer tissue and inflamed granulation tissue and no connections. I reject that, and my opinion is included in the literature. I have been able to create tumors by which to address this question. Could you please find out how specialists there think about this? I was unsuccessful in my attempt to come within reach of cancer patients. That is very obstructive, and I do not yet know how to get around it.

Please write again soon. We translate your short experiment reports and have them ready for the big publication. I have deleted the phrase dealing with Freud (compare to my last letter) and wait on your verification.

Your letters stimulate the work here. May I ask you to share the suggestions that you get from your bacteriologist friend with us? I will write you again very soon concerning some currently ongoing experiments about the bion problem that are very nice.]

14 Oct., Reich diary entry (*Beyond* p. 115) “I am on the track of the matter. 1. Local oxygen deficiency due to poor respiration makes tissue decay.

2. S bacteria form.

3. The tissue regenerates itself as a tumor.

4. The S form the metastases.

5. Sarcoma S is more dangerous than carcinoma S.”

15 Oct., Reich to DuTeil (corresp box 10, DuTeil flds, 2pp each***, Your Prep. 6 bions sprouted magnificently in bouillon and today on egg- and blood-medium, too. I can verify that it is a pure type 6 bion culture, electrically negative as well as gram-negative. My sincere congratulations. We were all overjoyed. Hopefully this continues to be so good.

In the meantime I learned to differentiate bions from other bacteria. All types of bions as well as their cultures are generally larger (2000X magnification) than staphylococci. They move very differently than rot [bacteria] or other wriggly rods, more on the same place, instead of darting through the field. I believe that there are two basically opposite types: bions that carry energy to start life and are created from inorganic matter and rods that develop when living matter decomposes.

You will always find bions in autoclaved moss; however, never the second type, this is the wriggly, quick-moving rods. Likewise in other substances. The bions are also electrically different. They are negative or positive. The rot bacteria and my S are either electrically neutral or immediately dead, not + or -. [Rot bacteria and my S (t-bacilli) are electrically neutral, neither positive nor negative in charge.] One suspicion: the red or blue gram reaction must be connected to the electrical charge. Please compare with us several types of bions and other bacteria for a similarity between positive and negative gram and the electrical reaction. We have already begun with this.

I will inform you about the S experiments in context. Have you already received a bacteriological diagnosis of it? It should be *B.pyoceanus*. I am not certain.

I have gotten the S from the blood of tarred mice both with and without tumors. I call it S2. It is microscopically the same as S1 and also electrically neutral. Long hours of faradization do not affect the cultivation. They smell just as sour and pungent and immediately harm the mice.

We are eagerly awaiting your Syclo. We want to repeat your KCl experiments with it again.

And now a big, big favor: would it be possible for you to get me blood samples from cancer-ill patients? Nothing more is involved than the sterile extraction of a couple drops of blood from the elbow vein. The blood is immediately put in bouillon, nothing else. I will take care of the rest here. You can send me the blood-bouillon in fused ampoules via airmail. Is that possible?

Havrevold and Hoel were so thrilled about your report from Paris that they immediately took copies to solicit money. Financially it does not look very rosy at the moment.

If you use egg-medium, then please only fresh ones. Old egg-medium gets too dry and often yields no results.

You will hear from me shortly about some detail experiments as addendums to the soot-incandescence- experiments.]

Reich to DuTeil, 15 Oct. (*Beyond*, p. 116-20, written on 13 Oct., acc to WR to DuTeil 14 Oct. letter) **not sent**. "I have just very carefully read the German translation of your letter of 8 October and I must tell you that I am quite unable to compete with you in the marvelous way you manage to approach scientists from other fields. What you have achieved with Bonnet, Martiny, and Reilly on the first attempt is more than one could have dreamed of under the best of circumstances. If these colleagues truly keep their promises and carry out the experiments until they succeed, then we will certainly be home free as far as the first matter is concerned—namely, the production of bions. These remarks do not apply, or at least not yet, to the second matter—that is, the cancer question. I would like to state briefly that I am totally immersed in this subject and did not get into it by chance. In the same way that the bion experiments developed logically from my clinical orgasm formula, so the studies on cancer tissue and the experiments on animals are developing equally logically and consistently. I sometimes sit here completely dumbfounded and cannot believe that this logic is possible. I would like to stress that although I work with a great deal of intuition, this intuition is backed up by very solid clinical and experimental facts. I am helped in all this, last but not least, by the cognitive method I use. [i.e., dialectical materialism]

I would like to attempt to answer the second part of your letter from this particular work standpoint. You are **completely correct that**, because of the close contact that we have now, fortunately, established with the world of bacteriology, all of us have entered into an extremely dangerous phase. If the work that we now do is successful, then we will certainly triumph. If anything goes wrong at any point, however, the setback will be far greater than the disadvantage of having no contact with this particular scientific field would have been. **The clarification of my working viewpoints with respect to specialists from other fields is important to me, especially because I agree with you about everything you wrote in the second part of your letter. I am, as are you, fully aware of all dangers, and I will speak with you, as always, in a very open and honest manner. It is preferable for you to be familiar with each small iota of my work as well as my character and clearly see the issue than to have some lack of clarity creep in and cause confusion over time.**

The bacteriologists with whom you are dealing in order to obtain confirmation of the bion experiments are highly specialized experts and I know that I could not engage in a discussion with any of these people in their particular fields. Although I am familiar from my medical studies with the fundamental aspects of bacteriology and its methods, the field of bacteriology has become much broader and deeper, and I would therefore not undertake to engage in any discussion of specific bacteriological problems. There can be no doubt that this is a correct assessment of the situation. On the other hand, it is equally certain and clearly established that the best modern experts in bacteriology cannot argue with me on the basic problems of my clinical experience and my specific experimental work. In this connection, I would like to express a feeling I have always had whenever my enemies, or people who review my work from afar, described me as a systematist, a philosopher, or a synthesist. I do not feel I am any of these three things. I am a scientist working in a field where I must use methods of research and thought developed entirely by myself that are not known in any of the existing specialized fields. I am referring to dialectical-materialist sex research. Recently I had the opportunity to say to a cancer specialist who was very arrogant to me in the presence of several

other physicians: 'Dear colleague, I fully recognize your exclusive competence in the field of cancer research in its present form, but I would ask you to take note of the fact that I claim absolute authority in all questions of sexual functioning and its relation to vegetative life. I do not know about your cancer problems, but you are not knowledgeable about my field of expertise either. I hope that in the interest of the matter at hand we can arrive at an understanding.' I have had this basic attitude for a number of years and I shall continue to cling to it. **I experienced an important approach, crucial for my later work, when, in the beginning of the 1930's, I had to fight hard for the Freudian Scientific Psychology of the Subconscious against opponents such as trained psychiatrists, experimental psychologists, neurologists, etc., all people from special areas with whom I could discuss nothing. The experimental psychologists attempted to refute or ridicule the Freudian Method of Psychological Examination by countering with the methods and rationality of awareness psychology. For example, a psychiatrist once said to me: 'It is ridiculous for Freud to claim that the kleptomania impulse is sexual...I have seen many kleptomaniacs and could not observe anything sexual.'**

Dear friend, just imagine that one had conceded this viewpoint back then. He was correct, and can, with a clear conscience, state that, according to his methods of examination, kleptomania has nothing to do with sexuality, for the kleptomania really knows nothing about it. However, with the analytical examination method, one could definitely determine that compulsive kleptomania is a substitute for sexual desire or masturbation. We supported this standpoint: 'We do not wish to interfere in your sphere of work; however, if you seriously wish to discuss our work with us, then we implore you to first familiarize yourselves with our examination methods and to adopt the reasoning upon which they are based.'

We must demand from the experts criticism grounded in our work [immanente Kritik]: to judge our work they must, if they are at all honest, temporarily abandon their own methods of thought and adjust themselves to our position and criticize us from the standpoint of *our* work. This does not mean that we would assume a superior air and declare that we know everything better and that the others are fools. [By now, probably they think what he's asking so outrageous, so beyond their concept of science, that they cannot help but hear his demand as arrogant. Yes?] No, certainly not. Everything, absolutely everything, must be done or attempted to explain our material and methods to each and every specialist with whose field we come into contact. For example, you are absolutely right when you say that I must very soon employ a young bacteriologist who will try to situate our results in the overall bacteriological scheme of things. [Again, is it already too late for this? Are these two kinds of science already speaking such different languages that translation is impossible?] However, if this should sometimes prove impossible, then we would first of all have to check to see who is right—existing bacteriology or ourselves. (I am reminded again of the diagnosis of staphylococci that was made in the case of structures that beyond doubt are not staphylococci.) I will do everything possible to accelerate, and bring about, this linkup; of that you may be fully assured. But I cannot and must not adjust myself to the ways of thinking and the methods used by bacteriologists lest I lose the thread of my own study methods and thought processes. I am certain that you totally understand what I mean here, and do not fear, even for a second, that you will misunderstand me. I have the feeling that you have now come up against a scientific world to which you must prove yourself a match in the capacity you have assumed. But I would like to be quite open with you. I have the feeling, and we have already talked about it here, that you are too optimistic in your assessment of the willingness of the scientific world to accept these matters today. Please permit me to retain my skeptical attitude until you succeed in convincing me that I am wrong in holding to it. I assure you that I will do nothing to hamper you in your attempts to convince the bacteriologists and biologists. On the contrary, I will make all the material and all the controls available to you so that you will be fully familiar with our work. I shall also be happy to learn of any objections, even awkward ones, you report to me. But you will not be able to make me surrender my skepticism about the willingness of the majority of scientists. I know all too well how the highly specialized worker is anchored in his own problems and ways of thought and I will not abandon myself to any dangerous illusions on that score. I accept the fact that I shall have to go on working for ten years and more, quietly but firmly convinced of what I am doing but without any means of arriving at a breakthrough. Please believe me that I do not want to play the role of martyr. I merely wish, using all the means available to me, to prevent you, a valuable and cherished coworker, from getting into difficulties by entertaining illusions about the possibilities of a quick breakthrough. **[Reich has the wisdom (as DuTeil perhaps may not) to forsee, for ex., the possibility of DuTeil losing his job as actually occurs in June '38]** Let us wait calmly and see what happens. We do not have to somehow raise ourselves up to participate on the same scientific plane as the current bacteriological or other disciplines. In our own specific fields, we are equally good scientific authorities. Yes, even better. Why should I hold back? A biologist or psychologist who does not quickly comprehend the basic function of life—orgasm and orgasmic yearning, the orgasmic convulsion of all living organisms, and the relation to the charge processes in living matter—or who has never himself even come close to this fundamental fact cannot, at least in my opinion, be regarded as an absolute authority in the field of biology. That is not conceit on my part; on the contrary, I tend to be far too modest. But I say this in the absolute, scientific, clinically and experimentally obtained conviction that sexual aversion flourishes not only in religious dogmas but also in the mechanical, materialistic, and mechanistically specialized scientific disciplines. Over and over again, I have found that extremely personal character weaknesses have a damaging effect on many people's scientific work, which should be objective. Let me give you an example. Kreyberg is the only cancer researcher here in Oslo. When we obtained the first tumors in our mice, samples of the tumor tissue were set in paraffin in a scrupulously correct manner and sent to his institute for examination, with a request that he let us know his opinion. After a few days he convened a meeting at which he expounded his view in an extremely professorial manner, proclaiming that it was nonsense to speak of a carcinoma when all we were dealing with was inflammatory granulation tissue. He asked me whether I could diagnose a cancer cell under the microscope, to which I replied yes, because I really am capable of that. **[Compare with Nic Hoel's and others' version of this story from the 1952 State Dept interviews]** I then asked him whether he had ever seen a living cancer cell at magnifications of 2000x or 3000x. It turned out that he had not. This cancer researcher is afraid that if I am allowed to examine his motives for even a moment, his status as a specialist will be questioned **[at a critical moment in his career/campaign for the university professorship; is WR unaware of this?]**; but he has absolutely no

reason to think that, because I very kindly and politely offered him the possibility of cooperating with us. Now he is trying to 'finish me off.' But the physicians who were present and who have to struggle with the practical work of treating patients were on my side. One elderly doctor said to him, after I had spoken: 'Yes, why shouldn't there be any cancer cells in granulation tissue?' And that is also correct from the standpoint of current cancer research. It is absurd to strictly distinguish granulating and proliferating granulation tissue from cancerous tumor tissue when, in addition, it is known that cancer can develop very easily from granulation tissue. But this example shows quite clearly that we are exposed to great danger—namely, the danger of colliding with all the forces of conservatism in science. To escape destruction and, on the contrary, to make gradual headway, I have employed the following people in my laboratory: a specialist in chemistry and physics, a trained bacteriological laboratory assistant, and a trained film maker. And in Dr. Havrevold I have found an extremely well-trained expert on internal organs and, in particular, on internal secretion. As soon as financial circumstances permit, I will be very happy to follow your advice and employ a bacteriologist and a cancer specialist. As you correctly observe, in this way I will discover in advance what the objections might be and will be able to take part in discussions armed with the necessary counterarguments.

At the moment, unfortunately, I am struggling with colossal financial difficulties. **It is not pleasant that I must inform you that, as it now appears, the ability to continue operations at their present scale is very questionable. I do not have a single official government resource behind me as support, in contrast to the many, many government-employed specialists armed with all sorts of research funds without having to worry about them. But exactly this situation obligates me to two things: first, to devote the utmost attention to detail in the development of my results, while at the same time defending them with firm determination using all resources available to me.**

I sincerely hope that you do not hold this long and, as you see, very poignant message against me.

Your Syco-tubes have not yet arrived, however the bion preparations, which I will hurry to examine, did.

We have translated your report about the three series of experiments ["Three Series of Experiments Based on the Tension-Charge Principle"] and will publish it in the major work. I took the liberty of deleting the words 'at that time distinguished from Freud' (...psychological tendencies...) in the lines of the third paragraph. I explained the reasoning in yesterday's letter to you. I request your permission to do this. You will find the fundamentals in the work 'The Basic Antithesis of Vegetative Life.'

You are undoubtedly correct concerning the influence of pH on the success of culture growth. Sharing this same suspicion, we have recently been looking around for our own pH-device. I also have a strong inkling that you are right about not adding milk to mixture 6. In any case we will re-examine the cultivability without milk. I will report in the near future on a very nice little experiment that we are currently completing. Likewise a few things about the developments in working with cancer.

P.S. I have just examined your bion preparations. They are totally fabulous, authentic bions. The electrical examination shows that they are strongly negative. We just seeded them onto bouillon- and egg-medium. I am including the results in this letter. They are gram-negative. **It just occurred to me that the gram-positive (blue) as well as the gram-negative (red) could be connected to the electrical reaction of the relevant structures; staphylococci should be gram-positive. Bions are gram-negative. Staphylococci are electrically positive, bions electrically negative.** We will investigate this further here. It would be an interesting little tale.

15 Oct., DuTeil to Reich (box 10, DuTeil flds, Reich German transl, French original "My dear Friend, Relating to the letter which was addressed to me by the translation of my small printed paper, I share your opinion about inserting the word 'anorganisch' before the word 'chemisch,' for it is clearer.

There is likewise and in fact a printing error concerning the first date of 3 September, which is really 31 August. For the rest, the translation is very good and I send it back to you enclosed.

Concerning the other errors indicated, the word 'fuchsine' should be written 'fuchzine' and there is a printing error in my text. Moreover, it's a question of the 'montee' of pressure and not the 'montree.'

These errors arise from the rapidity with which I had to have the printing done.

I still haven't heard any news about the Paris cultures and think that I'll hear in a few days. I'll inform you, of course, right away of the result.

I'm sending you by the same mail the three negatives [plates?] which you asked for. Kindly return them to me after you've used them.

I received and I thank you for the three reprints of my paper "Life and Matter."

Best wishes to our friends and to you...

PS I just received your letter of the 11th. It is destined for a public which is oriented so much to a biological and biogenetic position. (next handwriting illegible)]

PS I just received your letter of the 11th I have deliberately 'stylized' your thesis for an audience that is based only on the biological part, ((?-In manuscript quite illegible) simplified) and " by myself 'turned on' the functional point of view ((? -difficult to read)). In my great exposé I approach your interpretation even more.]

18 Oct. 37, “Erste Beobachtung über den S-bazillus an Präparaten von Krebskranken,” (OI box 8, Präparatherstellung und Protokoll, ms pp. 78-85)

20 Oct., DuTeil to Reich (box 10, DuTeil flds, German trans 2 pp*** I am replying once to your last three letters. Working on the rentree [return of students] has taken up my time, and I am obligated to be brief; excuse me.

I received with much pleasure the letter of the 15th in which you tell me that the Prep. 6 bions are true to the ordinary type and could be immediately cultivated as usual. This is a confirmation of our respective procedures which is very encouraging.

It is just perfect and I did not find any misprints. I'm going to put on a cover and will send them for circulation next week.

For the time being I have held up my experiments waiting for the results from Paris and also because of the preparation of exams and courses, which pretty much occupy me since my return from Paris relative to the possibility of setting up a more important laboratory, and I'll resume the works when I'm settled. I think you received the account of my stay in Paris and Pontigny and also the shipment of the bions and the microscope preparation effectuated during my absence. I'd like to have your opinion on this subject.

I had a visit from [Victor] Bauer whom I received rather coldly. I'm advising you of this, because he seems to interfere again a little too much in matters concerning you. This in case he writes you. I didn't tell him anything or almost anything, being I think entirely in agreement with you on this subject.

I was still more pleased about all the information of a microbiological order that you give me on the bions such as they result from research and the experiments you made. Moreover, they confirm those which I obtained myself, in particular on the two types of bions which are apparent following their origin. To note anyway that the short rods only appear in general rather late. As for the ovoid form, it appears in the first culture but likewise after a few days, the first form in the pure culture being circular [spherical] as in the original mixture.

This information which you send me entirely satisfies the demand that I addressed to you relating to the necessity of penetrating more deeply in the microbiological, which we had made up to the present time. I know, of course, that your position as a doctor entails a deep knowledge of biology and has alone allowed you to realize the elements of synthesis that we present today to the scientific world. But, I was able to convince myself, in hearing very notable, even famous doctors discuss with microbiologists that there was as much difference between microbiology and medicine—and even with medicine—as between the study of sociology and history, for example. This is why having to discuss now with the bacteriologists, I asked you to help me translate, in their language and in the deepest way possible, the notions of general order, either biological or medical which we have established.

This is what you have understood very well and I thank you for the valuable details you give me, which I lacked until now insofar as I could not penetrate them myself. I am in agreement with you that the day when it is materially possible, the collaboration of a bacteriologist will become a very useful thing and of a nature of simplifying our task remarkably.

For my part, if I obtain from the French government the help I solicited, my first concern will be to assure such a collaboration for myself, under penalty of being condemned either to come to a standstill on the spot, or not being able to discuss the difficult objections that are made to me.

On the other hand, the biological phenomena of a general order have, on the microbiological level, technical repercussions which, if we know them perfectly, will allow us profitable verifications and will give us the supplementary aims for having our theory accepted.

Of course, I agree with pleasure that you should send me a copy of the films that you are now developing on the coal, moss, etc. experiments. Send them to me quite soon so that I may see them at length and prepare the verbal commentaries which must accompany them. Moreover, I'll decide at this time if it is useful to have them shown at the same time as the others or if it would be better to separate out the questions in order to let the people digest them. If, in fact, I take the trouble to explain a theory based on the lecithin-cholesterin, potassium-calcium antagonisms, I risk weakening the theory if I show experiments in which it is not a question of these elements (which are not concerned with these elements).

It is necessary for me to fill the role of regulator between you and the scientific world so that the machine does not fly off the handle and get jammed up.

The Sy-clo tubes, if I understood it well, you were told a few days ago, were held back at Customs, therefore I can do nothing to hasten their arrival. For documentary purposes and if they asked you their value, they are worth around 50 francs each, or around 7 kroner each. It seems to me that they can't ask you a significantly large customs duty for items of such small value.

Concerning your request for cancerous blood, I'll deal with it right off, and if I can secure it, I'll make the necessary shipment right away. This won't perhaps be immediately, because I'll have to wait for a favorable occasion but I hope to succeed in this.

Concerning the clarification that you specify of your theory in its opposition with the two fundamental tendencies of Freud, I know them perfectly, and you only have to refer to the last part of my Pontigny lecture to see that I pointed it out expressly myself in this explanation.

But it seemed to me that in such a short resume, it hasn't the place at all to explain it, without being able to develop an opposition which rests on such technical details, at the very least as the theory was only placed there to serve as an opening to the experiments and to explain the meaning of them 'grosso modo' at the very least still since it could on the contrary be profitable to place under Freud's mark, that is to say of a classical theory, the explanation of a new theory. At the very least finally, in my opinion, there is especially between the two theories, considered from the point of view of their biogenetic result, no essential contradiction other than an interpretation of the primary causes. In other words, it seems to me that the Freudian interpretation and yours, if you interpret them broadly are not contradictory but complete each other concerning the genesis of living matter. Of course, I don't see any contradiction to a translation made by yourself and which thus would be certain not to misrepresent you, which would give even a bit more detail, about the part which belongs to you and your role in the origin of this theory.

On this subject, you will notice, moreover, that, if I had really interpreted the anguish in the Freudian sense of the death wish, it would have been impossible to develop the theory. The very fact that I put forward the electrical charge and discharge, as well as the movement of liquids, entails and necessarily implies your interpretation. It is no less true that the first idea of an alternation, of a change in directions in the tendency, is a Freudian idea which was good to place at the front of an explanation, to give it in some way a classical parentage.

I'll continue to keep you abreast of everything that concerns our common work. With our friends, with yourself, I am always, cordially, RDT

PS I would be surprised if the 's' is of *Bacillus pyocyaneus*, in fact, if we now for this bacillus three pigments: blue, fluorescent green, and greenish yellow becoming red upon oxidation; it is the pyocyanic alone, that is to say, the blue pigment, which is characteristic of the species.]

[German: I am answering your last three letters with a single letter. The work at the beginning of the semester keeps me quite busy and forces me to move brevity, which you will surely forgive.

Your letter from Oct. 15, in which you reported that the bions-6 were similar to the typical types and were, as usual, immediately cultivable, made me very happy. That provides a very encouraging confirmation of our respective methods.

I was even more thrilled over all of the micro-biological information you gave me based on your research and experiments with bions. By the way, you confirm the same ones that I have obtained myself, in particular the two types of bions that appear according to their nature of origin. Of note, though, is that the short rods generally appear quite late. But the egg-shaped ones also first appear in the first culture after several days: the initial shape in the pure culture as well as in the original mixture is circular.

This information from you is completely in accordance with the wishes that I had mentioned to you regarding the necessity of delving into the micro-biological specialist areas deeper than we ever have before. I am naturally aware that only you in your capacity as a physician have a deep knowledge of biology and that only you alone have the possibility to realize the elements of synthesis that we are presenting to the world of academia today. However, by listening to discussions of very honorable and famous physicians with micro-biologists, I could convince myself that the difference between micro-biology and medicine-and even my own medical knowledge -is as great as, for example, the difference between the study of sociology and history. Because I must now conference with bacteriologists, I asked you to help me translate the general biological or medical terminology that we introduced into their language as extensively as possible. You understood that very well, and I thank you for the valuable particulars that you shared with me and which I lacked up to now in as much as I could not delve into them myself. I totally agree with you that as soon as it is possible to do so, collaboration with a bacteriologist would be very valuable and could greatly simplify our tasks. As far as I am concerned, the first worry I would have should I receive the assistance from the French government for which I applied, to secure such a collaboration; otherwise I would either be condemned ((sous peine d'être condamné)) to come to a halt, or I could not discuss the different objections that people would make to me.

On the other hand, the general biological phenomena in the area of micro-biology have their technical effects. Once we are completely familiar with them, productive corroboration will be possible and new additional weapons will be created with which we can implement our theories.

Of course I will gladly accept your offer to send me copies of the films from the charcoal, moss, and other experiments that you are currently producing. Please send them soon enough so that I can thoroughly review them and prepare the dialog ahead of time. I will then decide for myself whether it is a good idea to present them at the same time as the others, or whether it would be better to allow the viewer more time to digest the questions by dividing them up in separate showings. Indeed, when I attempt to explain a theory based on the antagonism from lecithin and cholesterol, potassium and calcium, I am in danger of weakening the effect if I show experiments using materials that are not being discussed. I have to play the role of arbitrator between you and the academic world; otherwise something will go wrong, and we will falter.

If I understood you correctly, some days ago you were advised by the customs office that the syclo-tubes are being held

there. I can do nothing to expedite their arrival. According to the papers, in case someone should ask, each of them is worth about 50 francs, around 7 kroner per item. I think that they will not demand a significant import fee for such a low value.

In regards to your question about cancerous blood, I will get right on it and send it to you should I be able to obtain some. That will probably not happen right away, because I have to await a favorable opportunity. I do hope, though, that I will succeed.

I am very well familiar with the specific character of the theory in your contradiction to Freud's two basic tendencies. You only have to refer to the last parts of my report from Pontigny to see that I specifically emphasized this in the presentation. I found that such a short resume was not the place to present the contradiction without time to develop it, especially one based on such technical details, much less that the theory was only an introduction there used to explain the meaning in very general terms; even less, as it could, on the other hand, be advantageous for the presentation of a new theory to take place under the auspices of a classical theory such as Freud's'; and finally even less so because, in my opinion, there is no significant disaccord between these two theories when seen from the perspective of bio-genetic results other than in the interpretation of the basic, underlying causes. In other words, it appears to me that, given a broad interpretation, your interpretation and that of Freud do not contradict one another regarding the genesis of living matter, rather, complete one another. I naturally have no qualms about adding more details using a translation you obtained without betraying you to re-establish which essential parts of the theories belong to you and which to Freud.

For this purpose, you must take into consideration that it would have been impossible for me to develop the theory had I actually declared that fear in the Freudian sense came from a death wish. Alone the fact that I mentioned the electrical charges and discharges in addition to fluid movements suggests your interpretation, even connects it. It is no less a fact that the first step towards accepting alternatives, a change of view in trends, is a Freudian idea, which would be well positioned at the top of a report and would give it a sort of classical sponsorship.

I will continue to keep you up-to-date about everything regarding our mutual work. PS I would be astonished if it dealt with *S* and *Bacillus pyocyaneus*—indeed when one knows three pigments of this bacillus: the blue, the florescent green, and the greenish-yellow that turns red through oxidation, it is alone the pyocyanin, the blue pigment which is characteristic for the species/type.

I had the visit from Bauer, whom I greeted very coolly. I am telling you this because it appears to me that he intends to interfere too much in your affairs again. This is for everything that he wrote you. I told him nothing, or almost nothing, and believe that I thoroughly agree with you on this issue.

P.S. I just received your letter from 11. I intentionally 'stylized' ((?—pretty unreadable in the manuscript)) and 'simplified' your writings for an audience that is only familiar with the biological part by placing myself 'over' the functional viewpoints ((?--difficult to read)). I approach your interpretation even more in my major report.]

26 Oct., Reich to DuTeil (box 10, DuTeil flds, 2 pp*)** I would like to quickly thank you today for your last letter, and delay discussion about the many, new methodological, scientific findings in so many areas and their natural philosophical ramifications until I can find more quiet time to consider your arguments and address them. Most of all, to secure the productivity of our discussions in the interest of our mutual work, I would like to take as a basis a detailed explanation of the uniqueness of sexology, specifically the sexual economy. I am not certain about many of the methods, but one thing I do know: as the process of Life itself, the sexual process absolutely affects everything but everything that we study, in each area. It changes the location from time to time. I do not know exactly what I need to know for discussions with specialists. Therefore, I completely agree that you carry out the role of "intermediary" as well as your main roles as a friend, advisor, and colleague. I may add: Your objective collaboration in the bion experiments is more important to me than your connection to the world of specialists, as happy as I am that you fulfill that role in such an unsurpassable manner. But I will persist with my concern that sooner or later, here or there, something will go wrong.

And now to the matter at hand: I can share the joyous news that yesterday, for the first time, I was able to observe *S* bacillus in the blood of a woman with an ovarian carcinoma, who had been operated on six days ago. There were 1-6 rods in each section of fresh blood. They grew in droves within a few hours. That is the point from J, a long row of complicated experiments. All laboratory work is now devoted to working out the all-round, basic findings. I will have the first preliminary reports to you shortly. They concern, for the time being, only the methods of growing and evaluating the *S* bacillus. I obtained it up to now from four completely different sides always using the same method. I ask you now to please write and tell me whether you have enough time and interest to complete the controls in your laboratory in conjunction with a friendly specialist. The breeding technology is somewhat complicated. It will, for the most part, be dealing with growths from human blood and the proof of their carcinogenic effect of producing tumors in mice when given in specific doses. I can already share with you that I have collected sufficient evidence regarding all forms of the effects of hydrocarbons in the sense of carcinomas from the literature that the *S* bacillus here is specific. I am only awaiting some samples from other cancer-stricken patients before I report these facts; naturally only to you at first. I will leave it completely up to you whether and how far you wish to make practical use of this. The complete analysis, especially the serological, will require a long time. Day before yesterday I conferred with Professor Lindemann, the local colloid chemist, who is being treated by a colleague and was very interested. I showed him the incandescent soot experiment and the teeming earth. Through this I achieved that our chemical lab worker can work with him at length to learn this method, which is necessary for the chemical analysis. That will take some time.

I only need your consent that you will have a brief, preliminary report ready shortly in order to provide a short synopsis as far as things are certain. [this is what I think he meant] You have no idea how much luck [fun???) I had with this task.

Your syclo is still not here. I have also not received a notice from the customs office. Could you investigate this? We completed your 3rd trial here again but, as it turned out, not correctly. (I was not involved). As soon as the syclo arrives, we will repeat the trial again. I ask you to tell me exactly whether you used blood-agar or dried blood powder. It was not clear, but is definitely differently located [???) I think he means: It was not completely clear, but definitely turns out differently.]

23 Oct., Bonnet to DuTeil (see *Beyond* p. 155)

1 Nov., Reich to DuTeil (box 10, DuTeil flds) [I will write you only very briefly today. I am in the middle of the experiments to verify the various S samples. I wrote you that I noticed them for the first time in the blood of a freshly operated cancer patient, two hours after the blood was drawn. This obligates to work with extreme caution and exactness. My assistants, who only do the implementation tasks, do not understand more of what is going on there. It isn't easy for them to follow because I work alone in peace in my private laboratory. Furthermore, your third experiment with pure KCl caused quite a stir. One of the assistants was tasked with repeating the experiment following your instructions exactly and got no results. I checked in detail what he had done. It turned out that instead of using blood power, he had used blood agar. That is something completely different. Please do the following for me: Send me, please, several tubes of the sterilized blood sample that you used.

The same goes for the bouillon and KCl. I am convinced that you were right. I will repeat the experiment as many times as needed until it is successful. When you do the incandescent soot experiments, please note that, should a robust growth fail to appear on the fresh egg-medium, and then watch for the small, fine mounds. Smear the small mounds onto the same culture medium using an incandescent platinum wire. Let it rest until it grows, and then inoculate for the first time onto blood agar, where the blue-grayish culture grows most beautifully. Have you received any word about the whereabouts of the syclo-apparatus? We are anxiously awaiting it.

Lindemann promised to help me with the chemical identification of cyanide or hydrogen-cyanide in the S. I hope it goes smoothly.

Here at the moment we have a critical situation. The old psychiatrist [Ragnar] Vogt wildly attacked psychoanalysis and especially me. There will be a sharp response from Schjelderup and Raknes in two weeks; also some of the socialists in the Worker's Party are running amok, and added to this is the stressful work with the question about the nature and effect of S!!!]

5 Nov., DuTeil secretary Pomproynon? to Reich (box 10, DuTeil flds) "M. DuTeil is very ill [grippé] right now, but he wanted me to tell you he received your letter."

9 Nov., Reich to DuTeil, with 17 Nov PS and 15 Nov. second letter (box 10, DuTeil flds, 5pp total***, I wish to thank you for the news that you had your secretary give me. I hope that you have already recovered from the flu. It was really not so serious?

Today I would like to now report a fraction of the very exciting and turbulent cancer work that overwhelms me. The issue has a uncanny logic. It is up to me to highlight it amongst the many other experimental phenomenon. Everything gradually aligns with my dialectic-materialistic evaluation of the processes of cancer disease. I write down each group of phenomenon as they are clarified. This comprises a manuscript that exactly describes developments and connections.

At the end of every passage, I can determine what I know and what I still do not know. It is embarrassing yet unavoidable that this task may be destroyed and even my existence seriously threatened through narrow-minded, medieval attacks by patriarchal academics and eugenicist deeply worried about their intellectual existence. I will send you a copy of the manuscript as soon as it is edited and as soon as I hear back from you that you will officially accept this report.

As of now, I have successfully clarified the following:

Bions that are indistinguishable from the blue soot bions can be produced from human blood in one, but only in one specific way. These robust blood bions came from the blood of two women operated on due to cancer.

In both women, I could see S-bacilli immediately after drawing off the blood and breed them pure.

I also got the S-bacilli (charged, with Gram stain, macro-and-microscopic) verified up to now from:

Cancer tissue from a cadaver.

Surgical cancer tissue

Blood from cancer patients

Blood from mice coated with tar

Blood from mice that had been coated with tar and died.

Finally, and this is conclusive, from the blood of healthy people by isolating a degenerative growth on blood bions. All people can get cancer!!!

Animal trials showed:

The blood bions from cancer patients do not harm mice. On the contrary, they kill the S regardless of origin within 24 hours or they produce tumors after an extended period of time. However, if one inoculates with cancer bions from mouse blood and afterwards with a strong, usually fatal doses of S, the mice do not die. I must still complete experiments to establish whether they will still produce tumors and whether these tumors will heal after further bion inoculations.

I believe that the S-rods are produced after days and weeks from blood bions like the rods in the six preparations of the 6-bions.

The S-bacillus is connected with the destruction of red blood cells. Was that already too much? I do not wish to overwhelm or discourage you. The fact described here is absolutely verified. With the help of some friends, I will now concentrate everything on the assurance of a possible early cancer diagnosis using blood. It is a certainty that the cancer tumors do not represent the disease itself, rather represents a reaction of the body to the attack by S. The S-bacillus is derived from the body's own organism through self-accelerating decomposition.

You gave me an idea for a second experiment using blood charcoal to grow bions. It works very well. Please blow cigarette smoke on it without infecting it, even through cotton, into a) potassium chloride, b) bouillon + potassium chloride! At times it creates very pretty, pure slate-blue carbon bions.

I ask that you officially accept my report concerning the S-bacillus, but I also plead with you to refrain from changing anything or discussing it until my completed report is submitted.

Our bion publication is close to being published. Should you wish to review it prior to this, please write me. I believe it will be very good and in-depth. Hopefully the colleagues in Paris will find the patience necessary to see what must be seen. Positive results would mean a giant leap forwards and solely to your credit. Negative results would certainly mean a small catastrophe for me, especially with the situation as it is here.

When it can no longer continue here, I will relocate to France together with my entire laboratory and a few assistants. However, that would require positive results in the control experiments. I am not worried about the final recognition later, rather only that my currently very strained experimental work could experience a lengthy interruption and be endangered.

I know that I can easily share my worries with you. It feels good to do so at times.

How is your work going? Is your laboratory already set up? Please attempt to make bion mixture 6 using blood charcoal and bouillon then inoculate it first onto freshly made egg medium. One gets very nice results. We cannot replicate your potassium chloride experiments as long as we do not have the cyclo equipment. Have you heard anything about this? Where is the apparatus? Somewhere in a completely witless customs office?

PS Nov. 17 "I did not immediately mail this letter because I was very irritated over the current situation. I would now like to cordially thank you for the work, "From Psychoanalysis to Biogenesis". You have brilliantly managed to comprehensively depict what we call synthesis. I have read the German translation of your manuscript. I would like to publish it. However, I would suggest that we don't include it in the book on the bion experiments that is coming out now, because it is already completed, rather in the January issue of our magazine. I would like your permission to do so. Because our magazine is circulated world-wide, you would be rendering a service to both our issue and to yourself. I consider your work well suited to introduce readers to the overall problem, especially the less informed.

More and more the results concerning the blood bion-and-S-trials are taking on the character of inviolable certainty. I will leave the sentence "Regarding the Development of Vegetative Life" stand in my work and then publish a book about the preliminary investigations of cancer issues. I would very much like to hear from you how the issue on the Sorbonne turned out.]

15 Nov, Reich second letter to DuTeil "Thank you very much for the message via your secretary.

I assume that you are staying in accordance with the plan on November 17 in Paris and wish you every success from the heart. I ask you much about because it is extremely important for our work here, then to return to us a detailed report on the operations and discussions with protocols if possible.

For the demonstration, which you should take in case of Dr. Monod, I would like to announce a follow shortly: I do now even with the carbon test Carbo sanguinis, ie, blood charcoal. The still significantly better, faster and clearer results are as Glühruss. The procedure is exactly the same as in incandescent soot. I send you with the same post a small consignment C.S. The reconciliation is as far from broth + KCl on egg medium on blood agar. I do not know if you want to make this attempt. I consider myself strictly because I use not only broth, but broth + fresh egg medium.

That would be all I should write to you today. I am preparing a very long and detailed notice to you before about our new work, but I would not complicate the course of your work in France at the moment by further experiments. I assume that you are in agreement."

17 Nov., DuTeil to Reich (box 10, DuTeil flds, 1 p***) I have been too sick and tired to write you for some time. Also, work from the restart of the academic year has taken up my time. Finally, I also had to prepare for my second trip to Paris where I will

be for a few days. You'll find here the invitation sheets for the lecture I'm giving tomorrow evening at the Sorbonne. I hope it will have the impact that you want. The text is about like the one I sent you at the time of Pontigny with this difference: that I simplified and highlighted the synthesis and the consequences of a general biological order that I had already drawn from it. I will communicate these to you when the synthesis is entirely verified.

I cannot therefore write you at length today, nor give you the details about the experiments undertaken in Paris last month and that are still in progress. Having given contradictory results. It is necessary to neglect those that are negative in order to concentrate on those which are positive, exclusively. In any case, tomorrow I shall especially insist on the synthesis itself indicating the experiments as a verification in progress of this synthesis. That is the strongest position.

I received the proofs you sent [of *Die Bione*] and am amazed at the abundance of the space you so generously devote to my modest activity. I think it will nevertheless be useful, even essential, to add my lecture of today, which shows considerable progress over the others.

I stop now in order to pack my suitcase. Remember me to all our friends, and believe that I am always your devoted RDT.]

24 Nov., DuTeil to Reich (corresp box 10, DuTeil flds***, [My dear friend, I don't have the time today to write you at any length and especially reply to your so interesting letter of the 9th and the 17th of November. All that you tell me on the subject of cancer is noted, and I'll reply to you on this subject.

I only wanted to tell you today that the anticipated Lecture took place at the Sorbonne as agreed, and it was a great success. The hall was three quarters full, and there were a great number of doctors and biologists. On leaving, I received congratulations from numerous listeners who gave me their cards and with whom I am now going to be in contact.

I made the most of my visit by starting a third series of experiments at Leopold Bellan Hospital with the bacteriologist Pretet, a very open man and very favorably disposed toward us.

Let's wait for the results which will be added to those already underway. I can't tell you anything except that the whole thing is evolving favorably. But you probably understand that the official position of these scientists obligates them to great circumspection before allowing the results to become officially published. It is necessary for them to have a great number of tests, and it is only on a statistical basis that they will agree to commit themselves.

I am glad that the synthesis which I was able to draw, set up and present to France pleased you. It was only possible on condition of dropping many details, of 'stylizing' as we say in France. On the other hand, under this form, it causes the experiments themselves to pass, if not on the second level, at least in the level of verification. It will thus contribute a lot to the security of future progress. I stop for today sending to you as well as to all our friends my most affectionate sentiments. RDT.]

I do not have the time today to write you a long letter or to answer your so interesting letters from Nov. 9 and 17. I have absorbed everything that you told me about the cancer problem and will respond to this.

Today I only wish to tell you that the planned lecture took place on the Sorbonne, as planned, and that it was very successful. The lecture hall was three-fourths filled, and there were very many physicians and biologists present. Numerous listeners, who gave me their cards and with whom I will now get in touch, congratulated me at the end of the lecture.

I used my stay in Paris to begin a third series of experiments at the Leopold Bellan Hospital with the bacteriologist, Pretet, who is very open and very favorably attuned. We are awaiting the results, which will be added to those that are still running. I cannot tell you more than that the entire thing is developing very well. You will, though, understand that the official position of these different scientists obligates them to be very cautious before they will allow the official publication of the results. They need a large number of trials, and they will only be willing to make one statistical analysis.

I am very happy that the synthesis that I could outline, justify, and present to France pleased you: it was only possible under the condition that many details be omitted—through 'stylization,' as we say in France. But in return, this allows the experiments, while not secondary, appear simply as verifications: this way they will greatly help us in guaranteeing future progress.

30 Nov., Reich to DuTeil (box 10, DuTeil flds***) "Ihr Brief kam gerade recht mitten in eine Reihe ganz grosser Schwierigkeiten. [Your letter came right in the middle of a series of very great difficulties. I already wrote you, that a very old psychiatrist and eugenicist is running amok here against us. *Our friends hit good scientific scientists foggy for their convictions* [literal translation]-what I think this means: *Our friends are striking out at him armed with scientific convictions.*] Youth is for the most part on our side. The positive news from France was, thus, very valuable. Other than that, I am waiting patiently, and I can wait peacefully, for in the middle of these atrocities, the experimental tasks are progressing fabulously. I will write you only briefly today to facilitate the work in France with further details of my bion experiments: 1. Bion mixture 6 is now being made with the following changes with wonderful results: No milk in the mixture because the eventual development of lactic acid could

be detrimental.

On the other hand, a bit of bouillon is put in. Instead of charcoal or soot, I now use incandescent or swollen blood-charcoal. I only make the mixture now with **KCl**, not with Ringer. The Ringer substances are in the bouillon. The first inoculation is onto bouillon and KCl parts. The first re-inoculation [is] onto fresh egg-medium, no longer onto bouillon. Pure, gray-bluish mounds form. These are then smeared onto the same egg-medium and left standing until a thick growth appears. Now do the first re-inoculation from this onto blood-agar. I promptly get the loveliest pure bion culture from pure bions.

2. Each of the preparation prepared with incandescent blood carbon results in the typical blue-gray growths.

3. I get the same blue-gray carbon-blood bions from cancerous blood. [BLUKO bions] Tomorrow we are inoculating a patient with incurable cancer with carbon-blood bions obtained from cancerous blood for the first time!!! Will we succeed? You will get new cultures from me. Please, write us in detail!

I understand very well which responsibilities the colleagues making the controls in Paris have, and I will take my time. It would only be advantageous if I knew more concerning eventual problems that exist there. Perhaps I could eliminate one or the other.

All in all, dear friend, I believe that we are on the best path if the small-mindedness and ignorance sets us [does not set us???] a stupid trap somewhere. My heartfelt congratulations to your success in Paris. In the foreseeable future, you will receive copies of a very well done soil-, charcoal-, and soot film. In addition, we have now produced a slow-motion film 'The Genesis of Amoeba.' One can wonderfully observe how the moss decomposes, forms vesicles, which finally crawl away as amoeba.

That's it for today. Hopefully I will soon get your detailed report. Our book will be published in about two to three weeks with about thirty tables. It contains 250 pages, and it is only a preliminary report!"

1 Dec., (OI box 8, Präparatherstellung und Protokoll" fld) "Vorläufige Mitteilung über die Herstellung von sterilen Menschenblut-Bionen" **RECIPE FOR PREPARING BLOOD BIONS**

3 Dec., (OI box 8, "Protocol, 3.12.37" fld) "Diktat 3.12.37

After the introduction of autoclaving the freshly extracted blood for the preparation of bions, I began autoclaving the bions cultures, too, for added surety. The first autoclaving was from the 10 Ca II culture swollen for half an hour in a physiological salt solution. Examination of the autoclaved swollen matter showed a distinct change in the microscopic image. It was no longer a pure bion culture with its typical structure, movement, and color; rather I had the image of a mixed culture in front of me. Although the solution was evenly colloid, it was microscopically overcome by rods with two lobes like the transitional forms of S and egg-shaped cocci, which were far smaller than the remaining bions. This swollen matter was then cultivated over egg medium onto agar using the normal procedure. An examination of the culture showed, to my greatest surprise, no change in the picture as far as a correct pure bions culture, rather likewise a mixed culture of bions, S-type rods, and small, egg-shaped, quickly moving cocci. Now the unavoidable question of how it was possible to get a different result using a pure, autoclaved bion culture needed to be answered. My first assumption was that the bions were biological weakened by autoclaving them, and the biologically weakened bions created degenerative structures in the cultures. It now seemed clear to me that autoclaving blood supported the production of bions and eventually killed pathogenic germs on the one side, while on the other side autoclaving cultures should be strictly avoided because that seemed to produce degenerative structures again. The fact that biologically weakened bions produced degenerative microorganisms raised the question whether or not poor cellular respiration in the tissue of cancer-stricken patients is one of the main causes for the degenerative breakdown of tissue, as with the blood bions, and the creation of toxic, perilous organisms.

Reich to DuTeil, 18 Dec. 37 (*Beyond*, p. 130-32) Reich discusses the powerful monetary interests (e.g., in radium business) that will oppose his discoveries about cancer once they become public. "I am writing to you today on an extremely important matter that will undoubtedly have a quite decisive influence on our work. I must ask for maximum discretion. From the reports that I have sent you on the production of the S-bions and the blood charcoal bions you will probably have seen that, as was anticipated one and a half years ago, the work has progressed quite logically. Much more progress and much more complicated events have taken place here than I informed you about. The first cancer-therapy tests have been initiated with the terminal female patient about whom I have already written to you. For the time being, things are going well, but it is not possible to say anything definite yet. I assure you, without having to fear that you will regard me as someone who acts and thinks precipitously, that I now have 100 percent control over the methods of determining the disposition to cancer long before it manifests itself in the form of a tumor. To carry out the experiments on the therapeutic side, I would now need at least 200,000 Norwegian kroner and well-trained medical personnel. Ich kann Ihnen das hier nicht begründen, sondern nur im Vertrauen auf Ihre Freundschaft mitteilen.

A short while ago, my friend Sigurd Hoel drew my attention to the enormous danger that looms from the various trusts set up to manufacture and sell radium for the treatment of cancer. I do not know to what extent you are aware of the practices

and the brutal determination of the representatives of these trusts and of the people who profit from them. After vigorous and detailed discussions of this matter with our friends here, we realize that if my method of cancer prognosis and possibly also my method of cancer treatment were to succeed, it would completely pull the rug out from under the cancer radium industry. Everyone who lives off that industry, and there are tens of thousands of people all around the world who do, would turn against me. Not because they are bad people but simply because they would fight quite unconsciously for their existence and against the need to undergo difficult retraining. The most dangerous reaction of all would come from those people whose vast earnings would be jeopardized. (That was not my idea, it came from a very smart and foresighted person.) In addition, I learned yesterday that some years ago a journal article critical of the Belgian Radium Trust appeared. The article claimed that prices for radium were kept extremely high by the trusts and that profits of up to 1000 percent were being earned. I cannot check these matters, but to the extent that I know 'business,' they are probably correct. Unfortunately, I did not have an opportunity to discuss this side of our work while you were here—namely, how much scientific research is hampered by the profits derived from its incompleteness. Think, for example, merely of Davos, which exists only because tuberculosis cannot be cured. It would make me happy if you fully understood what I am trying to say. It is not an ideological anticapitalist or an ex-communist speaking in me but a physician who, under all circumstances, clings to his function and does not wish to be destroyed in the process. Because I am usually very naïve and these facts had to be pointed out to me, it would be wrong to claim that my behavior is paranoid. To put it briefly, I ask you to observe the utmost caution in whatever reports you make to anybody about my ongoing cancer work. In diesem Zusammenhange fiel mir ein, dass das grosse Interesse des Herrn Schraenen, der Sie so rasch und interessiert besucht hat, vielleicht, ich möchte niemandem nahetreten, doch noch andere Gründe gehabt haben könnte. [Google: In this connection, it occurred to me that offend the great interest of the Lord Schraenen, who has visited you as quickly and interested, perhaps, I would like anybody, but may have had other reasons.] You got to know Sigurd Hoel here and I am sure you were persuaded of his absolute intellectual honesty. He believes that the simultaneous existence in Belgium of a league against cancer and of a radium trust cannot be a mere coincidence. Only recently have I understood the reaction of local radium therapists to the initial rumors about my cancer work, although they were unaware of their own motives. My friends here and I still do not see how we will cope with this problem. I would appreciate it very much indeed if you could write and let me know your opinion, without making me wait too long for a reply.

Wir haben überdies bereits Versuche mit Tuberkulosekulturen in Gang gesetzt, die ausserordentlich erfreulich sind. Die Sache ist viel weiter gediehen, als ich es selbst anzunehmen wage.

Es bedarf nur noch eines Nachsatzs zu meiner Mitteilung and Sie über die Blutkohlenstoff-Bione: Sie sind äusserst positiv wirksam, doch nur bei einer bestimmten Art der Kultivierung und der Behandlung. Wir kae vor kurzem darauf, dass sie tödlich wirken, wenn sie bestimmten, bis vor kurzem noch unerkant gebliebenen Prozessen unterworfen sind. Auch darüber werde ich Ihnen demnächst mehr schreiben.

In the deep conviction that you will not misunderstand this letter and that you will assist me with your advice, I remain, yours most sincerely”

27 Dec., DuTeil to Reich (box 10, DuTeil flds, German transl) [I received your long letter about cancer, but I could only scan over it because I have so much to do to prepare for the beginning of a new semester. I will, thus, first be able to answer you in a few days when I've had time to calmly look it over and think through the purpose and the consequences of the advice and opinions, which you had the goodness to ask of me, for this deals with a very important issue. In the first place, it seems necessary to me to guard against any hurry in this matter where things progress so slowly that the danger of becoming outdated is practically zero. Bear in mind that the B.C.G.-vaccine was experimented on for ten years before it was announced and deemed suitable for introduction into praxis. On the other hand, I am awaiting a visit from the Dean at Marseille's School of Medicine, who is a very renowned cancer researcher and who recently gave a lecture about cancer, specifically experimental cancer, at the Central University in Nice. I have seen several pictures of this on film.

I am thinking of telling him about our work, and it would well be worth it, because he has already heard talk of this and had immediately recognized the bions under the microscope. This proves that medical journals have already spoken about it.

Of course I will keep you updated, also concerning articles that have been published in the French newspapers or will be published. You know as well as I how much patience one must have. My best holiday wishes to you as well as Mrs. Reich and all of your colleagues.”]

7 Jan. 38, DuTeil to Havrevold (box 10, DuTeil flds, 4 pp.****) I am delighted to receive your direct and personal update because it is now possible for me to tell you above all how much true congeniality I have felt towards you since the first time we met and how much I was drawn to your methodology and easy, profound deliberations. Since that moment, to know that you are on the side of our great friend, Reich, has always meant an element of security for me, and I am glad that I can now reveal it to you.

The disquieting news that you shared with me makes me sad, but it doesn't surprise me. In general, the price of discovery is persecution, which threatens the famous who prevail and people from title and position. I believe this is not the first time that our friend has had to bear such treatment.

The attacks that you cite are not new and, likewise, neither are a part of the arguments you reported. All psychoanalysts, without exception, have had to deal with accusations from the moral middle-class that they abuse their female patients. The necessary occurrence of transfer and the neurotic character of the ill continuously provide new material for these accusations, and I do not believe that one can seriously dispute with you about them.

As far as the experiments and their value are concerned, I will, if you so wish, examine two points here in succession.

First the practical side of the issue: I can assure you without hesitation that my answer to the Director of Medical Administration, should he write, will be the meticulous and exacting truth to guarantee all possibilities. It seems to me impossible and outrageous that anyone could even think of expelling a scientist under the pretext that the laboratory experiments with which he is occupied have been contested. Pasteur's experiments were disputed for more than 20 years, and no one had ever considered banishing him from France. My local experiments are discussed with great passion with some being pro and some being con. But alone the idea that I could be harassed because of biological experiments here in France would be absolutely insane. And this brings me to the second point that I wish to address.

I have told my friend, Reich, again and again both verbally in Oslo as well as in letters to him since my return home about my concerns regarding his overall espoused attitude and the methods he uses to distribute his reports. I already pointed out the dangers of mixing general policy with science, and this in such a systematic way, to him in Oslo. And although I agree with him on most issues, I have always protested against mixing the two areas, and, on my part, refused that type of commingling. He later wrote me that he had, in the meantime, given up that method and intended to henceforth retreat to pure science. However, one may not forget that he has already written a lot about the other issue, and that he is the official initiator of a publication that epitomizes to the entire world the systematic destruction of accepted, general morality and society. It does not matter which view one takes about this matter, one must recognize that that is the fact and that he started the offense. It is daring, and I always admire his courage, but it brings with it the risk of an unequal material battle. It would only be surprising if society would not defend itself. The way it does it is probably not exactly correct, but war is war, and I see hardly a means in this [entire line cut off at bottom of letter] Reich's methods in this scientific area [are] also full of risks, namely full of deliberately sought-after risks.

One matter is namely very illustrative: While Reich is exposed to unanimous attack in Oslo, I have been accommodated in the best possible manner doing the same work in France, and that is alone because of the way in which my approach differs from his. In only a few weeks, I have been greeted with enthusiasm and was applauded in Pontigny by the greatest scientists, at the Sorbonne, in Nice, and at the Academy of Scientific Ethics. And I have achieved applause for Reich's name. My method of presentation is, thus, better. That is clear.

I naturally must be extremely careful. The emphasis that Reich puts on the experiments themselves, perceived by him as the focal points of the issue, which is exactly the thing that could upset people the most—this emphasis I have placed on a general theory, a synthesis transfer, for which the experiments, in spite of their importance, only mean a verification of a specific point, as if biogenesis would be merely of secondary importance to us. The world accepted the announcement in this form. I did not pose as an enemy of Pasteur, rather as the continuation of Pasteur, who had been too narrowly interpreted up to now, and even then I got everyone on my side. I have avoided denying facts that are still generally accepted, such as Brownian Movement. Such a denial would have alienated the entire world. On the contrary, I have specifically stated that we intentionally added these movements as one of the defining elements of Life. By the way, I find myself in agreement with Kraus in this regard, who represents the opinion that the movements are not the result, rather the cause of Life, etc. In short, I strove to specifically use all of the old truths as support in order to build a new theory into the old ones, or rather vice versa. That was successful to such degree, that a newspaper article compared my synthesis with that of Louis de Broglie's in the past weeks.

Reich proceeds the other way around. He continuously repudiates everything that was known before and, what is especially of consequence, he does it even there where it is unnecessary. To deny Brownian Movement means, for example, to intentionally and unnecessarily shock all physicists; this gives them the impression that one has only superficially examined the question. The theory is just as original when one acknowledges this movement and even more so when one applies it. In short, Reich's impersonal and sometimes aggressive behavior is, in my opinion, the reason he has provoked animosity. Added to this is the combative behavior he has embraced in the social part of his endeavors.

A third point: I have drawn his attention to how indispensable it has become to have a specialist in bacteriology, a field that is complicated in its minutiae and where the facts of the case only manifest themselves via the second, third, fourth result, when delving in further individual applications. Reich and the bacteriologists do not speak the same language. One speaks in general terms and sees from above a whole. The others refer to underlying particulars. By systematically neglecting this, he gives the impression that he knows nothing about it.

You will recognize the letters in which I have emphatically pointed out this fact. I, myself, have had huge problems of this kind with bacteriological specialists. We have here a further reason for the misunderstandings that are handicapping our friend today.

And finally: Reich displays a much too impatient nature in this issue. Under the auspices of having discovered a world

with a single glance, he demands that everyone else recognizes and understands this as quickly as he, and this demand is in itself contradictory. There is no letter in which I was not forced to stifle his haste, and I still had to remind him in the last letter that certain vaccines or serums—even most of them—had to be tested more than ten years long in the quiet of the laboratories before they were announced or even released for public usage. By the way, I have the impression that it is the necessity of stabilizing his financial situation that drives him to seek an immediate, resounding success. I consider it extremely pernicious when material issues affect the course of the experiments by causing haste. I can admit to you that I am going through great fiscal problems myself at the present. But I have resolutely stopped the experimental part until I am able to sufficiently ensure it again. And when I am asked how far I have gotten, I answer that it will be six months to a year before I will be able to give any useable information. The people are then—and rightly so—convinced of the seriousness of my work.

All in all, I have the impression that our friend must reconsider three principle issues, namely: his political and social, or more like anti-social behavior (regarding the current form of society), a too intransigent manner of breaking with all of the present-day knowledge when presenting his discoveries, and an exaggerated urgency.

Even if this helps us to understand how such a campaign could get so unleashed, it does not, in my view, excuse his way of proceeding. On the other hand, even if Reich would recognize our diagnosis as valid, could he be able to change his behavior? I do not believe so. His temperament is as it is, and one must accept him as he is. Such persons are destined to be simultaneously idolized by some while being burned [at the stake] by others. Indifference or torpidity regarding such people is impossible. And I am very much afraid that there is no way to heal this, for the very same conditions would appear again in any other country. That is also true for France, where for the moment, as I have said, can only progress—and in a very clear manner—using an approach that is the exact opposite of the one our friend uses.

And, in closing, I would like to share with you a situation from the other side which backs up and reinforces what I have told you here. If I have not yet shared the results of the control experiments started in Paris with Reich, then it is because the researchers there demanded I give them my word that I would keep the results of which I have knowledge confidential because they (as did you) reached contradictory results, which indicates a certain proportion of successes and failures. They do not wish to commit themselves to a hasty conclusion until they can, after several months, build a statistical foundation under their research. I believe that they are right, and that this is the true, scientific stance: utmost caution when dealing with such an important issue that obligates official responsibility.

Those, dear Doctor, are the thoughts which your letter provoked. They will naturally have no influence on the amiable actions that I will exert to defend my friend during the inquiry you announced should the opportunity arise, even if I must provoke this so necessary opportunity. Recognizing the cause of an ill does not mean that a quick intervention to prevent disagreements should be hindered, and that is what I intend to do; however, it would also be necessary for our friend to help us and not unnecessarily and dangerously complicate our work. I am depending on you under these circumstances to guide and support him.

Tell our friends that my loyalty is towards our great Reich and that they should rest assured of this and not hesitate to count on me to help in any way possible.]

10 Jan., Reich to DuTeil (box 10, DuTeil flds) [I too must be very brief today. I am now working here on a huge scale and it is becoming complicated.

First of all, I would like to wish you a happy and personally successful 1938, both in my name and in that of all my Scandinavian colleagues. May the magnificent working relationship continue to develop and reach its goal!

You will be glad to hear that I could successfully cultivate a new type of bion over the past weeks. You will receive the notification soon.

Thanks to Havrevold, the blood tests on those stricken with cancer- and lung disease are assured through an official clinic.

The insane rampage has died down. I simply kept quiet, and allowed the heredity fanatics to demonstrate their service to Hitler until they were exhausted and made fools of themselves.

S-similar structures were found in your bio preparations that I am continuing here. I have the suspicion that the S have unusual connections to death, to degeneration as a whole. But, please, this is just an idea of mine.

I have to ask you once again to please let me know about the location of the Sy-clo. I desperately need it. Please, let me know!

I have now studied your potassium experiments again in detail and continue to work with them. You are surely aware that potassium displays atomic radioactive disintegration. *I believe I am on the track of understanding your experiments.* Not any longer, as the potassium-chloride crystals that were dried through boiling and immediately thereafter moistened again with NaCl no longer showed smooth crystals, rather uniform, moving structures at a magnification of 4500x. That is very exciting and appears to me to be very meaningful. I observed it yesterday and am writing today because of it.

I would be very happy to hear of any progress in your work. Do you even have time for it? Was there something new?

One thing more: I have grown S from further cancer tissue. Our publication will appear in the coming weeks. You will immediately get copies. I am now reading a story about radium from Rudolf Brunngraben. After that, I almost wanted to keep the book about bions away from the public eye. It is unbelievable what happens to the findings of industrious researchers in today's world.]

28 Jan, Reich to Lapique (box 10, DuTeil flds, copy on back of 5 Feb. Reich letter to DuTeil) I hasten to reply to your letter of 25.I. The publication of an extract from my preliminary communication of 8.I.1937 in the form in which you propose, I agree with you. Since the first detailed report on the Bionversuche and the cultivation soon appear next to your message of confirmation would hold the date of the first preliminary report. I am extremely sorry that you have been waiting for the GFilme [6 movie?] In vain. The failure of the mission explained by the fact that soon after initiated the sending of the first preparations Prof. Duteil in Nice, the control experiments at the university there, and during a visit here in August 1937 two film prints taken for the demonstration in Paris. If you wish, I am willing to make another copy. I beg you send me copies of the published notice.]

End of Jan., bion präparat 12, Präparat SEKO (OI box 8, "Präparatherstellung und Protokoll" fld, ms pp. 50-52, photos 112-114)

2 Feb., Reich to DuTeil (box 10, DuTeil flds) [In the enclosure, I am sending you copies of the letter exchange with Professor Lapique from the Academy's physiological laboratory. I had given my consent to publish the first report from 01.08.37 [8th of Jan.37] because at that time I had no idea that the added interpretations from L. had become outdated through the culture experiments. I first noticed that in the second letter. I canceled [this] with a telegram and immediately wrote the justification. I am asking you to take on the responsibility for this issue because I am not well enough orientated concerning the work of the biological Academy in Paris and do not wish to interfere. Apparently Lapique knows nothing from the other tasks. An interpretation of Brownian Motion from L. would damage my work here in spite of the simultaneous publication of my report. I am beseeching you to not keep me waiting for your answer because I have to finally report the decision to Prof. Lapique. Please notify me the best thing one should do in conjunction with the ongoing control experiments.]

In the meantime, I have produced bion preparation no. 12 in which large, nucleated, gram positive dye able and structured cells were found. They agglutinate S. Next week they/you will get a color preparation. [Don't know if he is telling DuTeil that he (Sie) will get a colored preparation or if the sie [they] is merely capitalized at the beginning of the sentence and Reich is stating that he is going to dye the bions himself during the next week]. Otherwise the tasks are progressing nicely.]

2 Feb., Reich telegram to Lapique (correspondence box 9, "Lapicque" fld) "Ersuche Publikation vorläufig nicht vorzunehmen. STOP Erklärung Brief folgt."

3 Feb. Lapique to Reich, in reply to Reich letter of 28 Jan. (next Lapique fld) "I have your letter of 28 Jan. about my proposal to publish an extract of your report," etc. [Please indicate whether you wish only to extract from your message, or even my observations, there are approximately 18 lines [of type]. I confirm the presence of amoeboid movement in your particles explain these movements but by the molecular motions, such as the Brownian motion.]

5 Feb., Reich longer letter to Lapique to follow up telegram (same fld as Lapique letter of 3 Feb.) [Google: Your second letter of 3 February 1938 prompted me to ask you by telegram, to temporarily withhold the publication of my message, and your interpretation. I was a misunderstanding that is clear, the intention by the publication in the form could be conjured up, a misunderstanding that should be avoided.]

The first communication from the 8.I.37 Bion mixture contains nothing about successful culture experiments. These were only secured the end of February. For Bion research was now for me with the question depends on whether the movements sighted by Brown should be interpreted physically or biologically. I made over the summer after cultures were managed sterile mixtures, experiments with cultivation of incandescent soot. The experiments were made positive. The message is then sent to you about the culture results with soot (and carbo sanguinis) is obsolete. My detailed publication is in next week. May I ask you, if it important, with regard to work since January 1937 with Prof. Roger Duteil on Universitaire Nice to get in touch? He controlled the first cultivation experiments in Nice and then stimulated at several laboratories Parisians further control experiments, which are still in progress. There is just the culturability of the Bion-structures now decide which explains the urgency of my telegram. I write today in this matter Prof. DuTeil. These circumstances that I overlooked in my first pledge to you and trouble caused, I apologize for.]

5 Feb., Reich to DuTeil (box 10, DuTeil flds) I will send a newspaper clipping from yesterday. We had a huge success. The biology professor Margaret [Kristine?] Bonnevie discussed Bions on the occasion of a lecture at the Student Association, in which she held that it is indeed doubtful, but urged the public not to reject the findings on principle only. It does not change that it fears basically the discovery. At the end I was able to quash some rumors the local fascist Profesores had sown, which would have checked my attempts to make public properly. So it is back to operation. But the dangers are not over. Newspapers in Sweden and Denmark have also expressed representations. This is also heavily bought. [This also comes at a

heavy price(?)] Most gratifying is that the Bion research goes splendidly. I have improved many experiments, and the cultures are even more secure. You will soon get a statement about it. In May I will be holding a demonstration of my public spaces with lecture slides, to which I will invite the authorities. We therefore decided to abandon our academic reserve and speak in public. It turns out that the intellectuals of all kinds face the matter sympathetically. I also bear the silence no longer. This [silence] only leaves opponents room to spread rumors.

The other-growing cells, you have probably received. They are curious should grow from moving amoeboid. If someone puts the objection of an infection, so I ask you to assure you that I regard the process of formation of these microscopic cells have come to the track. I was able to follow a specific procedure by which transformation of eggwhite occurs.

Any news? I will send cultures directly for Marseilles when you confirm to me that I should do it. This is all for today.

PS 6.II.38 A silly set of circumstances has revealed himself: "I telegraphed Prof. Lapique to wait to de publication, on Saturday, the 5.II after I had read the handwritten text of his difficult to read French letter. It urged that the matter should go to press on Monday. Today the German translation was given by my assistant. Then I saw to my horror that L exempts me if I wanted his Intepretation or not. Now he has my explanatory letter on Monday and probably will be the interpretation of Brownian motion leave by itself. Or change their information after the culture results and check your results, which I also sent to him.

List included, of "INSTRUCTIONS FOR SY-CLOS-TUBE (Prof. Roger Duteil)

The lecithin mixture enters the tube, A

The mixture KCl-Ringer-coal, etc. in the tube B

(Only very small quantities are used)

The broth comes first in the tube D

The plug on D is then pushed so far into the tube, that it closes the connection to C, so that inoculation does not occur prematurely during autoclaving or during the first manipulation. Then the whole is autoclaved.

Then on the contents of A and B in A and B mixed-best in A.

Inoculation brings a drop of the finished mixture into the previously empty tube C, then pulls the plug on D back slightly so that it releases the connection tube and let the broth bleeding over from D to C. A bit of broth remains in control D.

Now, a cotton ball will be placed in D to prevent infection of the control broth through the air the other tubes.]

7 Feb., Lapique to Reich (correspondence box 9, next Lapique fld) [Sellers transl.: It is certain that the agreement between you and me is very limited. It is a question only of the reality of the 'lifelike' (lebensartig) movements announced in your communication of 8 Jan. 1937. I am ready to confirm the reality, but in interpreting it, under my responsibility, physicochemically and not biologically, by a simple hypothesis, which attributes it to the Brownian movement. Do you want to take advantage of this offer?

I am not at all disposed to go further and to discuss the questions of culturing or artificial culture of the bions, questions which would not be within my technical competence, and which, moreover, it would not interest me to research the determinism and the physicochemical mechanism through your metaphysics.

If you have not made known your acceptance of my offer before two weeks' time, in agreement with your first affirmative reply, in accordance with my preceding letter, I shall present a negative report at the Academy, and your communication will be purely and simply classified."

8 Feb. 38, G. Van Den Areud (President of the Psycho-Biology group, Sorbonne) to DuTeil (box 10, DuTeil flds, sent to Reich, German transl) [google: Only afterwards did I learn from Dr. Allendy that you gave a lecture in Paris on "spontaneous generation. Excuse me, I take the liberty to write you: I am very interested in this question. I am preparing my doctoral thesis (Doctorate-of-letters) in natural philosophy (philosophy of sciences) before with the title: From matter to life. I would be very grateful if you could let me know if there is a documentation of the investigations which you have carried out together with Mr. Reich. Have you published anything? I am very sorry to have not had the pleasure and honor to meet you and I hope that you can not find this letter inappropriate.]

Reich to DuTeil, 9 Feb. (box 10, DuTeil flds) [Regarding my case with Prof. Lapique, today I received the attached letter, which seems quite unfriendly. I had in my investigation of the facts simply highlighted the biological interpretation of Brownian motion in order to orient Prof. Lapique over all. That was my obligation, materially speaking. But the term 'Brownian motion' seems to be more than mere matter. I only had three options. To accept the publication with the biological interpretation of the possibility of negative thoughts Prof. Lapiques to reject it or it harmless. However, as I will engage in no case in the course of the case in France, I think, if you decide otherwise, in about eight days (15 days I have to write margin) that you are using state of the control work as a representative of the thing the Academy to be decided. So I wait 16 days on your decision. The publication in

the proposed form can be positive depending on the state of the control work or be taken negatively. I can not judge here. By the same post I send you a Gram-stained preparation of Bion culture No. 12. I wonder whether such structures are known.]

About this time, Reich begins cancer model expt. (*Beyond* p. 144, 9 April; OI box 8, "Präparatherstellung und Protokoll" fld, ms pp. 57-59, photos 119-121)

12 Feb., DuTeil to Reich (corresp box 10, DuTeil flds) [I respond very briefly (because it's urgent), but precisely to your two letters about the Lapicque matter. My opinion can be summarized in the following alternative:

Either Lapicque published the statement under addition of a declaration by the Brownian motion, which would be absolutely bad. It would be a step backwards in terms of my on September 18 at the Academie des Sciences Morales reported message. On the other hand, even if we assume that the official checks of the cultures yielded positive and accept that their authors a publication, you can expect it impossible that Mr. Lapicque could study the matter in a period of 15 days, enough to change his view that seems so firmly fixed in his head. Added that the regulations of the Academy allowed him to discuss in his report a statement at the time and that we could at most add a second statement - which, if it takes as much time as they wish, would come out next year.

Now it is possible the opposite might bring the situation in order, by the attention to the interpretation Lapicque directs, which I have (as your representative) given last September at the Academie des Sciences Morales. You remember that I had agreed to a classification of 'Bions' in the physico-chemical field of matter under the condition however that will be all single-celled organisms that reproduce by simple division, there expected. The exception 'life would be' so the stage of differentiated organization reserved. Since this interpretation Lapicque especially with respect to the nature of the 'physico-chemical Bions' and their movements are pretty, he might agree to quote them in his commentary, the lower condition of course that he would be quoted in full, so they would of any future interpretation, there would now classified the 'Bione' under the various microbes, such as the questions today are to be as 'defined alive'.

I do not see any other solution of the problem, because as Lapicque proposing that the publication is in any case whatsoever to reject and would destroy all my efforts in France threaten to render. On the other side, namely, the matter their peaceable way. In a doctoral dissertation at the Sorbonne, you just now 'Bions' constantly, cited, and it gives me hope for a major article in one of the most popular literary magazines. This is the safer, albeit slower method.

So I do the following proposal: today I send my message and the Academie des Sciences Morales Lapicque and agree with him on this occasion that I myself have grown in a year the 'Bione in Nice.

If you agree with my other hand, the above proposal, please telegraph me immediately upon receipt of this letter. Grasp the telegram from dan so ((un assez clair telegrams)) that I can tell Lapicque the text, I will ask him that he includes this sentence in his own comment. If you see a hazard but in this approach, please let me also with by telegram. Then, I believe it is necessary to prevent the publication of the notice. It is enough opposition from rank personalities times ((personnages de deuxieme zone) have to) without having to have provoked opposition in the Bulletin of the Academie des Sciences.

So I expect your telegram and am, dear friend, Yours very sincerely.]

Reich to Lapique, ~15 Feb. (*Beyond*, p. 136, *Ca Biop*. p. 20) "I withdrew my permission.... extensively in a different context." (original in correspondence box 9, unlabeled Lapique fld) [google: After extensive consultation with Prof. Duteil, who controls the culture experiments and was the first in France to do so, I came to the conclusion, to ask either my first statement in extenso to publish, which is the central point of the whole question of the successful culture called would be, or to cease publication, provided that you do not agree to. To explain, I must add that I had not reported the production of Bion mixtures alone without the cultures to the Academy of Sciences. For this they would have been interesting.

Through the successful culture of the mixture of the colloid Prep. 6 also changes the nature of interpretation. I understand that you have based on the novel Professor Duteil interpretation that is imposed by the cultures. My interpretation is that some chemical-physikalische no absolute meaning in contrast to the biological. It does not coincide with that of Pr Duteil.

If we were only the identification with the purely physical as opposed to 'biologisch'-taken' Brownian motion publish', the fact secured the culturability of Bione would not be taken into account. More, then there is a risk that my studies would have completely misunderstood.

I therefore see no other option than: publication with the inclusion of culture results or no publication.]

Reich diary entry, 15 Feb. (*Beyond* p. 136) "In his letter of 8 January 1938, Lapique of the French Academy is attempting to downplay the cultures and pass them off as Brownian movement.

The director of medicine in Oslo is [Schreiner] afraid of the three idiots who want to get me out. He's afraid of me too; during our talk today he practically put into my mouth the words necessary for me to survive."

15 Feb., Reich telegram to DuTeil (box 10, DuTeil flds) "Falls gleichzeitige Publikation Kulturergebnisse nicht möglich bitte nichts publizieren. --Reich"

16 Feb. 38, Reich to DuTeil (box 10, DuTeil flds, 1 p***) [I telegraphed you today that the publication should not be made when the Publication miterfolgen not the positive cultures may fancy. You may remember that I meldtet in the first communication, the culture results. Lapique can not publish his interpretation, if it brings my communication in extenso, that includes the cultures. I will send you the original of the letter I wrote today Lapique for transmission and possible to addition of your own opinion.

With your verdict in the last letter I can explain myself completely agree. By now you have probably been the publication of our experiments which appeared a few days to get. Perhaps my own interpretation of the culture issue is likely to remove some difficulties, as it gives up the absolute antithesis of physical and biological interpretation, and in their place a the same time separating and uniting sets. By blaming is only one complication of physical features for the biological functioning. Whether that may be suitable for removing such heavy resistance in the biological world, I can not judge.

Because now introduce your interpretation of 18 September in itself a complicated debate must, which is not to provide a brief statement of the Bulletin, which also publish Lapique is not likely, I enclose a letter corresponding to the only alternative publication with culture results or No publication is left open. The well is the result: no publication. That does not matter. The public interest is growing not only in France but also here. The authorities, which occurred generically speaking a lot more cautious language. However, it can give a new storm, as the detailed fact could trigger message.

We have until 22.II. Time. So Lapique may still allow the general publication, if he sees that the matter is not easily dismissed. I sent you two days ago Gram-stained preparation and egg-culture of Preparation 12 bions. I already announced to you the first structured and nucleated Bione. The cultures are best to keep egg, with caution on pure blood agar, not on a light agar [i.e., only plain nutrient agar].

Please write me about the outcome of the affair.]

17 Feb. 38, DuTeil to Reich (corresp box 10, DuTeil flds, German transl 2 pp.**) [In relation to the whole as well as in the details, we are in full agreement with each other and have never been otherwise. Your telegram and your letter confirm this.

First, we agree that there can be no official publication if the cultures are not taken into consideration [in such a publication]. In your letter is provided to Mr. Lapique (the last) is absolutely correct ((parfaite)) in tone and very reasonable.

Next we are, despite appearances otherwise, also in the interpretation is entirely consistent that the known and cultivable Bions today are considered equivalent to living microorganisms.

Finally, there is agreement in the interpretation and the fact that the one like the change at will, both as 'alive' as well as electrochemical phenomena ((manifestations)) can be considered, because we—you and I—like to equate the two processes.

In these specific points also ((à ce stade précis)) and in that which directly concerns the Bions, our two interpretations overlap completely.

Our differences from each other to do with this particular ((précis)) question anything. They lie on a different rank ((endroit)) say will, in going beyond ((au dessus)). In this respect, namely, as your identification to all stages of life, reaching to the highest up, while I for my part of the moment when we using different 'and' an immutable type, which is called the type organisierten' Organismen, according to do have, acknowledges that you introduce in the present state of science must be something else, which is a 'organizing idea' and 'maybe' is not the raw matter ((la matière brute)) is reducible will—I say, away from her the nature ((comme nature)) is different—but, while it might not, is unzurückführbar to the already electric tension and relaxation in orderly matter. It is, if you want to appear to state that a certain level of awareness and autonomy, which is also fact.

If I wanted to try to compromise my interpretation Lapique move ((en suggérant)), so it is of course the recognition of the fact of the cultures included-was the essential. This is even the only goal that I set myself with that suggestion. I have simply made Lapique the recognition of the cultures easier, as I freed him from the need to simultaneously admit something he could consider a radical materialism, and even introduce this ((et de s'en faire l' introducteur)). This need for him was obviously very uncomfortable, he added with vehemence-done everything to avoid it by claiming a priori, it might be just 'slip cultures'. I gave him the opportunity to those cultures which equate the known microbes. That's all we could ask for. Then we would get the fact out there ((nous Courion ainsi notre chance)). That he would have agreed, is in fact not quite say.

Our three views can therefore be formulated as follows:

For you are electrochemical phenomena and life not only not unzurückführbar [irreconcilable?] with each other, but even one

can be determined through the other ((s) define). There is a limit on any level. For Lapique electrochemical phenomena and life are unzurückführbar [irreconcilable? incommensurable with?] each other, the boundary between inorganic bodies and microorganisms. Therefore he can not admit the transition from one to another. It is possible that there is a limit to how Lapique says, but it is higher and does not affect our work. So this is an intermediate view ((une position de conciliation)) to you in all that we are currently interested in, law, leaving the future open for those ((reserve l'avenir)), think like Lapique. And I was hoping to achieve that he will in relation to our particular point would rather point to a problematic future agreement on other points.

As you say, it is difficult to decide whether it has value, "apparently making" a concession, because in reality we do not avoid, in order to meet the contemporary biologists on the head. Revolution or evolution, that is, is not it? -The eternal question. But you see we in the basic ((sur le fond)) are in agreement with the same objective: the progress of the spirits, I mean the spirits of our contemporaries.

My view has, moreover, besides its diplomatic character of their own fertility. It allows the 'matter' to distinguish degrees of organization, whose highest hardly differs from the lowest grades of life and certainly quite capable differenzierten ((prêt)), is shaped by the spirit " be here when a spiritualist border maintaining of wants. About 'spiritualized' the matter in some way. Next: I rely on this view to durchmustern from a new point of the entire Bakteriologie " ((Réenvisager)), in the sense that it has made Tissot for the BK, and I have remarkable results. A first part, I will tell you soon.

I have vaccinated the albumen tube with preparation 12 again. It has risen wonderfully in 12 St. [for reasons] at 20 °. These organisms with multiple cores are very remarkable. Hold the middle between amoeba and protozoa. Could you: 1) I immediately know the formula of preparation 12

2) I need you to send a little blood charcoal (Carbo sanguinis send) which is not to be found in Nice.

I close this letter now so that I, if possible, still can expedite evening. Keep me informed.]

18 Feb., Van Den Areud to DuTeil (box 10, DuTeil flds***) ["I am eternally grateful that you have been friendly enough to send me your "Experiment of a Psycho-Biological Synthesis" ((Essai de synthese psycho-biologique)) and your report from the Academy of Science Ethics and Politics. Your trust in me made me very happy, and of course I will head your work only in accordance with the regulations and required guarantees of this university.

Contrary to your assumption, it is above all the scientific and experimental side that interests me. The philosophical part is secondary. I would very much like to know which characteristic properties "bions" have: How do they divide or how do they reproduce? Do you believe that the continuous dynamics ((le dynamisme aeterne)) of charge and discharge shown by bions are enough to put them side by side with life forms such as amoeba and bacteria ((pour le assimiler a))? If you don't mind, I will speak to Herrn Andre Mayer concerning your discoveries: I believe that his opinion about the organization of lower life forms cannot be ignored ((n'est pas a negligier)). Anyway, I am very happy to be informed of your experiments. May I hope that a letter exchange will develop between us?

I ask you once more to accept my sincere gratitude with the expectation that I will again be honored with a letter from you.]

24 Feb., Reich to DuTeil (box 10, DuTeil flds, 3 pp***) [I would like first before I respond to your letter dated February 17, ask whether you have my letter sent to Lapique Lapique continue, because I have not written directly to him from here, but via you. Hopefully there is no misunderstanding task.

Now to the major points of your letter: 1) Despite the fact that we seem to understand each other in fundamental ways, I believe there's one point on which we misunderstand one another. In my essay... I've tried to clarify this difference. It's not so that I believe there's only a unitary identity between physical-chemical and living matter; but rather, at the same time (along with the identity) there's a sharp differentiation between the two. I see this difference, as I've often said, in the specific interpretation of the physical functions that organic matter manifests. Because of this, I find no need for a metaphysical principle to explain higher life functions. **(Yet,) I can't object to the fact, that in order to experimentally clarify the development of the higher stages in the realm of the living [phyla?], we must assume the existence of an unknown force/energy.]**

2) That fact can you enforce such a concession to metaphysics for the first part of the hostility out of order. But I think that the shift of the border for living up a tremendous flood of new issues on its own already in motion.

3) You write that for me there is no level at a boundary between physical and living. But I think that there are several such boundaries, both between the living and nonliving, as within the living at various stages of development. The boundaries seem sudden complications of previous functions to fulfill. I hope to discuss on the basis of my experiments in the foreseeable future these subtleties of interpretation. The first thing I had to limit myself to the grossest to first create a secure framework for the practical and theoretical work.

So I'm totally with you understand that we must do everything possible to convince the contemporary biologists. More of that, we must refrain from anything that might be useful for this purpose. I just wonder whether hypothetical concessions we make, will not in time require more work than is necessary for Wiederaufräumen [to put things back together again? To reconstitute the integrity of our ideas from compromise?]. I think that the dialectical-materialist interpretation in contrast to the mechanistic and metaphysical allows a satisfactory agreement in the overall transmission without the aid of an entelechy principle. Just between us, I would like you to allow me to remark that we are the contemporary biologists, who also should not turn too easily. The task of this research is still above all the truth and not the philosophical principle.

For point 4 is to say that I for my part, a spiritualist border would not be maintained, not on principle or belief alone, but also from purely objective experimental reasons. Just my psychological work, it made me able to sift through a connecting line between the highest functions of expansion and contraction. For the rest, indeed preparation 12, run the control experiments the way, even opened a certain hope of getting into the field of cell formation.

This was my opinion on the basis of the available facts. I want to emphasize that I will address myself, of course, in principle against any kind of interpretation, if only the facts which we draw from the reality, acknowledged publiziert und. I would still propose that we do not prevent ideologically bound Publications by the Academy, provided and that is the decisive factor, and the opportunity is given to present our facts and interpretations in the official Publications organ the Academy of the public.

5) The blood of coal was sent to you some days ago. The preparation 12, I received even by the fact that I broke up about 2 cc of sterile blood serum in broth + KCl, then sitting down blood ignited coal, and stand the whole thing after 14 days let Durchschüttelung [filter?]. Applied to egg-culture medium, resulted to my great surprise these structures. A second, similar experiment was arranged, but unfortunately not the same entity, but only blood carbon Bione. Similarly, controls are still in the 12th attempt of the preposition If you have a determination of the structure of the preposition 12 is possible, I would be grateful. This is the question of whether such structures are already known. After completing my control experiments, I will give you notice again.

I have no confirmation from you of the mission of the publication. May I ask you to write me how it has affected how you like it and what you like about it.]

8 March, Van den Areud to DuTeil (mentioned in Raknes to Allendy 15 June 38 letter)

9 March, (bion lab ntbk 3, ms p. 71***; also in OI box 8 "Research Preparations 1937-39" fld, ms p. 12, photo 60) [Google: The Seko preparation in broth + KCl found after 6 weeks from the egg first seeded on a yellowish, cocci and rods from existing periphyton.

After four days, they began to appear round gray heap, which had the character of the Seko-growing up. They gradually overgrown the entire medium.

Microscopically it was shown that the structures were different from the first Seko cells. They were small, strong structured, clearly marked with nuclei.]

"Research Preparations 1937-39" fld, ms pp. 13-14, OI box 8 (photos 61-62), n.d. but right after 9.3.38 item in file: [p. 13] "Theorie PA

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10 March, DuTeil to Reich (box 10, DuTeil flds, German transl 2 pp***) [Concerning Lopicque, don't worry: I sent him a copy of your telegram and the connection was temporarily cut off. I must tell you that Lopicque is a member of both the Academy of Medicine and of the Academy of Sciences, and is now very aged, which explains why he's rather set in his opinions. He is the inventor of the chronaxia, a device used to electrically measure the duration of reflexes. He's one of the great specialists of the nervous system. He kindly wrote me a long letter and sent me a brochure of his works, in which I found interesting connections with the peripheral electrification which serves as a base (? which we think basic?) I pointed this out to him, moreover, and I await his reply.

I was able to resume the cultures of preparation 12, moreover, whose formula, which you had previously sent me, I found again. I sent a sample of it, at his request, to M. van den Areud, President of the Psychology and Biology Group at the Sorbonne. He passed it on to Prof. Debré of the Faculty of Medicine, who just the day before yesterday again asked me for more of them. In this way I could give you the identification that you requested from me, and this shows that little by little interest is growing around your works. It's just that much patience is required in this respect.] [Google: About the Preparation 12 whose formula you had sent me, I have rediscovered the way, I can win cultures. A sample of which I sent to Mr. Van den Areud the President of the Psychological and Biological Research Group at the Sorbonne. He had asked her and she has sent Prof. Debré of the Faculty of Medicine, who has now asked me yesterday for more. In this way I'll be able to give you the desired identification.

And this shows also that the growing interest for our work by and by. You have to have very much patience.

The magnificent edition of the Report No. 6, I have received with thanks. I am very grateful for the considerable space you have given me in it. I make use of that to increase the interest here in France even more. But you will probably know that the French in foreign languages in general are not very versed to read the book in the original text. We might, however, interpret in some bookstores. In Nice I'll take your own. For Paris, it would be the best, the thing Viktorion, rue de l'Ecole de Médecine, transmit, supplies most of the bookstores in France. I continue to believe that it would now be good to send the libraries of medical faculties in France, one copy each. I could also take over and cover letters ((les lettres d'envoi write)), if you wish. There were 15 copies required.

I'm with you distinguish an opinion on the necessity of being difference ((difference de nature)) and functional difference. On the other hand it is still far away from a decision on the functioning differences. There are many imperceptibly progressive stages between the disorganized, amorphous matter and differentiated life. In particular, the crystals lift, as you know, many properties in common with the microorganisms. It is observed up to you just as healing processes in tissues. Vira and the chemical can be classified, because on the one hand you can focus them on the other hand, they transferred a cellular disorganization. The same applies to the cancer.

Moreover, I touched the 'justice' ((justesse)) your comments highly and the justice that you understood my position and have the admirable care which they met to facilitate the recognition of the scientific world enables. I am convinced that the few concessions, I do weigh, small in proportion to the amount of approvals that I get a result. It is always easier to let someone go up a slope as imperceptible to persuade him to jump over a wall.

I beenige just by creating a small laboratory, where I will begin tomorrow with the help of a specialized assistant again with the experiments. The large sterilizations still require the municipal laboratory, where they meet me constantly friendly. I cultivate the drug 12, which I will produce new, very carefully. I have procured in the form of blood serum Hämostyl, filled in sterile ampoules. Control cultures also will I do. I intend to take culture experiments on agar at different temperatures and on potato. Could you send me these cultures a large enough number of protein-tubes and give me the exact formula?

I will keep you constantly informed of the results to date.

With best regards to all employees un hre to Ms. Reich, I am, dear friend, Yours very truly, RDT

PS I almost forgot to tell you how much I liked the excellent presentation of the work. With Klichees excited and it excites the admiration of all people to whom I show it.]

15 March, DuTeil to Reich (box 10, DuTeil flds, German trans 1 p***, need Sellers to translate **French original photos** 1650-1654) [Google: I am writing this letter to let you know two things:

1) I do experimental work have now seriously taken up again after I've set up a small laboratory next to the Centre Universitaire. In Nice I use the City laboratory require only the big and sterilization of such activities, the extensive material. I have also hired a new assistant specialist who comes every afternoon. I think you soon will be able to announce some interesting results.

At the moment Preparation 12 interests me very much. I have made it twice already and now just wait a few days to the cultures. I also have dealt with cultures from those on eggwhite, you have sent me. I told them there can now announce that I have gotten excellent growths, and not just on eggwhite, but also on ordinary agar [typical / ordinary agar] and also in Liebig's bouillon. The agar culture was macroscopically a different color, which tends to violet. The microscopic organisms are the same, in contrast to the observed and reported by you rod degeneration, they are the same giant cells, often multinucleate, accept the aging legs and Rakettform. The broth culture to me is also well done, with a very smooth pellicle, which rises at the edge and with snow-like flakes that sink to the bottom. What very much concerned me was the similarity of these organisms with the bacillus Yersinia (Bubonic Plague), and I'm not surprised at the lethal effects that it can achieve. One must be very careful and I am doing so. As the growing Yersin ((pousse)), this organism also at room temperature 20°. In the incubator, it grows at 35-37° a little faster. I am continuing my efforts. The organism seems to me however, is somewhat rounder than the Yersin [plague bacillus]. We will see what Prof. Debré will say, who has ordered about it.

2) That I need to talk with you about the other question is now following me very angry. But I am constantly harassed by the Reichert's representative, who demanded that I pay for the microscope that you lent me. This has been going since October Sun When I received this year by the French government would support, so of course I had this issue delivered to me in installments ((à condition de l'échelonner can take)). But at the present state of our budget, I expect this year in any event no longer to work on it. I make it so, so absolutely put on myself, barely to cover the daily cost for the laboratory and for my assistant, setting it **up for** me. If we lost my microscope, I can not continue the work. I respect the man now to reply in several days that I have for this camera, I have taken on loan from you, me as only you **bretrachte** accountable and not need to pay an apparatus owned by you and paid for by you ever has been. What do you think of this? What can and what should I do? In what way could you intervene? For I would now like the other does not bring you into trouble. Let me answer as soon as possible. When I agreed to have used the apparatus of propaganda for Reichert, I believed that it was a relatively permanent condition. The first complaint in

September but is now done, so **less [fewer?]** than two months **nch** of that agreement. Keep me up to date on the relevant BK results. If it is as an experimental procedure, let me know also. We could indeed, with **ewas** is good in his hands, find an effective support to the people who have already supported Tissot.]

Freud arrested in Vienna by Nazis, 16 March 38.

16 March, Reich to DuTeil (box 10, DuTeil flds, 2 pp***) That's all for today..."

PS 3 pm (*Beyond*, p. 137) "We have just learned that Freud has been arrested in Vienna by the National Socialists. **Diese Missetat bringt nicht nur unerhörtes persönliches Unheil, sondern ist überdies eine Schandtat von grundsätzlicher Bedeutung.** I ask you most urgently...to undertake everything in your power to organize a massive protest in the French scientific world against this attack on Freud by stupid barbarians....We are organizing here in Scandinavia."

19 March, Reich to DuTeil (box 10, DuTeil flds) "Die Ereignisse in Österreich lassen mir keine Zeit zu ausführlichen sachlichen Beantwortungen Ihrer Anfragen. Ich verspreche diese, Ihnen demnächst zu geben. Heute nur wegen des Mikroskops. Schreiben Sie bitte an Reichert, Wien, er möchte sich in der Angelegenheit direkt an mich wenden. Ich werde die Sache schon in Ordnung bringen.

Hoffentlich überstehen wir und unsere Arbeit die kommenden tollen Zeiten!"

22 March, DuTeil to Reich (box 10, DuTeil flds, German transl p***, French original: "Heute nur einen kurzen Brief: 1) Gleich bei der Nachricht über die Verhaftung Freuds hatten meine Schüler sowohl als auch ich selbst spontan daran gedacht, eine Aktion zu unternehmen [Regarding Freud's arrest, my colleagues and I have undertaken a spontaneous action.]. Aber die Nachricht ist einige Stunden später durch T.S.F. dementiert worden [Mais la nouvelle a été démentie par T.S.F. quelques heures plus tard et, de plus, j'ai eu confirmation de ce démenti par quelqu'un, fe mes amis, qui a reconstruit avant-hier a Nice le fils de Freud.] und im weiteren erhielt ich eine Bestätigung dieses Dementis durch einen meiner Freunde, der vorgestern in Nizza den Sohn Freuds getroffen hat. Es scheint, dass Freud bei guter Gesundheit und völlig frei ist und nur zurückgezogen auf einer seiner Besitzungen lebt. Ich denke, dass sie doch vor dem doppelten Skandal—im Hinblick auf die wissenschaftliche Bedeutung und auf das alter Freud[s?—den ein solches Vorgehen bedeutet hätte, zurückgeschreckt sind. Sollte aber eine neue Wendung eintreten und wir gewisse Nachricht davon erhalten, so ist es selbstverständlich und Sie können dessen versichert sein, dass ich unverzüglich eine Aktion veranlassen werde, nicht nur in Nizza sondern auch in Paris bei den Wissenschaftlern, die ich kenne oder erreichen kann und mit Hilfe der französischen Regierung.

Ich brauche Ihnen nicht erst besonders zu sagen, wie sehr ich mit Ihnen fühle in dem Unglück, das über Ihr Land hereingebrochen ist und dessen materielle wie moralische Auswirkungen Sie so stark fühlen müssen. Wir verfolgen alle voller Angst die Entwicklung einer Situation, deren Ausgang von tragischer Bedeutung auch für mein Land erscheint.

2) As you propose, I am entirely in favor of entrusting to certain clinics and hospitals the results of the present experiments on cancer (which I find *extraordinarily* interesting. What I obtained myself with preparation 12, which I made here twice, is truly stupefying. I have the very clear impression that we are succeeding in regenerating the red globules from blood charcoal, calcinated and reddened, and subsequently cultivating them. I obtained all the progressive forms between the typical red globule, a disk flattened and even hollowed out, and the polynuclear forms either circular or spindle-shaped, which reproduce themselves very actively. All the cultures exactly the same results, microscopically and macroscopically. On bouillon, I have an opaque scum showing strict anaerobes. At the Municipal Laboratory, the Director obtained the same result. On agar the growth is less active. I'm still working on this, and I will keep you posted.

I think that it would be good to entrust our first tubes to the Dean of the Faculty of Medicine at Marseilles, who is a known cancer specialist and is familiar with the experiments; he has even seen them here in the microscope. We shall see then if there is good reason to go elsewhere. I am starting with Debre in Paris by the identification of the preparation 12 bacilli.

3) This morning I received three tubes of preparation 11. Could you tell me what this is about?

4) I understand about the books. In a few days I'll send you a list of addresses to send them to if you wish to send them directly. If not, I can take care of it and accompany each one with a letter, which would perhaps be preferable."

22 March, Reich to DuTeil (box 10, DuTeil flds) ["**I am adding a few very important points to yesterday's short report should one wish to avoid an unpleasant surprise or distrust.**

I already informed you once that mildew spores are similar to the seko-structures. Of course, I was worried that the mold spores could have entered from outside and seeded themselves. I am now closing in on how the seko-structures came to be. One can find isolated formations on egg-medium upon which incandescent soot in bouillon + KCl was poured.

The structures that one notices in the seko-cells come from charcoal dust, and I know this for a fact, that penetrated the egg-white substance and interfused with it. In order to avoid misunderstandings and in reference to the control experiments that I am performing here, this must be regarded as a preliminary report.

I am now in the midst of pilot tests to determine how cancer cells are formed and have several very notable and hopeful results. The cancer problem ring is more and more complete. The damned part of the story is just that I have no possibility to further test the blood bions that I have microscopically examined on the ill. I believe that this problem can be solved in a few months if all necessary resources were available. Please provide information about the procedures over which you are oriented. You have no idea how much benefit it means for our work when we know that we are not alone.

Today America requested several copies of *The Bions*.” [nothing states that these copies were complimentary]]

Reich to DuTeil, 25 March 38 (Box 10, DuTeil flds, *** *Beyond*, p. 137-8) [Google: Today we got your letter of March 22, which I must first have translated. But I have understood [from your letter] that you are able to reproduce preparation 12. I can make you now the good news that we have now succeeded here in producing formations of late cells in pure culture. The technology is too complicated and technical requirements are too complicated to briefly explain it here. These attempts, as much as I would like to say, model the formation of cancer cells. I send you attached a tube of the strange red and yellow growths. I hope it succeeds you to continue it. I got it only today.]

Thank you very much for your efforts and for your sympathy with regard to the Austrian tragedy. We are all completely bewildered by this turn of events and for the time being we can do nothing but concentrate on how to rescue our work for posterity from the mystical flood. For this reason, and I say it quite openly, it would be very advantageous to force the pace of the work. In the long run, this madness will not continue. All that matters now is to survive the coming period.

[Google: Tubes I last sent you contained a growth, which I got by chance, when I inoculated by incandescent blood charcoal in broth + KCl inoculated on eggwhite medium. In recent years an increasing number cultures that I can not conceive otherwise than coming to life and proliferation of egg medium Reinz under the effect of the coal. The growths decay to strange rods and then let cultivate further. This is for today. I am writing to you in detail as soon as my head is free enough to bring the complex issues attempt to contact you.

This has evolved from an academic college Control Commission set up to want to verify the tests. I fear that it is founded with the intention of making trouble. Also for this reason, any positive message from France is of great importance.]

Reich to DuTeil, 30 March 38 (*Beyond*, p. 139-40) “The extremely stupid smear campaign conducted against the experiments here has taken on serious proportions. Among other things, Lars Christensen, who was our financial sponsor, is being severely criticized from Copenhagen on the grounds that at the time I did not have a residency or work permit. Now, under pressure from our friends, Christensen has inquired through the Norwegian embassy in Paris how the experiments are proceeding. All this is just for your information. I must stress once more that it is crucial to our work and our existence that we learn of any positive result that is obtained in France. We will be able to survive the immediate future only if we can counter this abyss of envy and infamy by responding with clear, factual answers based on experiments. Ganz besonders dumm und allen hier offenbar benimmt sich der hiesige Krebsforscher Kreyberg. [Local cancer researcher Kreyberg is quite especially stupid and everybody here so behaves.]

You will be pleased to hear that the S-mice keep on producing tumors. Yesterday I obtained a subcutaneous tissue tumor that penetrated into the skin and infiltrated along the blood vessels; the kidney and liver were totally invaded by metastases. You can see how upsetting the situation is when such facts are obtained in face of this smear campaign. I will keep you informed of developments.

Das ockergelbe Zellbionpräparat Nr. 13 haben Sie wohl sicher bekommen. Ich schicke eine Tube des 12er Präparats und eine des ockergelben Zellpräparats Nr. 13 direkt an den Dekan der Medizinischen Fakultät in Marseilles, wie Sie empfehlen.” [The ocher Zellbionpräparat No. 13 you have probably received safe. I will send a tube of preparation and a 12-cell preparation of yellow ocher No. 13 directly to the Dean of the Medical Faculty in Marseilles, as you suggest.]

6 Apr., DuTeil to Reich (corresp box 10, DuTeil flds***) [At the same time as your last letter I received your assorted samples, in particular your Preparation 13 of March 25. I subcultured it immediately on eggwhite and agar, and it grew up wonderfully with the same color. I am going to send some to Paris in a few days. Can you explain the exact procedure involved

to me, particularly since I am going to have cancerous tissues to study, from my laboratory assistant's uncle who is a surgeon at the hospital.

I wrote Cornil to inform him about your shipment. (Cornil is the Dean of the Faculty of Medicine at Marseilles.)

At this time I am conducting two series of experiments in my personal laboratory and at the Municipal Laboratory of the city of Nice, which having now grown larger, can help me much more and welcomes me with pleasure.

I have made a whole series of Preparations 12, and I have obtained almost entirely positive results. This is all written down and will be sent to you soon. In particular, I succeeded with Preparation 12 in the "h-tube" and also with a procedure called "duo," which consists of placing separately one part bouillon, another part KCl, blood charcoal and serum, and making the mixture with a sterile pipette when it is out of the autoclave. The organisms I obtain are aerobic, but not strict aerobes. The most interesting is the culture results.

In fact, obtained in the form of cocci, these bions subcultured on agar and eggwhite gave me cocci on agar and very long rod-shaped cells—almost filamentous—on eggwhite. But the main thing is: I subcultured in switching media [folding over]. The rods of the eggwhite culture became again cocci [when transferred to and grown] on the agar; the cocci from the agar became rods on the eggwhite. I again subcultured each of these two tubes on two different agars and eggwhite and obtained each time the pleomorphism of which I speak. So, as I have maintained for some time, it is certainly the medium that plays the essential role—if not to say, the only useful role—I will send you the samples shortly. But I still lack tubes of eggwhite, which is hard to obtain here because of the necessity of rendering them basic [alkaline?].

I have just been informed from Customs about the arrival of your works; I will get them tomorrow. For your information only, the cost of the Customs fees amounts to 165 francs, which, moreover, doesn't seem excessive to me considering the value of the works.

It's possible that a few days from now I may experiment on some sick patients at the hospital. In light of this eventuality, it will be necessary for you to make one small note on the present state of your animal experiments, and also several samples ready to be tested.

Concerning your letter of the 30th, it is necessary, I think, not to let yourself be overly influenced by the very natural reactions from people offended by these works. We are here to confirm the method and its results. The absolute reality of these results is the only reply one need offer in opposition to their objections. The difficulty is that, in order to certify this regularity, one needs to execute the works oneself over many months. And only someone who is convinced ahead of time will make the sacrifice of leaving his work to devote himself to this work. Thus, it is a vicious cycle from which it will take a long while to emerge. As for me, I have made up my mind and assumed the practice of waiting until people come by themselves to ask me for information.

Send me as soon as possible the formula and the procedure for Preparation 13 (rose culture) so that I may give Cornil all the information verbally if he comes to Nice. If that succeeds, we'll have valuable support here.

I expect to hear from you soon. My good feelings of friendship to all over there and believe me fondly, your RDT]

6 Apr, Krebsmodell 14 (OI box 8, "Präparatherstellung und Protokoll" fld, ms pp. 61); ms pp 62-63, n.d.: "Präparat 17 Eisenbione [Iron bions]

7. Nun erfolgt die Impfung auf Eiboden IV. Die Ei-Kultur liegt im Thermostatt, nachdem die Eprouvetten aufgeschüttelt waren..."

Reich to DuTeil, 9 April 38 (*Beyond*, p. 142-4, corresp box 10, DuTeil flds) "**I have yours of 6 April**. [Google: I was very pleased that you succeed the 12-trial promptly. Here is each culture preparation at 11 to prompt. I have to admit only that my original assumption, which would Sekozellen stemming from the mixture of the preparation of 12 is incorrect.

I am now the emergence of cell structure by specially-scale experiments have come to the track. I send you to the residence with a request for vorsichtiste Handabung these communications information as to the preparations 11 to 13th incl the ocher cells growing up the preparation entstamen 13th I could not reproduce it so far. But as I said, I have seen microscopically as they arise. They are formed by mutual Durchringung of Kohlebione and Einährbodensubstanz or auoklavierte Blutbione. It took weeks before I came out and it required infinitely Müseler investigations.]

Thank you very much for the opportunities you offer for carrying out experiments on the cancer studies. But, as I have always done in the past, I would like to speak to you quite openly on this occasion. You are sufficiently familiar with my situation that I do not have to explain in detail to you why I am forced to take maximum precautions. The cancer therapy, which in principle I now have 'in the bag,' so to speak, represents the high point of two decades of scientific work conducted under extremely arduous circumstances and in the face of major risks. I have come to see people—that includes academics—in a very dark light, and you will understand that I do not wish to let something like this slip from my hands. However, from a purely objective standpoint, it would be crucial for all concerned, and particularly of course for people suffering from cancer, to have an opportunity to clarify the question quickly. I am not trying to ignore the fact, I have available the results of a large number of

experiments, conducted with animals and under the microscope, which have provided me with a more or less satisfactory picture of the way in which cancer cells form and how the bions act on the cancer cells. I would be very happy to make these data available, provided that I received from the experimenter or clinician in question the assurances of his strictest discretion, and provided also that he kept me informed in detail of the results. I urge you not to regard this as exaggerated mistrust on my part. If anything, I tend to be all too lacking in caution. But this time I am dealing with what is, in effect, the main part of my life's work. I would therefore recommend that, if possible, you draw up with the head of the clinic in Marseilles or some other clinic a written agreement that animal experiments performed there would be carried out on terminal cancer patients using the bion cultures from this laboratory. The agreement should also guarantee that I have priority in the use of any practical therapy that may be developed. Please consider whether such a request can be made to the clinics. The work would involve applying to human beings the results of the observations I have made here under the microscope, in vitro, and also on animals. Naturally, I would prefer if I could personally take part. It is extremely important to have a good understanding of the idiosyncrasies of the bion problem in order to arrange the tests in such a way that they yield positive and not negative results. The bions are in effect special organisms that one has to have studied continuously for years to take full advantage of any potential they offer. The details obtained from assessing the effects of the various bions are too complicated to explain in an understandable manner in a brief report. Furthermore, I have carried out a number of experiments with pneumococci, tubercle bacilli, streptococci, etc., which are all extremely promising, but only in conjunction with certain types of bion cultures that I am carefully maintaining and propagating.

For about two months, I have been working on a model experiment for cancer cells that provides very satisfactory insight into the process by which the cancer cells organize themselves. As soon as I have completed this experiment, and cooperation with the clinic in Marseilles or in Paris, or with both clinics, has been settled to everyone's satisfaction, I shall make available to you the details of this very interesting procedure, which I believe is also of critical importance to cancer research. Among other things, it confirms your statement that the essential thing about all our bion experiments is not so much the media themselves as the effect achieved on the media and the changes that occur in them. That is also part of the cancer problem."

[Here one could place Reich in the tradition of Pasteur and Dubos, the "environmentalist" tradition in microbiology vs Koch's monomorphism and rigid Linnaean species outlook.]

13 and 14 April, DuTeil to Reich (corresp box 10, DuTeil flds, 3 pp.***) [Dr. Cornil, Dean of the Faculty of Medicine at Marseille, should be spending part of the week at Nice from the 19th to the 24th of April for a Congress. He should spend a long while with me during this stay, to examine all that I have here. I ask you to send me right away by airmail a note giving me the present state of the cancer question, as well as the formula for Preparation 13 rose which you sent me and several details on the experiments for causing cancer as well as for stopping it in animals. It is essential that I give him all the most detailed explanations. You know that he is the Director of the Cancer Institute at Marseille and occupies a first rank position in French medicine. If I succeed in convincing him, that could have very rapid and important consequences.

On my part, I've made very interesting experiments every day here. I immediately made a preparation with KCl, and another with KCl, bouillon and hemostyl. A positive result. Moreover, the last preparations 12 succeeded and completely resemble the cells of the cancer preparation that I obtained. Again, please send me the useful notes so that I can inform Cornil, whom I'll have the opportunity to have in my laboratory.

I've just received the photos that you sent me and I thank you wholeheartedly. To all the others and to yourself, very affectionately, RDT]

(D to R, 14 April) [I just received your small dossier, which crossed in the mail with my letter asking you for it. It is accompanied by your letter of 9 April and was preceded last evening by some photos which gave me the utmost pleasure.

In this way I am now equipped for Cornil's arrival next week, [to be able to explain to him] the genesis of the large organized cells. The comparison with the cancerous cells that I have is striking. If you should need specimens of cancerous tumor, I can send you some in one or two tubes.

I did not find in your dossier any trace of the magnificent culture of "rose vif" (bright rose) from your tube marked "13" of 25 March. These are the same large cells. I continued with the culturing, which starts out rose colored, on agar and on eggwhite, but tends to become gray again at the end of two to three days in the autoclave; whereas at room temperature it grows wonderfully well and rose colored. We are trying to identify them [the cells] at this time.

Concerning cancer treatment, I understand your hesitations very well. However, it seems to me that we can proceed without fear with a scientist like Cornil, if only because he looks (searches) in an entirely different direction. [He has taken a different route in his studies.] In any case, and as you are kind enough to propose it to me, I accept gladly—for myself alone—the paper of your general directives. If need be, it might be useful for me to allude to (refer to, hint at) a result, even without speaking of the procedure, if only to stimulate the interest of my interlocutor [Cornil]. This, of course, following the opportunity and the personality with whom I find myself—and you could count on all the nuances of my eventual report in such a case. I understand very well the importance of the question in which you are concerned and I take care of it as if it were for myself.

I close this letter so that it may be sent immediately. I am once again wholeheartedly yours...]
REICH NOTE with these letters (**photo 1658**) seems to be typed excerpts from Scharffenberg's 16 April *Arbeiderbladet* article; includes WR's handwritten note at bottom: "Gemeine Verdrehung, eines pestkranken, Schreiner und Asketen

Reich to DuTeil, 19 April (*Beyond*, p. 144-6, orig in corresp box 10, DuTeil flds ***pp 2-3) "You will probably remember that when you were here I warned you about specialists. At the time, I had the impression that you took this a little amiss and later interpreted it as an expression of my quarrelsome nature. I can assure you that about eighteen years ago, in the battle over Freud, I experienced to the limit all the dreadful sides of human nature that are now manifesting themselves, even though at the time it was not my own cause that was involved. I have always had the feeling that you are too optimistic, that you believed it would be an easy matter to convince the authorities as long as one had something good to show them. I often expressed my doubts to you on that score. In the meantime, we have discovered that partially disclosing individual experiments or dealing with fragments of the whole involves an enormous risk, namely severe disappointment, which then does more harm than the good that might have been obtained. We have no doubt at all here that bion research cannot be understood, and therefore the practical details of the work cannot be mastered, without one's taking into account *all aspects* of sex-economic theory and the dialectical-materialistic method. The way I have set things up here with my assistants is that they must carry out accurately and independently the entire set of experiments, starting from the very beginning, before I demonstrate the more advanced, complicated experiments to them. Therefore, it is also impossible to understand the cancer experiment and to carry it out in practice unless, for example, one is firmly convinced from personal observation that (1) the protozoa really are seen to develop from disintegrating grass tissues and (2) in principle precisely the same process is involved in cancer tissue.

If I were to announce today the hitherto unreported cancer experiments that have been going on for the last one and a half years, it would be the coup de grace for everything. The work is comprehensive and follows a logical path [p.2 of orig] down which one must laboriously and diligently proceed, beginning with the preliminary experiments with grass and earth. I would therefore like to make the following proposal to you, if you wish to include third parties [i.e., Cornil] in our cancer research.

I am trying as far as possible to devise simple test procedures that are achieved gradually, without any initial relation to the cancer problem, and that thus will automatically open one's eyes to the cancer question. Please let me know whether you are in agreement. I would then arrange the experiments in a logical manner and finally lead up to the cancer experiments. Choosing any other route appears too hazardous to me. I will therefore wait to send you the cancer reports until you have given me your approval or made some other suggestion. The first proposal that I would like to make today is that these experiments should above all focus on continuous and extremely accurate observation of how protozoa form from decaying moss and grass tissue; this phenomenon should be studied and confirmed. The processes can be directly observed under the microscope. Then some model tests with egg nutrient media and charcoal heated to incandescence could be introduced, and finally we would come to the work on cancer tissue itself.

Now to continue your personal work, I would like to explain the technique used to grow gram-negative s-rods from cancerous tissue. For this, please use only sterile cancer tissue, meaning avoid tissue that lays on the surface and, instead, surgically remove metastasis from cadavers or use those from freshly operated [patients] taken from interior tissue under sterile conditions. A bit of tissue with liquid is seeded onto an agar plate. Two types of structures always grow from this: a bluish-gray that in no way differs from carbon bions; second, a greenish-blue shimmering [one] consisting of short, rapidly moving rods. The two growths should be separated. Should you be unsuccessful in immediately growing the short, gram-negative rods, I am willing to send you further local cultures. These rods, which I personally names S-rods because I created them the first time using sarcoma tissue, till mice after one to two days using a strong dosage, or with a weaker dose or less virulence, after weeks or months. Most of these mice produced tumors, and some of them also metastases. Should you be successful in doing this, we can proceed to bion-cancer experiments on mice. Perhaps it would be a good idea for me to send you the manuscript, which now consists of more than 100 pages, to you for review. It contains the stages of my experiments for the past 1 1/2 years. However, you understand, dear friend, that I have learned not to inundate with too many facts at once, which only confuses.

I will now mention only briefly that there are many other experiments needed to study cancer, for example blood analysis and [p.3 orig] the direct examination of cancer tissues at great magnification. I tend to shred sterile cancer tissue on a specimen slide and to examine it directly following the operation, within 12 hours at the latest, at a 3000X magnification. One can study cancer cells very well by doing this. They are nothing more than jerky moving clusters of vesicles which are only a bit contractile. Studying the border tissue is very important. I have made several photos where the organization of decaying vesicular cancer tissue can be directly observed.

In the enclosure, you will find information about cancer model experiment no. 14. The cancer cell-like organization of the egg white culture medium can be very nicely followed by carefully studying the penetration processes. I urge you to temporarily keep experiment 14 to yourself because [it is the key to the problem of the origin of cancer cells].

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I would like to give you the reasons for my extreme caution, so that you do not decide that I am inordinately apprehensive. My opponents here have submitted various inquiries to Bonnet, Martiny, etc., which is of course quite inappropriate, because they did so without my knowledge and without my approval. [Aus einem Brief des Dekans Schreiner an meinem Assistenten Dr. Havrevold ersehen Sie, welcher Mentalität wir ausgesetzt sind.](#) [From Dean Schreiner's letter to Havrevold, you can see what kind of attitude we are up against.] An article written by Scharffenberg, who is a psychiatrist here in Oslo, casts doubt in an extremely insulting way on your competence to deal with scientific matters. I am including a translation of this passage. Please let me know what has happened to Bonnet, Martiny, and the others, so I can see to quashing any rumors that arise. It is better to be aware of an unpleasant truth than to live in a world of illusion. Finally, I must ask you, if possible, to enjoin those colleagues who are seriously working on the matter to take whatever steps they can to ensure that we are granted the necessary peace and quiet to continue here. It would be sufficient for one or the other of them merely to confirm that the experiments are being carried out and that it will take a long time to complete them. That's all for today. I hope that these comments will to some extent have made it easier for you to accomplish your own very difficult work.

...Now some good news: In Scandinavia, many very positive articles have appeared in publications, [just as in Hungary]. In Copenhagen in June, the director of the University for Educational Science [pedagogics] will hold a radio lecture about the bion book, and in Copenhagen the identification of cell tissues is being carried out.

Congratulations on your successful attempt to produce cells. I have recently obtained the round cells from a Preparation 14 that rested for two weeks. I actually believe that I have succeeded in artificially creating cells of the cancer cell sort; however, I would like to point out one small mistake. Cancer cells are elongated, and only when they are dying do they assume a circular shape and then resemble the cells from Preparation 13.

Please write again." [R encloses translation of excerpt from Scharffenberg 16.April article in *Arbeiderbladet*:***] In France are now to control experiments have been set in motion by 'Professor Roger Duteil from the University of Nice.' This was told in Dr. Reich's magazine in 1937 (page 206-207). Mr Roger Duteil himself came to Oslo in the summer and spent 10 days in the laboratory, and the Bion-work brings two essays by him where he is explicitly described as 'a professor at the University of Nice'.

However, there is no university in Nice. In the popular calendar 'Minerva' (Department of Universities and colleges in 1937, Page 1200), only a 1933-built 'Centre Universitaire Meditrranean' is called, is part of the University of Aix-Marseille. The task of this institute is just to keep rates on Nice visiting foreigners.

. Hohschulprofessor or university can hardly be Mr. Roger Duteil, his name is found in any case not in 'Minerva' register [note in Reich's handwriting at bottom of p. "common rotation and a plague Schreiner soket]

Reich to DuTeil, 21 April (*Beyond*, p. 147, corresp box 10, DuTeil flds) "Over Easter, a fantastic smear campaign broke out here, but we have launched a successful counterattack. All I have done is to draw up an explanation of the fundamental principles involved. I am sending you a copy with the request that, if you think it is correct to do so, you should distribute it to the press in France. **It is hard to believe how fantastic and childish people's reactions are to the issue of the origin of life. Obviously, forces and fantasies of which we are totally unaware are behind all this.** We are holding our own very well, not just here but throughout Scandinavia. But this smear campaign [Hetzkampagne] might make it impossible for me to go on living here. If that happens you will hear about it. That's all for today, but please write soon to let me know how your talk with the cancer researchers in Marseilles turned out."

23 April, Reich to DuTeil (corresp box 10, DuTeil flds) TELLS HIM OF IDEA OF INTERNATIONAL COMMISSION
“I’m reporting today only briefly, that I hope will make an end of the irrational and fruitless press campaign...”

Reich to DuTeil, 25 April (corresp box 10, DuTeil flds) “Today again only a short situation-report. I ask you before all else not to believe that I was somehow aggressive and provoked the matter.”

26 April, DuTeil to Reich (corresp box 10, DuTeil flds) “I’m responding only to your last letter, the one in which you announced to me your project [Sellers trans.: of proceeding to experiments in the presence of a rather large number of scientists invited by you, and where you want me to be a member of this meeting.

If material circumstances allow me, it is with the greatest pleasure that I will be present, because I believe my presence to be very useful considering the experiments which I have made, on my part, and which continue with great success since I succeeded with Preparation 13 as well as the preparation of the autoclaved blood, and also a continued series of the 13 with the cancer. I will give you the details later, as soon as my conversations with Cornil are finished, I think next week.

The month of June, however, is entirely impossible for me, because there are exams from the beginning until the end, and I can’t be absent one day. In addition, the month of September is extremely difficult for me for a similar reason: I prepare the students for the exams that take place the first of October, and it is the time when I have the most students. The more practical time—and even the only one possible for me—is between the beginning of July and 15 August, as I did last year. I also think that this vacation time is when the people you want to invite will most readily be free.

Yesterday I sent directly to *Arbeiderbladet* the information they need on the University Centre [at Nice].

Thank you again for your communications, to which I shall reply at greater length in a few days. Affectionately RDT.]

Reich to DuTeil, 28 April 38 (*Beyond*, p 149-51, corresp box 10) “After spending a few fantastic and so far extremely fruitful days engaged in the battle against the most narrow-minded and corrupt of personal attacks, launched under the guise of a scientific critique of the ‘bions,’ I now find some time to report the salient results to you.

Yesterday my proposal that we put an end to the senseless debate in the press by demonstrating the experiments in public before an investigatory commission was published in the local newspapers. The medical faculty at the university here has already publicly accepted the proposal in principle. The proposal [Vorschlag] itself was made by a plant pathologist from Copenhagen, Dr. Paul Neergaard, who, as you know, is working together with a bacteriologist to analyze bion cultures. Now we actually have to get this public commission together and conduct the experiments in public. This has enormous advantages and consequences that will also affect all the rest of our work. As you can see from the press clippings that I sent to you, the background to the campaign against the bions was nonscientific in nature. Now we have to address ourselves to the task as follows:

1. The members of the commission should not be drawn solely from Norway. Instead the makeup of the commission must be international.
2. A preliminary working group must put together precisely the test and verification procedures to be used. In the coming week we will hold a meeting here to work out the initial proposals. I would like you to participate in this preliminary group and to make suggestions based on your experience.
3. The matter [Sache] must be very thoroughly prepared. The demonstration cannot take place before August or September.
4. The commission will probably have to be paid for from private sources and we are initiating the work required to raise the funds. Please give your opinion on this as well.
5. Please let me have your suggestions [Vorschläge] on who could serve on the commission from France. I would imagine that the most suitable members of the commission would be people who have not rejected the bion book but instead have adopted a wait-and-see attitude. Also suitable are those people who are carrying out the identifications. Above all, it is necessary to avoid the many traps which our opponents will set for us. Principally, these will be of a tactical and bureaucratic kind. We must try to demonstrate our best test procedures.

[Google: 6. In the first glance I thought of as authorities for the control commission and for the witness function:

Professor Palmgren, anatomist, Stockholm (to him I have already written)

Dr. Paul Neergaard, plant pathologist, Copenhagen

Professor Bergersen, anatomist, Oslo

Would Debre or Colijn in question? Travel and accommodation would be paid of course.

As witnesses could include:

Professor of Psychology Schjelderup, who has already accepted.

Sigurd Hoel, writer

Dr. Ola Raknes, religion, psychology

Dr. Philipson, doctor, Copenhagen

Professor Thjøtta, Bakteriologist (whom I reject on a provisional basis for the Commission for his behavior is not objective.)]

The university here in Oslo has requested that our opponents Kreyberg and Thjøtta be allowed to sit on the commission. But Kreyberg is absolutely out of the question [kommt überhaupt nicht in Frage] because the commission must be *objective*, which Kreyberg was not.

I have just received your letter of 26 April. Many thanks for your willingness to participate. We will now certainly succeed in getting our views across and will spare ourselves twenty or thirty years of being hushed up and put down [Kaputtgeschlagen]. People will have to accept the facts. At any rate [jedenfalls], the first success scored was that the machinations aimed at having me deported miscarried, and for the time being I shall remain here. You will therefore receive from us an accurate list of proposals on how the tests should be performed.

The day before yesterday, a representative from an international press agency who happened to be in Oslo paid me a visit and I brought him up to date on the matter. He is going to write a long report for the American papers and for the rest of the European press. I will be allowed to see the report before it is published and have an opportunity to make corrections. In the near future I will send you an article I have written in a simple style for the local press entitled 'How I Came To Study the Bion and Cancer Problem.' This article is intended not to report the results that are already available on cancer research but merely to acquaint the public with the conditions that led from sex research to cancer research. [Found this in Mss box 9, "Von der Sexualhygiene zum Krebsproblem," "This article was sent to friends but not published.] Dr. Raknes will answer your inquiry concerning Roald Dysthe. I do not know the man. He is supposedly a wealthy and respectable businessman. I ask you to give me an exact report about your conference with Cornil. I discovered here that Martiny and Bonnet were negative, while a third and perhaps a fourth person are continuing to work on the issue. It was not a good idea to not share the issue with me then, for I could not refute the local rumors. You obviously had the same problems with the authorities here as I do. [using "als": You, as the authority, obviously had the same problems here as I did.] However, I would very much like to hear more about it.]

I am sending you a film about amoebae, shot for the most part in time-lapse mode, for possible demonstration purposes. ...From this it is apparent that the same autoclaving that promotes the formation of bions immediately after the preparation is produced, has a lethal effect after a few days have elapsed, just as it does on normal bacteria. The objection that an airborne infection is responsible was conclusively eliminated last year in Nice by preparing the bions and their cultures in a closed system."

28 April, DuTeil telegram to Reich (corresp box 10, DuTeil flds) "Telegraphiez si Roald Dysthe bon." [Telegraph whether Dysthe is OK.]

30 April, DuTeil to Reich (corresp box 10, DuTeil flds) [I am replying very briefly to all your recent letters. 1) My trip to Oslo is all settled. But I must be back at Nice on 15 August. I'll tell you in three or four days whom from France I think it useful to invite. I think that if Cornil consented, it would be a very important thing. I'm occupying myself with this matter immediately. Perhaps Debré would like to come also, and this would be significant. But it's necessary to wait until he has finished the verification, or rather the study he is conducting at this very moment at the demand of Mr. Van den Areud. It is necessary to avoid speaking of any result whatever, positive or negative, as long as the verifications are not finished. And there are none yet save mine, which really are—that is to say, none which has been made during the number of weeks and even months necessary to assure conviction and commit to a responsibility. I am sending you the letter I sent to Raknes yesterday. It is why considering the circumstances and the urgent importance of warding off the attacks you are subjected to for quite different reasons, I am of your opinion about the utility of a public demonstration so the report of the whole of the commission will attest to it—as well as the separate report which the commission of each country will make. In order to avoid the translation difficulties we had last year which cost us to lose a lot of time, we need to make plans for the French commission to bring a secretary.

2) Right off, a piece of good news. An intern at the Nice hospitals who is preparing his doctor's thesis will be happy to write his thesis on the consequences of our work and of my synthesis. He plans to work every afternoon with me. I accepted in principle. As his theories will be accepted by Cornil, the circle closes. Moreover, the thesis on pure science that I am preparing at this time on the biological part, might be very nicely complemented by his thesis from a medical point of view. It is not a question, moreover, of cancer only, but of the whole of parasitological theory.

I am confirming to you the positive result of all my recent experiments with the help of my specialized assistant-aide. And in particular I was able to show Dysthe, at the very moment when it was appearing in quite small colonies, the pale rose culture identical to the Preparation 13 which you sent me from Oslo. They were amazed. It is a question of a preparation 16: KCl, bouillon, blood charcoal, and venous blood drawn by sterile means but neither centrifuged nor autoclaved [= hemostyl? See 7 May report]. I did not centrifuge on purpose, for I think that we have gone beyond the stage of analysis and must approach that

of a more and more complete synthesis—and more and more approach even the conditions of nature, which is not analytic but synthetic. Equally positive are my results with the cancerous tissues and with blood, also not centrifuged but this time placed in the autoclave—and this despite the coagulation. There is no failure in my last series, and I start on a new series Monday. I await Cornil this week.

Enough for today. To all yours, and to you yourself very fondly.]

30 April, Reich diary entry (*Beyond* p. 151) “Sigurd Hoel against Scharffenberg. Proof that the fascist newspapers obtained all their arguments from Scharffenberg.

Scharffenberg doubts DuTeil’s effectiveness as a teacher. DuTeil submits documents.

In May, lay people will become involved in the discussion on the bions.”

May 38 (Manuscripts box 9) “Von der Sexualhygiene zum Krebsproblem” ms to friends, never published. 25pp, also Engl trans of a related ms*. ...1.) Cancer has a particular preference to target surfaces of the body's sexual regions and sex organs, especially in women. In Germany, for example, 10,000 women die annually of cancer of the genital organs. The most common cancers for women are breast-, cervical-, ovarian-, and genital cancer.**

2.) Cancers appear most frequently at the age following the peak of sexual function, that is during menopause between the ages of 40 and 50. Therefore it must definitely be connected to the decline of sexual activity.

3.) Cancer appears in the form of a sarcoma, the worst form of cancer, preferably during adolescence, seldom perhaps, but when, then extremely aggressive. This must also have something to do with sexual development during the process of sexual maturity.

4.) One could say that the proliferation of cancer is living one's death with full awareness. The growth of cancerous tumors actually succeeds at the expense of the entire human body.

Conservative cancer research has the same slogan available for cancer as the old, conservative Psychiatry has for mental illness: “hereditary predisposition.” As of today, no one has succeeded in proving these hereditary predispositions. In addition to heredity, irritation and [p.18] the effects from the smallest, invisible life forms (“virus”) are also assumed [to have an effect]....

Longer, more polished version of this ms in next fld in Mss Box 9, as “Von der Sexualhygiene zur Biogenese und Krebsfrage.” Mss Box 9, ms May 38 “Die Bioelektrische Funktion der Sexualität”

7 May 38, DuTeil to Reich (corresp box 10, DuTeil flds) “Retrospectives des Operations ayant et faites aux mois de Mars et Avril 1938, “Summary of the Experiments Done in March and April 1938,” 4 pp Dates: 12.3.38 2 tubes made plus samples (in incubator for three days)

The three samples negative, the two tubes positive.

Cultures: on agar and on bouillon made 21.3.38, result positive on 24.3.38.

24.3.38 Made a tube of preparation 12. Cultures: on bouillon agar and on albumin agar 29.3.38? On the three media 30.3 On the bouillon a surface pellicle, on agar and albumin agar, macroscopically the same. On agar cocci, on albumin agar, rods. The rods subcultured on agar 31.3 produce cocci on the first [strain I]. The cocci subcultured on albumin agar 31.3 produce rods on the first subculture. A new subculture and mixing on the first produces the same results as before.

Note: This pleomorphism, which is well known in bacteriology, confirms the microbial nature of these organisms.

25.3.38 One tube plus three samples: after 4 days, 29.3.38 the 3 samples are negative. Tube P12 positive.

26.3.38 One tube plus three samples: after 4 days, 29[sic].3.38 the 3 samples are negative. Tube P12 positive.

Cultures: 2 tubes of bouillon on the 30th, result positive on the 31st.

26.3.38 Same preparation in the h-shaped tube, autoclaved and inoculated in a closed manner of the curved branch in the right branch.

First positive result is obtained 8 days after.

Cultures: subcultured 31.3.38 on Oslo albumin, result positive on 7.4.38 and subcultured 1.4.38 on Oslo albumin.

A positive result on 8.4.38. Cultured again 6.4.38 the culture of the 31st on two bouillons and one agar, one of the bouillons and the agar takes two days after (8.4.38); the other bouillon takes 4 days after (10.4.38)

The tube of the first is again cultured on 6.4.38 on one bouillon and two agars, the three tubes are positive 9.4.38.

- 29.3.38 1 tube plus three samples, 3 samples negative, P12 positive on 1 April.
- 29.3.38 The preparation is made differently, placing the bouillon in a tube, and the KCl, hemostyl blood charcoal in the other tube. All of this is placed in the autoclave for 25 minutes and mixed after removal from the autoclave. To sum up, the same procedure as for the h-shaped tube. Positive results on 30 March 38.
Cultures: Oslo albumin and agar on 30.3.38, result positive on 4.4.38.

Preparation K: Formula: Kr bouillon KCl autoclaved at 130° for 45 mins.

- 31.3.38 [?] tubes made and left at room temperature.
Cultures: inoculated the same day on bouillon, agar, and albumin agar. Agar and albumin agar negative, bouillon positive 4 days later, 4.4.38. Subculture a bouillon from the stock of 14.4.38. Positive on 15.4.38. Subcultured two tubes of bouillon on 16.4.38, positive on 17.4.38.
- 11.4.38 Preparation PK14: Formula: kr bouillon KCl hemostyl.
Subcultured on bouillon agar and albumin agar on 14.4.38. RESULTS TOTALLY NEGATIVE. [ms note says 'Failure']
- 16.4.38 Preparation 16: Formula: nonautoclaved blood, KCl, bouillon, blood charcoal, left at room temperature.
Culture: subculture on the 16th on bouillon, agar. The bouillon is positive on the 21st, the agar on the 22nd, by 2 gray colonies. On 26.4.38 we notice on this agar the appearance of small rose-colored colonies, mixed with the gray colonies. These very small colonies have never exceeded 1 millimeter in diameter.
A subculture of these colonies is carried out on bouillon on 28.4.38, positive 7 days later, le 5.5.38, small tufts or flakes.
This bouillon subcultured on two agars on 5.5.38 produces 2 days later very lightly rose-colored cultures, this time united.
The microscopic examination of a sample shows short rods.
The grey colonies are subcultured on 29.4.38, on agar 30.4.38; a positive grey result with a tendency toward rose reflections; subcultured on 2.5.38 on albumin agar, result positive 3.5.38. Grey culture subcultured also on agar on 2.5.38, result positive on 3.5.38.
The first bouillon of 20.4.38 subcultured again on agar on 29.4.38; resultat positive on 30.4.38; grey colonies which are almost transparent with yellowish reflections.
The stock is subcultured on 29.3.38 [sic] on agar; result positive on 30.4.38 as a grey culture.
- 15.4.38 Preparation T: Formula: blood bouillon KCl both put in (two passes thru?) autoclave and left at room temperature.
Cultures: one of the tubes is subcultured on 16.4.38 on bouillon and agar; dubious result three days later.
The other tube is subcultured on 20.4.38; result positive on 21.4.38.
- 16.4.38 Preparation 13: Formula: pt p 12 2 tubes left at room temperature.
A microscopic examination was made immediately after the mixing. At 2000x magnification we see the formation of organisms que of small particles of carbon continuing to infiltrate. Rods and smaller cocci accompany larger organisms.
This symbiosis of different forms is seen elsewhere in most of our complex preparations. Notice that the large and infiltrated elements—sometimes giant-sized and calling to mind the red blood cells—always clearly aerobes, presenting a fixed circular form, lengthening only at the moment of division, but in an ellipse always wide, whereas the small organisms that are indifferent towards oxygen, present a very remarkable pleomorphism, transformations sometimes being made on the microscope slide in the course of several hours. One cell which had been noted as infected because of long filamentous rods, no longer showed a single rod two days later, but rather only cocci.
Cultures: subcultured on bouillon agar and albumin agar on 20.4.38, result positive on 21.4.38. The usual grey cultures.
- 2.5.38 The primary tube of P13 was subcultured only on 3.5.38 on agar, result positive 4.5.38.
- 16.4.38 Preparation PK 15: Formula: pk KCl kr bouillon hémostyl p12, one tube left at room temperature.
Cultures: a bouillon agar and an agar [inoculated] on 20.4.38, positive on 21.4.38. A new agar was inoculated from the stock on 3.5.38, result positive on 4.5.38. Grey culture, yellowish, almost transparent.
- Control. On 3.5.38 a tube containing simply kr and bouillon plus blood, but without KCl, established 11.4.38 was subcultured on agar on 3.5.38. At the end of five days, a negative result.
- 2nd control: A tube from 11.4.38 containing sterile KCl and nonautoclaved blood drawn in a sterile manner, was subcultured on agar 6.5.38, produced a positive result 7.4.38 ((7.5.38?)). Macroscopically: large round colonies.

CONCLUSIONS

From this series of two months of experiments, under the strictest possible conditions of sterility, the results are that:

- 1) All the preparations made with KCl, with the exception of one PK 14, cancer hemostyl, KCl, produced positive results.
- 2) Inversely, the only preparation which produced nothing contained all the elements of those with positive results, except KCl. A first conclusion, then, seems to be that it is KCl that plays the essential role in this phenomenon.
- 3) Without entering into the statistical domain, it seems that all the cultures tried out after the establishment of the preparations have only produced negative results or delayed results. The length of time for the culture to be carried out effectively is on the average from 3 to 4 days. When the preparation shows signs of autoculture (autoinoculation?) such signs are only shown in a similar length of time.
- 4) In any case, a rapid culturing, if sometimes it succeeds on bouillon, by contrast never succeeds directly on agar. The passage on bouillon being a necessary intermediary between stock and agar.

This phenomenon was particularly clear for the P 16, whose direct culture on agar, although made only 4 days ((after it was prepared)) produced only rose-colored colonies incapable of development. It was necessary to subculture one of these colonies on bouillon in order to obtain, 7 days later, a positive result, and to subculture it again on agar in order to obtain an appreciable result.

It results from these observations, that the materiality of the facts seems to be established. The probabilities that foreign infections or a technical error, given invariably the same result in over 60 operations, are practically nil, the samples (12 cases) shown to be negative.

It also results from this that a very small number of failures, still unexplained and which could be due to the ionization of the atmosphere; and temporary failures, or rather delay, in the appearance of the phenomena on certain culture media, renders illusory the notion that one experiment could have any value whatsoever concerning the materiality of the phenomenon. Only a methodical series like this one, spread out over several weeks, can have any significance.

Therefore there is good reason to consider any result insignificant if it is negative (for the reasons stated above) or if it is positive (an accidental infection being possible) if the result is attained by one experiment alone. From a statistical point of view, such a result can only be considered in the context of the sum total of all results. Nice, 7 Mai 1938"]

11 May, Reich to DuTeil (corresp box 10, DuTeil flds) „Ich komme endlich dazu, Ihnen einiges Grundsätzliche zur Situation sowie zur geplanten öffentlichen Demonstration der Versuche zu schreiben. Zunächst allgemein:

- 1) Ich bitte Sie sehr, meine Bestrebung zu unterstützen, klarzumachen, dass ich nicht Politiker bin, und dass die Angriffe und Verfolgungen, die ich erdulden muss, fast ausschliesslich auf Grund meiner naturwissenschaftlichen Arbeit erfolgen, und dass ich wegen meiner Jahre zurückliegenden parteiischen Tätigkeit niemals zu leiden hatte. Ich bitte Sie, mir zuhelfen, die Anschauung zu zerstören, als ob dieser ampf gegen mich etwas mit einer heutigen parteipolitischen Richtung zu tun hätte.
- 2) Übermorgen am Freitag halte ich einen Lichtbilder- und Filmdemonstrationsvortrag vor der hiesigen medizinischen Studentenschaft, die mich dazu aufgefordert hat, in den grossen Räumen des medizinischen Doktorenkollegiums. Meine Sache steh ausserordentlich gut. Von einer Verweigerung der Aufenthalts- und Arbeitserlaubnis kann wohl keine Rede mehr sein. Wir haben einen grossen Sieg davon getragen.
- 3) Ihren Brief an Herrn Dysthe habe ich gelesen. Es war nicht gut, wenn auch sehr wohlgemeint, dass Sie mich nicht genau über die Vorgänge in Frankreich dauernd unterrichteten. Aus Ihrem Brief hatte ich den Eindruck, dass die dortigen Autotritäten etwas anderes sind als die hiesigen. Gegen die Ausnützung der Namen habe ich ausdrücklich protestiert. Ich bitte Sie sehr, mir den Hergang der französischen Affaire mit Bonnet mitzug teilen. Ich schicke Ihnen in der Anlage einen Brief, den Bonnet an Dr. Gording schrieb. Da muss ich vor allem wissen, was mit Debres ist. Sie schreiben mir, dass er Identifizierungen vornimmt, doch Bonnet soll Dysthe erklärt haben, dass Debres nichts von der Sache weiss und nichts damit zu tun habe [knows nothing of the matter and has done nothing about it]. Es ist klar, mit welchen Mitteln gearbeitet wird. Doch bitte, ich muss über jeden Schritt, der getan wird, orientiert sein, wenn ich keine unnötigen Fehler machen soll. Von Bonnet, dessen Brief ein schlechtes Dokument ist, bitte ich Sie, einen Protokollauszug zu verlangen, damit wir die Möglichkeit haben, nachzuweisen, was er falsch gemacht hat.
- 4) Ich bitte Sie, mir mitzuteilen, wer jetzt mit Identifizierungen arbeitet, und ich schlage vor, keine 'Kontrollen' bisheriger Art mehr zuzulassen und den Standpunkt einzunehmen, dass es Autoritäten in diesem Gebiete jetzt nicht gibt, und genau so wie Sie sagen, die Sache monatelang erst studiert werden muss. Die Namen der identifizierenden Mitarbeiter halten wir absolut von nun an geheim. Sie sind doch wohl mit meiner Erklärung, die ich an die Presse gab und Ihnen zuschickte, einverstanden.

Ich freue mich sehr, dass Sie herkommen. In diesem Jahre ist so unendlich viel Entscheidenes passiert, dass wir uns unbedingt verständigen müssen. Darf ich Sie bitten, den Betrag, den ich Ihnen im vorigen Jahr geliehen habe, ich glaube, es sind im ganzen Kr 400.--, als meinen Beitrag zur Reise anzunehmen. Jedenfalls darf die Sache an Geldschwierigkeiten nicht scheitern.

- 5) Zur öffentlichen Demonstration lege ich Ihnen meinen Vorschlag bei. Schreiben Sie mir bitte, wen Sie noch von dort mitbringen können.

Ich danke Ihnen noch für die Bestätigung des Gelingens der Versuche 11-13. Ich habe manchmal das Empfinden, dass Sie aus freundschaftlichen Gründen meine Nerven schonen wollen. Bitte tun Sie das nicht. Es ist mir lieber, klare Wahrheiten zu wissen und Schwierigkeiten zu kennen, die man überwinden kann, als auch nur die geringste Illusion zu haben. War Cornijl bei Ihnen? Ich kann es mir nicht versagen, nochmals Sie zu bitten, sich nicht allzusehr auf die Arbeit und Überzeugung mit den sogenannten Autoritäten einzulassen. Ich kenne die Sache viel zu gut aus dem Kampf um Freud. Ich habe das ausserordentlich viel für meine eigene Arbeit jetzt an Erfahrungen gelernt.

PS Haben Sie die Zellen des Präparats 14 bekommen, die ich Ihnen vor einigen Tagen sandte?"

13 May, DuTeil to Reich (corresp box 10, DuTeil flds) [I am only taking a few moments to tell you that, if we have a little patience, we shall arrive at an interesting result a few weeks from now.

In fact, an intern of the Nice hospitals interested in our work, has arranged to come every day in the afternoon to work with us at my laboratory, and carry out himself according to my directions, any useful experiments. I am having him resume all the series of the experiments that I have made with great success for two months. I send you the summary of them here confidentially. [See 7 May report] If the results are conclusive, this intern, who would clearly make them his thesis for the Doctorate in Medicine, will do his thesis on the subject I suggested to him: explaining the "accidental infections" in surgery based on the results of our work. Moreover, he will ask Cornil, Dean of the Faculty of Medicine, who is already himself interested in our work and who saw the organisms under the microscope, to be the supervisor of his thesis. Cornil should then at this time make an official decision. He himself will then be obliged to come and attend very seriously to experiments. This will be in a few weeks because the supervisorship of the thesis is made on the basis of the choice of subject.

Furthermore, a woman biologist who works in bacteriology in a laboratory in Nice asked me if she could come work with us. The series [of experiments] that's beginning will be conducted seriously and at length by specialists, and if it produces the expected results, it will lead to official recognition for us by an indirect route.

This is all I have time to say today. I hope it will help you not to worry so much in the days to come.]

14 May 38, Reich entry in Bion lab notebook 3 ("Basic Research III, ms p. 88, OI box 7***): "Beobachtung!!!! Lava aus Island zeigt durch gehend bläsige Strukturen. Bei KCl zeigt starke elektrisch..." Says moss on the lava seems to be developing bionously from the lava bions themselves. The lava had been brought to him by Norbert Ernst, a former German Trotskyite contact, who had escaped to Iceland in 1933.

Monday 16 May, Reich to DuTeil (*Beyond* p. 152, corresp box 10, DuTeil flds) "1) On Friday I gave a lecture on the bion problem to several hundred Norwegian physicians who had been invited by the executive body of the local medical association. Bacteriologists were also present, including my opponents Kreyberg, Scharffenberg, and many others. I think I can say that it was an enormous success. Using eighty photographs and 250 meters of film, I was able to demonstrate that the work is deadly serious. We all agreed that at least 80 to 90 percent of those present understood what was involved. Our opponents [Die Gegner] kept quiet. I shall publish the text of this lecture together with the associated photographs.

2) In der Anlage schicke ich Ihnen ein Schnittpräparat eines Tumors nr. 87 von einer Maus, die mir S gezüchtet aus dem Blut eines gesunden Menschen gespritzt war. Ich habe hier selbst die Diagnose gestellt und werde sie von anderer Seite stellen lassen und bitte nun Sie, einen Krebspezialisten die Diagnose stellen zu lassen. Der Schnitt stammt aus dem rechten hinteren Bein und Glutealmuskulatur eines Maus, die einen etwa walnussgrossen infiltrierenden Tumor hatte. Die Sache ist, wie Sie begreifen, von absolut entscheidender Bedeutung. Ich bitte Sie, mit die Diagnose sobald wie möglich bekannt zu geben. Everything is going well here and we have learned a major lesson: for us, authorities cannot be regarded as authorities."

Weds 18 May, ("Research Notebook 1937-38" in OI box 8) Last entry in this notebook (on guinea pigs), before a break, with next entry "New York, 18 Sept. 1939, Arbeitsprogram")

23 May, Reich to DuTeil (corresp box 10, DuTeil flds) "Ich schicke Ihnen in der Anlage einen Brief, den ich heute an Bonnet schicken musste. Ich bitte Sie dringend um Aufklärung der Widersprüche, da ich sonst hier überhaupt nicht mehr weiter argumentieren kann. Bonnet soll auch zu Dysthe gesagt haben, dass Debré von der ganzen Angelegenheit nichts weiss, während Sie mir schrieben, dass Debré nicht nur interessiert ist, sondern sogar Proben von Ihnen nach bestellt hat. Ich kenn mich überhaupt nicht mehr aus und bitte Sie, mich restlos zu orientieren."

26 May, Reich to DuTeil, Emil Walter and Paul Neergaard (*Beyond* p. 152-53) "This is to let you know that I am accepting a very sensible proposal made by Dr. Paul Neergaard of Copenhagen. He wrote to me on 23 May 1938 to express his opinion that it would be much better if the public demonstration of the bion experiments scheduled for the summer was not

performed by me but by some other person or persons. I would therefore ask you to consider whether it would be possible for you to write to the local university, perhaps jointly, and arrange the organization of a public demonstration of the experiments. The medical faculty here already stated several weeks ago in a daily newspaper that this type of public verification should take place, but that it must be paid for by the supporters of the bion cause. Please let me know as soon as possible whether you would be willing to negotiate in my place with the local university.”

31 May 38, (bion lab ntbk 3, ms p. 94-95***) formula for “Präparat 15 ec [Egg medium production corrected to 7.4 pH.

- 1). Two to three crystals of potassium hydroxide are laid on a metal spatula and held in the tip of a gas flame until they are not only melted, rather begin a rolling boil and start to evaporate. As soon as possible, the liquid potassium chloride is inserted into the egg medium in such a way that this is slit open. The injured places in the egg medium immediately begin to liquify.
- 2). Under a microscope, a sample of the liquified egg medium shows the immediate formation of all bion types of Preparation 6, cocci, rods, and nucleated cells inside the egg medium.
- 3). About 5ccm of autoclaved bouillon and potassium chloride mixed in equal parts is added. The whole thing is then placed in the autoclave at 120° for half-an-hour.
- 4). The thick colodial, brownish-yellow solution is diluted in bouillon + KCl test tubes and put in the thermostat. All bion types can be observed.
- 5). A control test tube is used to determine when the movement and the disengagement of the cell constructions from the egg medium are amply advanced enough to perform the inoculations. After 2-5 days, several of the same types of egg medium (store horizontally) and bouillon are inoculated. The growths from the egg mediums are transferred farther onto egg or agar.

Variation: The egg medium completely liquefies without the addition of bouillon + KCl in the course of 2-3 days and only then diluted in bouillon + KCl.

[ms notes written below:] [Afterwards] mix egg medium to pH 7 with C-blood.

Potassium hydroxide [???

?? B + K diluted and corrected to pH 8.

Sample:

Justification: pH 8 egg medium is the [basis??] of Preparation 15. Too much KOH in the preparation means [from Life until death the same pH ???].]

2 June, Reich telegram to DuTeil (corresp box 10, DuTeil flds) “Erwarte dringend Bescheid Affaire Debre Bonnet. Gruss, Reich.” **2 June, DuTeil letter in reply to telegram:** “En réponse à votre telegramme reçu à l’instant, je vous envoie en communication, deux lettres de Monsieur Van den Areud Président du Groupe de Psychobiologie de la Sorbonne, où il me demande pontanément des renseignements et me dit avoir communiqué des échantillons au Professor Debré qui m’en demande d’autres.

Vous voudrez bien, après en voir pris connaissance, et copie si vous le désirez, me renvoyer les deux lettres.

Je ne puis faire davantage aujourd’hui, devant partir dans quelques instants pour Aix où j’ai des examens, auquel je dois assister. Je vous réécrirai après la Pentecôte.”

5 June, DuTeil to Reich (*Beyond* p. 154, corresp box 10, DuTeil flds) D has “been suspended from his post at the university because, it was claimed, his work with Reich, done privately, had involved the university’s name in a public polemic.” “I think you have received the two letters concerning Dr. Debré that I sent you before my departure for Aix. I would be very grateful if you could send them back to me when you have copied them. I especially beg you not to publicize them at all and to ask the press not to publish any of the names contained therein.

Just as I foresaw from the beginning, in requesting the utmost prudence concerning publishing any names, the press campaign launched in Norway just had grave repercussions for me personally, which I am sorry to be forced to tell you about right away.

The Council of the University of Aix-Marseille, upon which I am dependent, informed me through the Rector that they considered it very regrettable that the University Centre of Nice had been involved through my fault in a controversy in which its status was discussed. Further, the University Centre had appeared to be forced to conceal private work. Consequently—and the decision comes from the Minister—I have been suspended from my post and forced from duty without salary. In any case I will no longer belong to the University Centre as of the end of this month. I will write you with my new address.

There is no need for me to tell you, my dear friend, that the measure which strikes me is of an exceptional gravity for me and my family. Not? That I can regret, however, what I did in what I deem to be the interest of science and for our friendship. I regret especially to no longer be able to be a great help to you, except in a private or personal capacity (the current

experiments go very well). I still should [be able to], if I should manage before long to balance my material situation, which was already not so bright but which now suddenly collapses.

I hope they [Reich's enemies] won't find out about it too quickly in Norway, where if need be you could say that I am ill and have taken a vacation. In any case, this summer's trip seems very uncertain, and my presence there might perhaps do more harm than good.

In any case, don't take steps of any kind on my behalf. It would only aggravate the situation.

Believe me, my dear friend, in my affectionate regards for you and all of yours, RDT.

PS What sparked things off was that you wrote in *Die Bione* "University of Nice" instead of "University Centre of Nice." Since the seat of the University is Aix-Marseille, these two cities have taken up arms!]

"I will try to reply as calmly and factually as possible to your deeply distressing letter of 5 June. I thank you for the originals of the letters from von [den] Areud. We have copied them and I am returning both originals to you herewith. We will obey your wishes not to publish the names in the press. Doch die schwierige Situation angesichts der gemeinen Vorgangsweise der Gegner erfordert es unbedingt, dass wir wie bisher klar, offen und eindeutig die Tatsachen, wie sie wirklich sind, unseren Freunden in der Welt durch das Mitteilungsblatt bekanntzumachen. Denn nur in dieser Weise können wir am Ende die Gegner schlagen, indem wir nämlich deren Vorgangsweise aufdecken.

In diesem Zusammenhange muss ich Ihnen etwas mitteilen und Sie dringend um die Erfüllung einer Bitte bitten. Sie sollen Dysthe, wie sich jetzt herausstellt, dem Agensanden einer Clique, einen Brief von Bonnet vom 23.10.37 gezeigt haben, in dem von positiven Resultaten in Paris die Rede ist. Dysthe wagt nun die Behauptung, dass dieser Brief von Ihnen gefälscht ist. Sie begreifen den Ernst dieser Sache. Um klar und rein in jeder Hinsicht dazustehen, ist es unerlässlich, dass Sie mir das Original dieses Briefes hierher einschicken. Sie können sicher sein, dass wir davon ohne Ihre Zustimmung nicht den geringsten Gebrauch machen. Doch wir können nur mit den klaren Waffen kämpfen, die wir haben.

I would like to assure you personally, on behalf of myself and also a number of very important friends, that we are entirely at your disposal and you may count on us in all respects, if necessary even for financial support. It is vital for us to maintain solidarity in such a battle. But the situation requires that I speak very openly to you, drawing on all the battle experience and severe disappointments of the past eighteen and more years. Right from the start, I asked and warned you not to rely too much on universities and authorities. Ich kenne die Unsachlichkeit und Fähigkeit zu unglaublichsten Massnahmen seitens bestimmter Offiziösen nach allem Kampf um Freud. Man muss natürlich alles tun, um Autoritäten zu gewinnen, doch gleichzeitig hat man allen Grund, das tiefste Misstrauen zu ihnen zu haben. My error in the matter that now affects you so badly is that I failed to prevent one of our coworkers here [Havrevold] from seeking assurances from Paris to allay his own lack of certainty. Unfortunately you have not kept me informed fully and openly from the start, because if you had I would not have persisted in the view that decent work is being performed in Paris. But there is nothing that can be done about that now. You write that nothing should be undertaken from here. We will not do anything that you do not wish us to do, but in view of these very painful experiences I must ask you again to have a little more trust in my method of going about things. If I managed, by myself, to survive for twelve whole years simply on the basis of honest and open publication of the facts, despite going through some extremely bad periods, then a group of people of various types and widely differing backgrounds should now be able to stand fast, but only under one condition [Bedingung]: we must hold true to and never deviate from the line of openly declaring our scientific facts, and we should above all abandon any belief in the goodwill of academic authorities. There is only one way for us to go, and many people are saying this: we must arouse the conscience of public opinion against all the behind-the-scenes maneuvering and dishonesty of a few people in high places who are particularly adept at this kind of behavior.

The arguments with which you were dismissed are fully in keeping with the dreadful methods that such cliques use against any kind of serious work. There is no sense describing to you in detail how I felt when I heard the news and how deeply touched I was by your almost superhuman decency [Anständigkeit] in trying to show consideration for me in this situation. Please direct this consideration elsewhere, in the interest of our cause [Sache]. There are many decent truth-seeking persons in the world who know nothing about us. They can only give us their support if the waves created this time by all the nastiness finally reach them and bring us to their attention. I am firmly convinced that we would be putting ourselves in serious danger if we stood by quietly and considerately, merely observing what was happening and leaving the field entirely clear for the meaner elements to do their dirty work. Please keep me fully up to date on what actions you are considering at your end and what impact the matter has had on your friends and on people in general.

Ich bin auch garnicht einverstanden damit, wenn Sie glauben dass Ihre Gegenwart im Sommer mehr zum Bösen als zum Guten sein würde. Im Gegenteil: Gerade nun sollen Sie im Sommer bei der öffentlichen Demonstration, ob sie nun von der Universität veranstaltet wird oder privat von uns, anwesend sein. Es ist nicht unbescheiden von mir, wenn ich Ihnen sage, ein Wissenschaftler hat allen Grund, stolz darauf zu sein, dass ihm so etwas passiert wie Ihnen jetzt. Das beweist ja nur, wie recht wir haben. Ihre Hilfe in der Sache bleibt objektiv die gleiche, ob Sie nun offiziell von einer Universität aus oder privat arbeiten. Denn entscheiden tut doch nicht der Titel, sondern die Sache.

For the time being I will not undertake any official action until I have convinced you that open battle is the best way and until I have received from you all the material that will enable me to make the correct moves. This material includes, above all, the letter you received from Bonnet on 23 October 1937.

I urge you to accept my invitation and come to Norway as my guest as soon as you can get away. You do not have to have any fears on my account. As things stand at present, my enemies are seeking ways to force me out of the country, but they will not be successful.

PS When official agencies repudiate our work, that is all right. But when other official agencies confirm our work, that is wrong!!!! Objective science.”

Reich to DuTeil, 13 June (corresp box 10, DuTeil flds***) [“I urgently ask you for the quickest possible completion of the following:

- 1). In the interest of the issue, it is absolutely critical that I obtain the original letter from Bonnet to you dated October 23, 1937. Everything depends on this letter.
- 2). Please send me a short but sufficiently informative description of all visits, requests, commitments, cancellations, that you have had since October of last year. We urgently need all original documents that you have concerning the bion issue.
- 3). A description from you personally telling how you evaluate the matters in Paris, naturally based on objective documentation. Finally, a synopsis of the work that you personally carried out.

We are slowly coming to the realization of who has caused this affair. Please, put all consideration aside because this is a gigantic case of abuse. The man should not be spared. I will send you details as soon as everything is together.

Your documentation concerning the connection with Debrè worked wonders here. Do not keep anything back out of false concerns. Everything is at risk. I expressly ask you again to send me the Areud letters. You will get everything back as soon as it has been copied.

I emphasize: A crime has been committed, a full-fledged criminal act. You understand! Please act quickly!

Write how your situation has further developed. I believe just as little as our friends that the issue in France, your suspension, will proceed quietly.

Courage and patience! We will get through this, but only with relentlessness!]

13-14 June 38, Reich to N. Hoel, S. Hoel, O. Havrevold (correspondence box 9,”1936-39” flds, 8pp*)** “The continuing attacks from people, who attempt to appear as experts, demand several factual and several personal clarifications. I shall repeat my statements from Friday's conference:

- 1.) These “experts” are laymen in our field. The psychiatric specialist, for example, has no inkling of the central role that sexual dysfunction plays in one schizophrenic process.
- 2.) Furthermore, these “experts” display a horrifying lack of knowledge in their own fields when they attack our work.

In the following, I have collected a couple of facts that I have long been considering for a detailed publication. Please use them as you feel best. It is a matter of fact that I may not and will not respond directly to critics who slander and personally insult me.

I. Factual

1. The “physiologist” Hoffmann declared my basic hypothesis of the dichotomy of the vagus and sympathetic nervous systems and the corresponding sensations to be absurd. This shows that he has no idea concerning the past 10-15 years of research on the vegetative nervous system. I did not invent the vegetative function's dichotomy, rather simply pointed to it as a well-known fact in my orgasm thesis and used it upon which to base physiological experiments. The only thing I actually accomplished with this is proof of an increase in electrical charge on the periphery of the vagus and equaling sexual reaction. You will find some quotations from the world's current written authority: Müller, *Lebensnerven und Lebenstrieb*, from which you can determine just how uneducated Hoffmann is.

2. Hoffmann claims that the catatonic's nipples showed no difference in potential when compared to the rest of the skin surface. A purely technical comment: If one firmly sticks about 2 cm in diameter bell-shaped electrodes to the erogenous areas one wishes to measure and does nothing more, then of course there will be “no difference.” I provided a detailed description of the technique in my publication, *Experimental Results...* One must “tickle” next to the point electrode using non-conducting cotton. Only then can it be determined whether a

catatonic's nipple shows the same positive reaction as a healthy person's. If it doesn't react, the question is still open whether this signals that the catatonic has a poor ability to accept charge in the erogenous areas. This should currently be examined in Dikemark. In healthy people, the erogenous areas can be at the same level as the rest of the skin surface or be a substantial increase beyond.

3. According to Hoffmann, the skin potentials are simply an expression of the difference in potential between tissue fluids and the electrode fluids. Writing something like that means the following: One applies a series of complicated tests in order to measure the human prime potential to determine the function of electrode fluid!!! For what purpose is one even measuring the prime potential? That could even be determined on non-living membrane systems. This speaks against it: the cardio wave that one observes on the primary potential proves alone that the heart's electric current is transmitted from the center of the body through peripheral fluids and expressed there. I would especially like to point out that as soon as I heard those completely irrelevant objections coming from Hoffmann, I initiated the "indirect discharge" in order to completely invalidate his objections (page 30, *Experimental Results...*) Using this discharge method, only electrical stimulation on the tickled area is expressed, which lies in an entirely different region than the stationary electrode. Hoffmann does not know this fact, although it should interest him as a professional. Hoffmann does not know that below the "skin" membrane lie millions of membranes inside the body working together as a complicated electrical relay system. Professor Kraus in Berlin based his "Fluid Theory of Life" on this. The whole organism, thus, is a membrane/electric system. The body's electrical response can oscillate through the nerve system as well as through body fluids ('ion envection'). Likewise also from the organ center to the periphery and vice-versa. Waves 28 and 29 were taken under the same electrode-fluid conditions. Both with indirect charge. In one case the arousal increases; in the other case the same contact with the lips produces a downward sloping wave. In the second case, the kiss was not passionate. From this one can see the complete instability of "expert" opinions.

4. One expert in physiology finds it especially amusing that I consider an orgasm an "electrical discharge". To this I respond from a purely physiological viewpoint. Every muscle twitch is accompanied by electrical charge. The orgasm consists of contractions (compare Müller p. 673) not only of the genital muscles but also of muscles in other parts of the body. In these statements, I have, therefore, merely compiled facts already noted in physiology and applied them to my sexual economic theory. In photo 21 in table 6, one sees the rhythmic electrical discharge by ejaculation. It is a picture from the experiment that was witnessed by Hoffmann, Löwenbach, and Schjelderup without them recognizing what was on the photo. The only picture that exists from this event was taken in their presence. Hoffmann's statement that this experiment brought no result is, thus, a lie.

5. Hoffmann tries to make an impression by saying that the Eindhoven Triangle "explains" the difference in the location of the electrocardiogram's waves. That is not correct. The Eindhoven Triangle is merely a sketch showing the different discharge possibilities of electric cardiograms and is not a scientific explanation of their appearance. Hoffmann's explanation demonstrates that he does not have the slightest idea of the difference between a scientific description and a scientific explanation.

6. The psychological affects resulted in only a decrease in the prime potential. the so-called "psycho-galvanic phenomenon" (Tarchanoff). It is correct that only a decrease in the prime potential has been depicted up to now. This can be explained solely by the fact that neither the tickle phenomenon, nor the erogenous sensations in the erogenous zones, nor the vegetative flow in orgasmic sensations have been measured. Because sexual affects have never been measured, one has either never seen positivities or, if they were seen, not correctly interpreted. Positivities, as I reported dozens of times in my writings, cannot be artificially produced, meaning that there are also unquestionably no 'sources of error'. The vegetative sensation of pleasure is the only 'sensation' that results in a steady, physical, wave-shaped charge-intensification curve. That this is possible can be explained by the facts, unknown to my opponents, that an organism is a complicated membrane-electrolyte system in which electrical energy is concentrated or distributed all around, respectively.

7. Hoffmann and Löwenbach claimed that a moist cloth would give the same phenomenon. Picture 31, table 10 in my publication shows a negative deflection when connecting two non-polarisable liquid electrodes through a moist cloth. Schjelderup, Löwenbach, and Hoffmann saw this deflection during the so-called experiment evening at my residence. Positivity neither happened nor was recorded then. Hoffmann's incorrect claim is in reference to the

control experiment in Dikemark. In the course of this, Hoffmann had laid the different electrodes used for measurement alternately on the palm of a catatonic and on the moistened sheet, then tickled along side it with cotton. In both cases, physical tickling fluctuations presented. Löwenbach as well as I had overlooked the fact that the neutral 0-electrode had not been removed from the leg. This is very easy to prove: It would otherwise not have been possible to lay the measurement electrodes in constant alteration on the hand and sheet. Thus the infamous “living cloth” was born. After Hoffmann and Löwenbach resigned, I repeated the experiment dozens of times without attaching to a body and never saw the tickle phenomenon or even any positivity on the moist cloth. This experiment can be carried out easily and at any time. This fact is also published in my brochure on p. 34. I declared the remarks from Hoffmann, Löwenbach, and Schjelderup concerning this issue to be indiscriminate, unproven, and totally incomprehensible errors.

8. Pressure on the plexus and change in the prime potential above the navel. Löwenbach was the first to notice the decline in potential above the navel when inhaling deeply and when squeezing as during defecation, during the initial experiment. Neither he nor I understood the occurrence at that time. I first understood this phenomenon much later when I discovered the orgasm reflex and began to assess the role of breathing in the vegetative function of the abdominal ganglia. That I was able to create such a connection is in itself enough proof that our viewpoint about the uniformity in the overall functions of the vegetative apparatus is correct, and that we are, thus, the experts here.

9. During my experiments after October 1935, that is following the resignations of Löwenbach and Hoffmann, I used the electrodes they prepared, where I did not use the indirect discharge method that I developed myself. I had learned the operating technique for the oscillograph from Löwenbach. I had three different specialists determine the apparatus' operating methods.

Personal:

1. Löwenbach himself determined the positivity during arousal during the initial start of the experiments – as in the case of the experiment with Nic, for example and explained the appearance of these positivities to me.
2. Hoffmann claimed that Löwenbach, whom I paid a monthly salary of 260 kroner, was working for a Rockefeller grant.
3. Both Hoffmann and Löwenbach agreed with the final questioning in “ur-contradictions”. [could not find a translation I liked-I'm pretty sure this means that they agreed to a final discussion after they had been fired]
4. Hoffmann did not quit the experiments, rather I stopped working with Löwenbach and Hoffmann when I saw that they were falsely directing the experiments, a problem from my standpoint. Compare included letter to Schjelderup.
5. When Hoffmann writes about a “circle of ignorant laymen” with whom I have surrounded myself, we must very strongly state that everyone who works in our circle is an expert in some respect.
6. Löwenbach, who should remain here at the moment, is a half-Jew and was fired from the Kaiser-Wilhelm-Institute in Berlin. He was paid for six months by me and displays a national-socialistic conviction that explains a lot in this situation. I must end with a concern that very much distresses me: Some of our friends show the tendency consider our opponents authorities when that they are not and to deny us the authority, which we fully deserve. This results in the attempt to seek the verification for our work from a different party. Based on very painful experiences, I may warn against this. Whoever does not understand our theory of sexuality will not in the least little bit understand our physiological, clinical, and biological work. I find myself unable to respond to all of the irrationally justified arguments, both personally and in terms of time. The includes the insults that accompany them. This does not mean that I am overlooking the objections. Just the opposite, I collect all of them and will, as always, respond to them point by point in a later publication to clarify all dissent that can be clarified.

PS By the way, the brochures orientate in detail the experiments and control experiments.]

14 June 38, Raknes to Allendy (corresp box 10, DuTeil flds***) [“I thank you cordially for the beautiful volume of ‘Explained Dreams’ that you had the honor of sending me. I’m sorry to have been able to read only the first two chapters so far, but these as well as the general plan of the book have made a very good impression on me. I’ll tell you more about this when I have finished the reading if, as I hope, I’ll be able to come to the congress we shall have a chance to talk about it.

Today I'm writing you about another piece of business. You will undoubtedly remember the lecture given in Paris by Mr. Roger DuTeil on spontaneous generation about which you spoke to Mr. Guy van den Areud at the beginning of February. On 8 February Mr. van den Areud wrote to Mr. DuTeil and obtained a test tube from him. In light of the research that he was proposing, Mr. van den Areud addressed himself to Prof. Robert Debré of the Faculty of Medicine. On 8 March he wrote again to Mr. DuTeil because 'Professor Debré asks me to ask you if you will kindly send us some more samples of "bions."' Since then, here in Oslo we have had no information on the fate of these samples.

For about two months, a lively discussion has begun among the Norwegian doctors and biologists, on spontaneous generation and the experiments of Dr. Reich and Mr. DuTeil. In this discussion, it would be very interesting to know the results of the research done by Mr. van den Areud and Professor Debré. You would do us a great service if you could obtain from Professor Debré precise information on the following questions: 1) Have the tubes sent by Mr. DuTeil to Mr. van den Areud been examined by Professor Debré? If yes, 2) by what procedure were they examined? 3) Can we have a copy of the protocol? 4) Did the test give vague or precise, positive or negative results?

I know only too well that it is a lot to ask of you, but the matter has become so important that even so I dare to ask. If you can't obtain this information or you are forbidden to try, I ask you to let me know as soon as possible—and to have the kindness to excuse my audacity.

Awaiting your reply, please accept, dear Sir, my very devoted sentiments."

15 June 38, DuTeil to Reich (corresp box 10, DuTeil flds***p 1) ["I thank you for your telegram and for your letter, received during exam period and which I don't have time to go deeper into.

I shall tell you here only that I am very discouraged. Certainly if you think my presence beside you in Oslo this summer is necessary, I would still be willing to grant it to you. But unfortunately the question arising for me today is not to find out what I will do this summer, it is to find out if I will still be alive this summer and how I'm going to be able one week from now to feed my children. My situation is tragic and is still worsening. The private works that earned me my official position and contributed to my livelihood were taken away from me as well, since they know that I am no longer at the University Centre. About 10,000 francs, which would have enabled me to live this summer, has suddenly disappeared in this way. I don't exaggerate in saying that at the end of this week I will no longer have anything, because all this year's courses are over and I can't anticipate anything from the next semester's until the month of November. And this thunderbolt was so unexpected that I may not even be able to return [to teaching]. Already for several months the expenses of my work have put me into debt and I have had to take on some very heavy financial commitments. At Nice, with this history, I can no longer find one cent of a loan, thereby losing any hope of making up my losses.

Forgive me for telling you all this, but it is necessary. If I don't have by several days from now about 10,000 francs allowing me to attend to the most urgent things first and to organize the summer for me and my family until the fall term begins, I don't dare predict what will happen to me and my family. If one of your friends around you agreed to lend me the francs, it being understood that I would pay them back as soon as possible, I could manage and continue to struggle. This is my last hope. Let me know right away about this possibility.

Two or three days from now, as soon as the exams are over, I'll reply to various parts of your letter. Here and now, I stress the stupidity of the hypothesis of a supposed document that nobody ever saw and which I never used! Why bother?

Believe me, dear friend, in my affectionate regards."]

16 June, Reich to Schjødt (*Beyond* p. 156-58) "With reference to our last conversation, the following is a summary of my general report on the affair that is still pending. In August 1937 Dr. Roger DuTeil...worked for fourteen days in my laboratory in Oslo, performing some experimental procedures, and he took back with him to France a number of preparations, films, and descriptions. According to the reports he sent me in his letters, the films and preparations were demonstrated in Pontigny and in Paris. It was his impression that the presentations were received in a positive, wait-and-see manner. Control experiments were to be carried out at several laboratories, including the laboratory of the Medical Faculty, which is headed by a Professor Debré. His assistant is Dr. Bonnet. Sometime around 4 October, DuTeil was staying in Paris with Bonnet, who had promised to look into the matter. In response to a specific request from me, DuTeil informed me that the work in Paris was still going on and had yielded contradictory results. I waited several months without making any further inquiries. In November 1937 the first campaign against my work started in Oslo with five meetings at the Psychiatric Society led by Professor Vogt. One of the financial backers of the institute [Lars Christensen] was bombarded with rumors about me from an unknown source in Denmark. Dr. Havrevold asked me to name the laboratories in France at which the control studies were being conducted. After some hesitation, I told him the laboratories and gave him Dr. Bonnet's name. A certain Roald Dysthe, as I later found out, then traveled to Paris and to Nice. Bonnet supposedly told him (this is what he reported in a meeting attended by Professor Schreiner, Dr. Gording, and Dr. Havrevold) that Debré knew nothing of the bion work and that he, Dr. Bonnet, ought to know, because everything for Professor

Debré passed through his hands. This took me completely by surprise because DuTeil had written to me from Nice that Professor Debré had (and I quote) ‘ordered more’ bion cultures. In addition, Dysthe had reported that, at his urging, DuTeil had shown him a letter from Bonnet dated 23 October 1937, i.e., about three weeks after DuTeil had visited Bonnet. According to Dysthe, this letter contained confirmatory information about the work in Paris. But Dysthe had the impression the letter was a fake. He deduced this from the differences in the signatures: the letter from Bonnet to DuTeil had a signature in tiny letters while a letter from Bonnet to Dr. Gording, in which he states that he had obtained ‘only negative’ results, was signed with a larger signature. The accusation that such a decisive letter was a forgery is a matter of great consequence.

The attack by the psychiatrists failed. In March, the first provisional publication on the bions appeared. It was around then that the second campaign got underway, although this time it was based not on my psychotherapeutic work but on my biological research. It was in this connection that the articles known to you appeared and the attacks by Dr. Kreyberg were published. The article published by Dr. Havrevold in *Dagbladet* and *Arbejderbladet* contains the most important facts needed to refute Dr. Kreyberg’s assertions.

At a meeting of the Socialist Physicians Association, Dr. Kreyberg stated that he had been in Paris, had talked to Bonnet in the presence of Debré, and Debré had merely laughed at the whole business. I do not know what Kreyberg said when he was with Bonnet, but quite a bit can be deduced from his behavior here. I immediately wrote to DuTeil that he should try to clear up the Debré affair for me. I then received two handwritten letters [8 & 18 Feb.] from the president of the psychobiological department at the Sorbonne [van den Areud] that show clearly that Professor Debré not only was interested in the matter but had expressly ordered more bion cultures and in fact had made very precise inquiries about the nutrient media, etc. I later answered these queries through DuTeil. Once I had this proof of the accuracy of DuTeil’s statements in my hands, I wrote a letter to DuTeil asking him to send me all the material, in particular the letter from Bonnet to him. So far I have not received a reply. An objective analysis of the facts permits only three possible conclusions. 1. I am a swindler, as Scharffenberg and Kreyberg claim. I am willing to submit all the documents to a court and also to carry out the bion experiments in public. 2. DuTeil is a swindler and a forger of letters who claims to have received positive reports from Bonnet, although this was not true. 3. Bonnet first wrote a partially confirmatory letter to DuTeil and then a totally negative letter to Dr. Gording.

Bonnet should himself have made an exact record of the experiments, but he did not do that. Nor has he so far replied to my query.

Nonscientists, again for reasons unknown to me, have intervened in a scientific matter and created confusion. This is particularly true as far as Dysthe’s slanderous attack on DuTeil is concerned. It is possible, at least judging by what has already happened here in Oslo, that Dr. Kreyberg, by virtue of his official authority in Paris, has without basis created a situation I do not understand but one that poses difficulties and dangers to our work. I believe I am right in assuming that these actions are the reason Dr. Roger DuTeil was suspended without pay, as he recently wrote and informed me.

These, in brief, are the facts. I would ask you to use all legal means at your disposal to seek clarification of this matter, which is assuming a criminal character.”

18 June 38, DuTeil to Reich (1 pp***) [“One word only, because I am still in exam period: to return the two letters from van den Areud, to which I add a third that I found again and which is inserted between the two you had, to complete the correspondence.

I add that I never sent other samples as they asked me to do, because my experiments weren’t ended and I was waiting to have some characteristic cultures. Tell me if you think it would be necessary for me to send some now.

I also add to this sending a retrospective note about the experiments that I made in my personal laboratory during the months of March and April. You could make a copy of it and return this copy, which I might need, to me.

I shall write you at greater length in a few days when exams are over. To you and all your friends, very affectionately, RDT]

20 June, Reich to DuTeil, Emil Walter, Paul Neergaard (*Beyond* p. 159) “On 24 April 1938 the Medical Faculty in Oslo announced publicly in the *Dagbladet* that it would be a very good idea to investigate the bions. Dr. Neergaard proposed that instead of a private commission, which would have little effect, an official agency should be appointed to carry out the verification. In consultation with Dr. Walter, Professor DuTeil, and Dr. Neergaard, Dr. Havrevold and Nic Hoel wrote a letter to the Medical Faculty in Oslo on behalf of the Institute for Sex-Economic Research. We have now received a reply dated 17 June, which says that at a meeting of the faculty the following decision was made: ‘The Medical Faculty is not convinced that the experiments which are to be verified exist in such a form that they can receive serious scientific consideration. The faculty therefore sees no reason to appoint a member to a control commission.’

Nun bleibt noch immer die Möglichkeit einer privat durchgeführte Demonstration. Ich bitte darum, uns mitzuteilen, ob diese private Demonstration stattfinden und die Teilnehmer als eine Art Vorkommission zur Vorbereitung einer späteren offiziellen Kontrolle zusammentreten soll.”

21 June, Reich to DuTeil (corresp box 10, DuTeil flds***) “Ich bestätige Ihnen dankend den Empfang der Originalbriefe von Areud und des uns sehr Wertvollen Protokollauszuges über Ihre Versuche. Ich kann Ihnen jetzt nur ganz allgemein sagen, dass Ihre Detailangaben über den Zeitpunkt der Kultivierbarkeit ja sogar über die Art der Übertragung der Präparate auf die Nährböden mit meinen protokollierten jedoch noch nicht publizierten Erfahrungen fast bis auf die Tageszahl übereinstimmt. Dasgleichen teile ich Ihnen mit, dass ich einen neuen Versuch seit etwa zwei Moonaten durchführe, der unsere Behauptung von der ausschlaggebenden Rolle des Kalium restlos bestätigt. Es ist gleichzeitig ein Versuch, der das komplizierte 6er-Präparat ganz kolossal vereinfacht und dessen Kultur sichert.

Nun aber muss ich Sie noch einmal dringend bitten, [extra emphasis in original] und das wichtigste aller Dokumente, um das ich Sie schon zwei Mal gebeten habe, umgehend einzuschicken. Wenn eine Dokument uns hier nicht zugänglich wird, dann können wir folgende Behauptungen der Gegner [claims of the opponents] nicht widerlegen:

- 1) Dass in Paris nur negative Resultate waren, während Sie mir am 17.11. vorigen Jahres schrieben, dass widersprechende, also auch positive Resultate vorliegen.
- 2) Wir können die Wirkung der Publikation des Briefes von Bonnet an Kreberg nicht entkräften.
- 3) Wir können nicht korrekt gegen die Verleumdungskampagne auftreten, die Sie betrifft, nämlich, dass Sie einen Brief gefälscht hätten. Ihre Bemerkung vom vorletzten Schreiben genügt in keiner Weise, um das zu widerlegen.

Ich brauche ferner dringend einen genauen Bericht nicht über die Personen, mit denen Sie in Verbindung waren, die habe ich ja aus Ihren Briefen, sondern über die Resultate der verschiedenen Besuche, die Sie empfangen. Auch ich habe es ja immerfort erlebt, dass Offiziöse mich begeistert aufsuchten, jedoch nicht mehr zu sehen waren, wenn wir von einer Meute von Gegnern überfallen wurden. Ich bitte Sie, nochmals zu überlegen, dass jede Rücksicht in dieser Situation für mich und damit für uns sowie für unsere Arbeit nur katastrophal werden kann. Ich erwarte also mit Ungeduld den Brief von Bonnet [an Kreyberg?] 23. Oktober. Bitte geben Sie mir auch schriftliche Bestätigung darüber, dass Sie Proben des Seko (?) oder Bluko (?) Präparats an [lebe?] geschickt haben. Senden Sie bitte vorläufig keine weiteren Kulturen, ehe nicht die Äusserung von Debré vorliegt. Ich erinnere an den Widerspruch, dass Kreyberg behauptet, Debré wusste nichts von der Sache und lachte nur, während aus Dokumenten hervorgeht, [bottom line partly cut off:] “_____ Kulturen in Händen gehabt und damit arbeitet.”

21 June, DuTeil to Reich (corresp box 10, DuTeil flds, 2 pp***) [I thank you for your letter of 17 June, which arrived just this morning, and for the hope it gives me of having aid from Oslo, aid that has become indispensable...because I no longer have any courage. The eadline is tomorrow, Tuesday, by noontime. I am counting the hours, hoping for the liberating telegram at each ring of the bell.

It is necessary on the other hand for us to reach an agreement defiitely, concerning a document to which you seem to attach an unwarranted importance. This document might produce a negative effect, moreover. By keeping certain documents in my possession without communicating to anyone in the world, I really acted with responsibility which is not fragmentary but global (complete), and because I am the only one able to judge whether communicating them—even to you—might be useful or harmful. The event that just happened confirms that I was not wrong—since it is a publication from Oslo that set off the press campaign of which you know the unfortunate, if not tragic result. Actually, I alone can judge the repercussions in France of such documents, and it is my imperative duty—even in your interest—to ward off the dangers we would all risk because of the ignorance you have of the reactions in France.

Now what you are talking about is a classic example of this necessity. Admit for an instant that what they reported to you is exact and accurately interpreted, that is to say that one day a researcher informed me personally and confidentially that he obtained a certain positive result, among several others that were negative, but asked me not to take account of it until he had verified statistically; that subsequently these statistical experiments, if he made them, were negative—and that he then concluded that the sole positive result stemmed from an error, included moreover, in the percentage of accidental errors seen in all laboratories (let's say 2% to clarify matters). That he decided then to continue, trusting in his personal discretion, he purely and simply replied ‘negative,’ without entering into the details and about what he might consider an error of detail from his laboratory. I sympathize wholeheartedly, you see, with your hypothesis in the most favorable way; and I wonder if you think one can deny such a researcher the right to do as he did—and if you don't think it would be both highly incorrect and useless, even dangerous, to make use of a personal fragmentary confidence made in the course of work to try to make this person appear to contradict himself! Incorrect and even worse, when one knows the risks to which one would expose the person in question, considering the actual tone of the arguments; as for me, I am incapable of envisaging such a thing. Useless because the person would continue to hold to his stated position, or might well increase its denial in order to explain it. Dangerous, because a new argument might immediately be added to the first one, producing new repercussions in France, where I need a few weeks of silence in order to reestablish myself.

But all this is only a hypothesis made by you based on the report of a certain M. Dysthe, who passed by my house in a gust of wind, doing what he could and interpreting as he wished. You say that you had some ‘partial’ knowledge about a document. You should have said that you had no knowledge of such a document, for I never spoke to you or anyone else about such a document. The awareness of such a document that Dysthe claims to have had is nonexistent, since he gave me his word not to take my dossiers into account, and I fail to understand how he therefore could have referred to it. If a document might have interested you or been useful to you, you know that in due time I would have personally communicated it to you directly and would not have needed M. Dysthe to make him see it. I fail to understand, moreover, how one result alone—refuted subsequently as they tell me—can be useful and can not, on the contrary, be dangerous—for case that only statistics can prove.

There is the question finally settled, my dear friend. It shows the danger of intermediaries and also the danger of too great a confidence in the work of people you have never seen. It is because I was distrustful that I telegraphed you to ask your information about Dysthe. Unfortunately, the information arrived a little late and was insufficient.

All this is perhaps quite useless, for time passes and I wait in growing anguish. In any case, believe in my good affection.”

22 June, Reich wires DuTeil Kr.700; DuTeil telegram to Reich “Merci. Victoire viendra.”

22 June, (or shortly after), Bion lab notebook (“Basic Research III, ms p. 133-135, OI box 7): “Notwendige Annahme [Required Assumption]: Degener→ Reihe→ hohe Entwicklung...”

23 June, Raknes to DuTeil (box 10, DuTeil flds) [I thank you for your letter of the 19th that I only received yesterday morning at the same time as your telegram, to which Dr. Reich replied by sending you a sum of money. At this time, it is very hard for any of the members of our group to help you in a material way, each one being burdened with as many obligations as he can fulfill. And among us friends, it is impossible, so to speak, to obtain help at the time of this press campaign by official science against Dr. Reich.

At Dr. Reich’s home I just read your letter [of 21 June] in which you discuss the hypothesis of the documents which have not been produced. Speaking frankly, I think you are wrong not to let Reich see the documents he asked you about—it is neither right nor possible that you take all the responsibility for the outcome of these experiments. I would undoubtedly respect your loyalty toward M. Bonnet or whomever it is, if he or they make a point of showing the same loyalty towards you and toward Reich. In the present state of affairs I see only a communication to Reich of documents concerning him, and which could be considered a disloyalty. And I must add that the hypothetical discussion in your letter seems to me to be of very questionable value, at a time when it is facts and nothing but facts that are important.

The three letters which you have let me see, I am returning together to you. Please excuse my frankness again, and believe, dear Sir, in my very honorable sentiments.

PS June 24. I just received a card from Dr. Allendy, who wrote to Prof. Dubre. The latter replied very briefly, according to Dr. Allendy: ‘Mr. Debre does not seem to be at all interested in the experiments and there is nothing positive to expect from him.’]

23 June, DuTeil “Réponse a la Communication du 20 Juin 1938” (1 p) “Le projet de Commission préparatoire contenu dans la Communication du 20/6/38 semble souhaitable dans son principe et je l’approuve en ce qui me concerne.

Il paraît toutefois indispensable que cette Commission comprenne des représentants des principaux pays d’Europe (ou plutôt un représentant de chaque pays) Ce représentant étant choisi parmi les spécialistes de la biologie ou de la bactériologie, étant bien entendu que chacun d’eux ne participe aux travaux qu’à titre privé et sous sa responsabilité personnelle.

En ce qui concerne la France, je suis bien entendu tout disposé à venir et il m’apparaît même que ma présence est indispensable, puisque je suis le seul à avoir mené les expériences de contrôle véritables, c’est-à-dire statistiques. Mais il me paraît utile, étant donné que j’a été personnellement attaqué à propos de ces expériences qu’un autre Français fasse partie de la Commission, don’t l’un des rôles doit justement consister à contrôler, d’après les dossiers, mes propres expériences de contrôle. Je ne puis être à la fois juge et partie.

Si vous êtes de cet avis, je vais m’inquiéter dès à présent de proposer le nom d’une spécialiste français qui accepte de faire partie de la Commission à titre de membre actif.

Sentiments les meilleurs. RDT”

25 June 38, DuTeil to Reich (corresp box 10, DuTeil flds) “I was unable to respond sooner – other than by telegram – to the message [envoi] that you and your friends were good enough to send me, because these days [lit: “all of these days”] have been spent putting in order all of the things [affaires] that could be taken care of and trying to make headway [lit: “see clearly”] with

the others; in short, preparing my [campagne de rentrée], in order to have access to resources after the holidays [vacances], assuming that I will be able to send the latter [les ressources] or have them sent to my family.

Happily, as I told you, this business has not gone beyond university circles, where it has been interpreted in a contradictory fashion, since, while some were abandoning me, others on the contrary were taking advantage of my newfound freedom in order to propose arrangements [associations] for private-school teaching [enseignement libre] based on the strength of my reputation. So much so that I am now assured of having work, reasonably speaking [or "quite a lot" - passablement] beginning August 15th, and certainly much more after October 15th, in private secondary schools. If I can, moreover, succeed in getting off the ground [or "up and running"] a project for an institute for the teaching of philosophy at the superior level [not sure – at the university level?], I will find myself with more or less equivalent resources [again – not clear – does he mean money?] by November. I have started the publicity (see attached a copy for the secondary level), and I think it will pay off, because my teaching reputation is well established, and this year I had great success with the license [en license? – is he referring to a degree? A teaching certificate?]. The most important thing is that I be able to hold on until October and be able to afford the costs of the publicity necessary for my [relancement], and that will be possible with what is left from what you have already sent me. In these circumstances, I think I will even be in a position to reimburse your group monthly, starting next January, what you sent me, and which, literally, saved my life.

Of course it is important, more so than ever, in the coming weeks during which the students are enrolling, that my name be kept out of this, and that we avoid putting me in a difficult position with regard to new personalities. Although Raknes's request was addressed very discreetly and by an intermediary (Allendy I think), I am, I confess, upset that Von Areud and especially Debré were mixed up in this strange [lit: "extravagant"] business based on absolutely personal letters from Van den Areud to myself. I can see Debré reproaching me [tomorrow?] for my indiscretion, and rightly so [à juste titre], and all of that can only sink me in deeper. That's why I am more convinced every day of my decision not to bring third parties into this business, and you will forgive me for having had to decide – you understand why – to destroy everything in my files that comes from third parties or is related to them. In this way I will no longer risk seeing documents misinterpreted that were glimpsed indiscreetly, seeing Debré, Bonnet, Decourt, Pretée and all of them [tutti quanti] confused [mixed in together], and seeing once again, moreover [par surcroît], the Sorbonne and the Paris hospitals set up against my indiscretion, while I have had enough from the University of Aix! A lot of patience is needed in this business. I already have, even here, around me, a growing circle of [people who are] convinced. People who are more and more specialized will join them, and we will achieve [lit: "win"] through a fine rain what a thunder strike will never give us. If, tomorrow, as I hope, an intern writes his thesis based on this work, this will be a small, very small, but certain step. Then there will be another, and so on. And the resistance will be surrounded, and will fall, necessarily, of their own accord [lit: "by themselves"]. That, my dear Friend, is what I can tell you for today. I will respond to no attack in the press. So it will cease, I hope, to be concerned with me. I owe no account to anyone of those questions that are personal, and won't allow [anyone] to encroach on this sacred domain; I will not fall into any trap. Abstention, and that's all. As for the French scholars who did me the great of honor of being interested in the affair and to write to me personally [about it], I will always agree with them on their interpretation [lit: "the interpretation they give"] of their results; and I have no right to do otherwise—short of making them acknowledge [lit: "observe"], between us, that they have made, if need be, an experimental error. If they ["on" – more like "someone"] hadn't gone over my head to ask them [the French scientists] directly for their opinion, none of this would have happened, and we would answer (with) the simple truth, notably: experiments still ongoing and unfinished [inconclusive?]. You wrote to one of them to ask him for the protocol for the experiment (he himself wrote to me about it). On this ground, private discussion is perfectly legitimate, and I approve [of it]. Polemics, however, are bad, and always reflect back against the one who set them off. Thank you again, my dear Friend, and with all my heart, to you and your friends. I am very affectionately yours, RDT

30 June, Reich to DuTeil (corresp box 10, DuTeil flds) "Ich bitte Sie, mich über das Schicksal der bisher Ihnen zur Identifizierung geschickten Präparate (S-Kultur drei Zellenkulturen und 10 Ge T-Tumor Haematoxilin-Eosin- Schnitte zu unterrichten. Ich möchte gern wissen, ob Sie diese Präparate weitergegeben haben und wenn an wen, ferner, was dazu gesagt wurde. Dies ist sehr wichtig, weil ich nun die bisher eingelaufenen Identifizierungen zusammenstelle."

Reich to DuTeil, 30 June 38 (*Beyond*, p. 161-4, **not sent**, corresp box 10, DuTeil flds) "Zunächst möchte ich nur der Korrektheit wegen Ihnen mitteilen, dass ich die Ihnen zuletzt gesandten n.K.700.—mirgeborgt habe, sie aus meiner eigenen Tasche ganz allein zurückzahlen muss und die Summe als vom Institutsfond an Sie ausgezahlz, notieren liess.

I am extremely happy that you have succeeded so quickly in setting your situation straight. We can deduce from this the moral that nothing is ever as bad as it seems [immer wieder nicht so heiss gegessen wie gekocht wird]. It is a major benefit

that the whole business was not hushed up and that the hullabaloo that arose without any prompting on our part drew the attention of quite a few good people to my work and to the successful control experiments you have carried out.

But now I must correct a few comments you make in your letter. You seem to believe that I was the one who unleashed the press campaign and kept on fanning the flames. That is not true. Apart from one basic statement to the press, in which I asked for calm, I have not responded to a single line or a single objection or reproach from my enemies. They have also ridden themselves totally into the ground and exposed themselves to the world. The press campaign is still continuing because some Norwegian scientists and physicians simply could no longer tolerate the disgraceful behavior exhibited by, in particular, a psychopathological, querulous person like Kreyberg. They are now struggling to eradicate the shame that two—no more than that—private persons have inflicted on Norwegian science. I cannot prevent my friends and the others from doing this, nor do I want to. This is how Debré, Bonnet, and others were brought into the discussion, and they are not entirely blameless. Let me just make the following clear.

1. Bonnet was not in the least entitled to write a private letter to Dr. Gording stating that negative results had been obtained.

2. Bonnet had even less right—unless he was thinking of publishing and justifying his results—to write a letter to Kreyberg in which he repeated what he had said to Gording and also to receive Kreyberg in Paris and report to him on this matter. Kreyberg published Bonnet's letter here in the press.

You still seem to be of the opinion that I get into these difficulties for some political reason. I have already written to you once that this is not true. I am not involved in any political activity, I do not belong to any party, and I have always been attacked solely because of my scientific work. But one point must be cleared up because it affects our cooperation (and I thank you for being open with me). The bion experiments have a long history, and you have not been involved in all the events in that history. Without my studies in the fields of social sciences and orgasmo therapy, there would have been no bion theory and no bion experiments and therefore no successful control tests in your laboratory. I understand very well that people focus on that part of the work which is of particular concern to them. But in the interest of this tremendously important work, you must concede that I cannot now abandon the orgasm theory merely because someone like Kreyberg or some aged psychiatrist does not like it. Nor can I concentrate my activities solely on the bions, which, incidentally, those people hate most of all. Anyone who wishes to espouse the noble bion cause must of necessity also bear the burdens arising from its history—namely, in the field of scientific and experimental sex research. And the word *sexuality* as well as the scientific work in this field will long remain a stumbling block. We have to reckon with that. I have no illusions that a psychiatrist like Scharffenberg, a pathologist like Kreyberg, or a bacteriologist like Bonnet will very soon declare themselves willing to consider the bion experiments in their total context and to verify them using the methods that derive from my sexual theories.

I fully share your opinion that we can only make headway step by step; but thunderclaps like those which we have just experienced cannot be avoided, although we must do everything we can not to provoke them. When they are instigated by enemies, however, we have to strike back forcefully and courageously, making no allowances. I fully understand your wish to show consideration for your French academic colleagues, but when I am fighting for my scientific, material, and intellectual existence, I cannot tolerate such consideration.

There is another point that requires clarification. So far I have sent you a series of preparations, mainly for identification purposes. These are preparations that nobody else has been given in this form and in this manner to work on in his laboratory. I have every right to know precisely what has happened to these preparations. May I remind you of an S-culture, three types of cell cultures, etc. Please let me know what became of them. If Debré ever had any cultures in his hands that I produced, literally by the sweat of my brow and under the enormous pressure of my financial and social situation, then I don't care how well respected he is as an academic, I have the right to demand from Debré that he tell me what has happened to these cultures. And I am sure you understand not only that this claim is justified, but also that it must be put into practice.

When you write that the French scientists have done you a great honor in showing interest in the matter, I for my part would like to claim the honor of having sent the French scientists, for the first time in the history of biology, experimentally produced spherical cells for their examination and identification. Under no circumstances can I place my work lower than that of some academic teacher, precisely because I do not enjoy financial assistance from the government and I do not have major laboratory facilities. Instead, I personally have to find the funds for each bit of difficult research. In addition, all this has to be accomplished in a world situation I did not create and am not responsible for. I have become its victim although I have done nothing bad. I must therefore ask you, my dear friend, to keep me informed of every step affecting my cultures, my reports, and my work, because in the final analysis it is I who am responsible for what happens in my laboratory.

Our cooperation has yielded such excellent fruit that I have been so bold as to reply to your openness with equal candor. I am certain that you will not hold that against me but on the contrary will regard it as an expression of the absolute trust I have in you."

2 July 38, Henri Bonnet to Reich (corresp box 9, "Bonnet" flds, Reich Engl transl) "I did not keep the experimental protocols of the 'bions.' The experiment was made with the samples which Mr. DuTeil brought us and they have been conducted according to the technique which you have indicated to him and of which I do not know anything. My part was restricted to the sterilisation procedures and to the observation of the tubes thereafter. After three months everything was still sterile in all series.

Mr. DuTeil can give you the details. I have asked him to give them to me right after the receipt of your letter but have not heard from him.

PS I can also tell you that the cultures which were shown to me as being cultures of 'bions' contained, without doubt, accidental contaminations."

8 July, DuTeil to Reich (corresp box 10, DuTeil flds) Tells Reich he will be on a radio broadcast, on Radio Côte d'Azur, on Weds 10 Aug & 17 Aug at 20h 15 (8:15 pm); 15 mins each time, talking about the bion work. Says he is extremely fatigued.

1 Aug., Reich to DuTeil (corresp box 10, DuTeil flds) [I intend to publish our magazine from anew this fall under the title "Journal for Life Research". The colleagues at the institution will take over the editorial work corresponding to their field of expertise. I would now like to ask you to inform me whether you would be willing to draw for the magazine as a member of the bion research staff. The meeting in this regard will already take place in September, so I ask that you give me notice by the end of August in one way or another.

The report of your two radio broadcasts has been announced to all of our supporters throughout the world via a newsletter at the beginning of August. You will not be heard only in France.]

3-16 Aug. 38, Reich writes about creating iron bions (bion lab ntbk 3, ms p. 139-144, photos 24-29); note that he begins to work on iron bions at the same time he first writes about Krieselwelle. More on iron bions ms pp. 145-146***, 15 Aug.

Aug. 1938 (OI box 8, "Newsletter- Aug. 1938" fld, photos 53-55) 3 pp mimeographed newsletter of the doings of the Int Inst for Sex-Economic Lebensforschung. : Institute for Sexual & Life-Research, No. 13

August, 1938

Radio Broadcasts

We would like to announce to our supporters that Professor Roger du Teil will be giving two lectures on French radio. The first one took place on August 10th, and the other will be on August 17th at 8:15 pm and last 20 minutes. Station radio Cote d'Azur, P.T.T., setting: French state of Nice, wavelength 253 m, 16, 100 shortwave. [kw could also be kilowatt].

Likewise von Rasmussen from Copenhagen will give a lecture over Danish radio also addressing research done at the institution in the near future. The exact time of this lecture will be announced.

Explanation

The following explanation from previous local staff members of the Danish magazine, "Sex and Society", was sent to the present director of the Danish regional branch of "Sexual Health":

We have received notice that the hitherto existing editorial committee for the magazine "Sex and Society" has been replaced by another. We Norwegian staff members listed below take the liberty to request that our names be removed from your list of staff members. We were familiar with the viewpoints of the previous editors and could offer support; however, we have no contact with the present editorial staff.

We request that you publish this explanation in the next edition of "Sex and Society".

Signed: Dr. Raknes

Dr. Nic Hoel

Sigurd Hoel

August Lange

Dr. W. Reich

Arnulf Overland

Afterwards, the Director, Instructor Worm, gave the following response:

I verify the receipt of your letter and the termination of staff membership with "Sex and Society" for:

Maja Dahl, Gunner Leistikow, Leunbach, Morgens Klitgaard, Tage Philipson, Ellen Siersted, Nic Hoel, Sigurd Hoel, Ola Raknes, Wilhelm Reich, Arnulf Overland, August Lange.

Since I do not know the individual addresses and because you sent me the notices of termination for both the Norwegian and the Danish staff member groups, I ask that you accept this notification verifying the terminations, signed and with two copies (one for the Norwegian group).

At the same time, I am sending a copy of your official notification as well as this letter to the current editorial staff of the magazine, who should receive it by June 21.

As the Regional Director, I now have a multitude of responsibilities to do justice to all sides, which I shall fulfill to the best of my ability.

Concerning the past:

Here I would like to express our sincere gratitude, also on behalf of the Regional Organization, for your indisputable efficient and effective contributions to the magazine, organization, and, thus, for the issue at hand during the past year. Surely no one can voice plausible criticism concerning the form and contents of the magazine, and the course is--in spite of the risks--kept strictly according to the wishes of the coalition, which could be seen in a consistent huge increase in interest for the magazine from one issue to the next, and when sales have not reached dizzying heights, this is due solely to its irregular publication.

I do not believe that I am insulting the current editorial staffs when I say that there would probably not be a publication acceptable to the coalition were it not for the old staff members.

Concerning the Transfer of the Magazine to the Regional Organization

I do not know the reasons leading to the sale of the magazine, but it was always the wish of the Board of Directions of the Regional Organization to purchase it.

The contract embodies the inexplicableness of incomprehensibility regarding the advantages for the Regional Organization, as far as one must regard it as advantageous to have a completely free hand in all directions (!). I am naturally assuming, and must accept, that the seller was sane at the time of the sale, and I must, then, recognize the contract. As far as concluding that one cannot stand one or the other person, or how much it was expected that one editorial staff member or the other would be selected for the editorial board, and that one should have stated reservations before the contract was signed now has no bargaining significance after the contract was presented, which is now the case.]

3 Aug., Reich to Henri Bonnet (corresp box 9, "Bonnet" flds***) [Thank you for your assessment from July 2, 1938. Unfortunately, today was my first chance to verify it. Your information is very important for those of us here. Based on the letter that you wrote Dr. Kreyberg in Oslo, which was published in the newspaper, the impression was given that you are fully trained in the technique of bion creation and have completed bion experiments which indicated that my already published results were false. In reality, Herr DuTeil only set up one experiment with you, which failed.

Concerning your comments that the bions you saw were merely coincidental contamination, I wish to emphasize that the preparations that you saw consisted of materials heated to incandescence or autoclaved at 120° for an half hour. In the first two years of the bion experiments, only familiar structures appeared after extreme sterilization, which one used to contribute to airborne germs. However, in the meantime cultures resulted (cell formations) that were not possible to identify.] [along with 2pp "Translation from the German manuscript (page 4, Nr. 5)", possibly dated 5 Jan 1952, first p.***) "Several readers of the reports about the bion experiments have been puzzled by the report of the Norwegian Dr. Kreyberg in Norwegian newspapers in 1938. According to this report, Dr. Bonnet of the French Academy of Science had controlled the bion experiments himself and could not confirm the results.

We do not know how Dr. Kreyberg arrived at the statements made in the newspapers. On the other hand, the archives of the Orgone Institute are in possession of a handwritten letter from Dr. Bonnet to Dr. Reich, in which Dr. Bonnet states implicitly that he did not undertake any bion experiments since he was not familiar with the technique used. He added that he had found 'air infection' in the bions which Dr. DuTeil had produced. These findings of Bonnet can easily be explained by the fact that in sterile bion preparations, besides PA and T bions, through degenerative processes rod bacteria which are usually present in air infection can be found. Microscopic observation proves that rod bacteria can be obtained through division as well as through disintegration of organic matter. This remark by Bonnet, therefore, is no refutation of the experiment, but is simply a statement on the presence of bacteria which can also be found in the air. The air germ theory, on principle, cannot be used as an objection against the organisation of organisms from bions which cannot be found 'in the air.' The process of the natural organisation of primitive organisms takes place in nature in the same way as in the laboratory experiment.

In general, all objections on principle to the Bion Theory should be met with the following statements:

a) The existence of the visible and measurable atmospheric orgone energy proves in itself the correctness of those bion experiments which led to its discovery.

[p.2] b) The bion experiments produced unknown forms which could not be cultivated from the air."

9 Sept., Reich to DuTeil (corresp box 10, DuTeil flds) "Ich habe schon so lange nichts von Ihnen gehört, dass ich gern erster das Wort wieder ergreife.

Zunächst möchte ich Ihnen mitteilen, dass die Kultivierungsarbeit mit verschiedenen weiteren Stoffen fabelhaft geht. Heute es steht für mich nicht mehr die Frage, ob Kultivierung möglich ist, sondern wie es möglich war, dass man diese Kultivierungsmöglichkeiten übersah. Desgleichen gilt für mich heute nicht mehr die Frage, ob eine Luftinfektion eine Rolle spielen kann, sondern die Frage, wie es möglich war, der Frage der Luftinfektion einen derart breiten Raum im Denken des bakteriologischen Bereiches einzuräumen. Sie können einen Versuch durchführen, der Ihnen eindeutig zeigt, dass die Gebilde, wie wir erhalten, indem wir Stoffe in bestimmter Weise sterilisieren, kultivieren, in der Luft überhaupt nicht vorhanden sind und nicht vorhanden sein können. Ich führte folgende Versuchsreihe aus:

Ein Bouillon-Glas im Laboratorium eine halbe Stunde offen,

Ein Gefäß mit Leitungswasser an einer staubigen Strasse, eine halbe Stunde offen, dann in Bouillon geimpft.

Agar 5 Minuten offen

Leitungswasser direkt auf Bouillon,

Staub von einem Schrank in Bouillon,

Schmutz von der Hand in Bouillon, etc.

Die Aufwüchse in der Bouillon etwa von Leitungswasser gab oft gar kein Resultat, oder wenn so erst nach 48 bzw. 72 Stunden. Desgleichen Wasser, das an der staubigen Strasse stand. Die Luftinfektionsgefahr ist also zwar vorhanden, aber nicht mehr so gefährlich.

Zweitens gibt keines der so erhaltenen Bakterienpräparate auch nur in einem Falle Aufwüchse von den 10 Typen, die ich hier fortgesetzt weiter kultiviere. Nimmt man den Beweis hinzu, dass man die Entstehung der Bione unmittelbar im Mikroskop verfolgen kann, so steht die Sache einfach eindeutig und klar aller Autorität und sich gebenden Hütern der Unwahrheit gegenüber. In den unsterilen Kulturen finden sich nur schlängelnde Wurststäbe, Subtili und bewegte, kleine nicht kontraktile Kokken. Ich hörte gern von Ihnen, wie Ihre Arbeit weitergeht.

Nun noch eine grosse Bitte: Die materielle Lage des Instituts hier ist jztz recht schwierig. Ich bitte Sie daher sehr darum, die Ihnen vom Institutsfond geborgte Summe von etwa Kr. 1000.—entweder auf einmal, wenn es geht, wenn nicht in monatlichen Ratenzahlungen abzutragen. Ch überlasse es gern Ihnen zu bestimmen, in welcher Weise Sie die Summe zurückzahlen. Richten Sie sich dabei so ein, wie Sie es können.

Hatten Sie Erfolg mit den Radiovorträgen? Könnte ich die Radiovorträge in Kopie zugeschickt bekommen?"

Reich to DuTeil, 16 Sept. (*Beyond* p. 167-8) "My dear friend, Quite honestly I do not understand your silence. There are a number of still unanswered questions to which I requested your reply. Now I have another question or request for you that is of decisive importance for our work. Since my existence as an émigré citizen of the German Reich would be extremely endangered in the event of a war, I must take steps to ensure that the approximately ten types of bion culture that exist and that are the sole tangible proof of the accuracy of the bion theory survive the war. I am therefore writing to Stockholm, to England, and to the French Academy in Paris, as well as to you, to find out whether and how it might be possible to hand over a series of each of the cultures available here to a biological or bacteriological institute for safekeeping. Each of the bion cultures requires a special type of treatment that I would describe in detail. Please let me know by return mail whether you could perform this act of kindness for me and find out who in Marseilles, Nice, or Paris would be willing to accept a series of bion cultures."

(original in corresp box 10, DuTeil flds) "Ganz offen gestanden verstehe ich Ihr Schweigen nicht. Es stehen eine Menge Fragen unbeantwortet, um deren Beantwortung ich Sie gebeten habe. Ich komme nun wieder mit einer Frage bzw. Bitte, die für unsere Arbeit von entscheidender Bedeutung ist. Da meine Existenz als emigrierter deutscher Reichsbürger im Falle eines Krieges ausserordentlich gefährdet ist, muss ich dafür sorgen, dass die vorhandenen etwa 10 Typen von Bionkulturen, die den einzigen greifbaren Beweis für die Richtigkeit der Biontheorie bilden, den Krieg überstehen. Ich schreibe daher nach Stockholm, England, an die Französische Akademie in Paris und an Sie, um zu erfahren, ob und wie es möglich wäre, je eine Serie der hier vorhandenen Kulturen einem Biologischen oder Bakteriologischen Institut zur Pflege zu übergeben. Jede der Bionkulturen bedarf einer besonderen Art der Behandlung, die ich detailliert angeben würde. Ich bitte Sie um umgehende Antwort, ob Sie mir diesen Freundschaftsdienst erweisen können, in Erfahrung zu bringen, wer in Marseille, Nizza oder in Paris eine Serie von Bionkulturen übernehmen würde."

28 Sept. 38, DuTeil to Reich (corresp box 10, DuTeil flds) "I did not write you sooner because I had to prolong my vacation for health reasons. And since my return, events have become so worrying and untenable that I have been awaiting the end of the nightmare from day to day to write you something that makes sense. Alas, we are undoubtedly in the last hours before the most terrible upheaval that our planet may have known. I am preparing to evacuate Nice, which is probably going to be the city on the front if Italy, as they claim, marches against France. I am leaving tonight in order to put my wife and children in a secure place. I intend to return then to Nice, where I will take a military office if they want me.

I have the impression that Norway is still the best location for the continuation of your work, as long as something isn't stabilized one way or the other.

On the other hand, if that [stability] was somewhat arranged, I would now be able to find a laboratory that would agree to take our 'pensionnaires' (residents, i.e., bion cultures). I would be able to maintain certain cultures, if not all. As for money matters, we are of course still waiting, and today I had the greatest difficulty collecting the few dollars that will help me save my family and enable it to survive a few weeks.

Let's hope, nevertheless. It's the only recourse remaining to us. And allow me today to embrace you affectionately with all my sadness, but with all my heart also.

PS I have put the microscope under cover—there is no risk."

30 Sept., Munich Pact, in which Britain and France caved in to Hitler's demands for cession of the Sudetenland from Czechoslovakia.

4 Oct., Reich to DuTeil (corresp box 10, DuTeil flds) "Gestern kam Ihr Brief vom 28. September. Ich habe eine lebhaftere Vorstellung von der Aufregung, in der Sie da unten lebten. Uch wir hier haben einige recht schwere Tage mitgemacht, obwohl wir nicht derart unmittelbar gefährdet waren. Nun ist ja die Sache aktuell vorüber, doch ich persönlich rechne mit dem Frieden und bereite auch schon alles vor, um zu sichern, was zu sichern ist. Die französische Academi in Paris erklärte sich bereit, die Ergebnisse up to date [sic] zu publizieren. Ich weiss nur noch nicht, wiviel ist jetzt schon mitteilen soll.

Schreiben Sie bitte, wo Sie bleiben und ob die Arbeiten in diese unruhigen Zeiten durch Sie weitergeführt werden konnten."

Fri. 3 March 39, Reich to DuTeil (corresp box 10, DuTeil flds) „I'm really surprised about your consistent persistent silence, which I can suggest to me in any way. I ask you to understand that this state must contribute in proportion to the last year worrying to me.

S hay my letter relates to some questions from the Reichert in Vienna. Then I heard nothing from you and could therefore give Reichert no proper information. I gave him recently, the decision that I return the claim to the demonstration microscope to him and he agreed because transfer or handoff of the microscope with you is now needed.

You will be interested to know that I am for about two weeks in the possession of radioactive bions.

Your experimental correction of Bonnet's claims will soon be published in appropriate context here."

Sunday 11 June, DuTeil to Reich (corresp box 10, DuTeil flds) .: It's a long time since I've heard from you, and my letter from last October was never answered. Following the events you know about, I myself have undergone terrible difficulties, and I have had to swim back upstream day by day after numerous struggles, and using only my arms, against the current that threatened to carry me away. I have taken a position which is beginning to almost compensate for my former one, and which will compensate for it once I have paid back the expenses that I had to incur in order to entirely reorganize a private school and to publicize it. I hope if nothing happens to have balanced and stabilized for the beginning of fall term in November. In September I think I shall be in a position to send you something of the aid you sent me at the time of the catastrophe [i.e., June 1938].

After a waiting period in which I devoted myself entirely to recovering my losses, I nevertheless continued the work that interests us. I cannot give you the details today. But a series of experiments have led me to sense a close relationship between the success of the experiments and the state of atmospheric ionization, which would explain certain failures. I can even tell you that I undertook a series of tests by one of my correspondents from North Africa where sunshine allows for clearer observations; and I am awaiting the results. In addition, I have proof from various communications from people whom I did not know before, that the idea slowly but surely makes its way. And I have high hopes that a day will come, if one does not rush, when we will have our revenge.

This letter, my dear friend, aims only to retie the knot which unfortunate circumstances have momentarily loosened. I hope to receive an imminent letter from you giving me information on the state of your own work and on the general situation.

Send my respects to Mrs. Reich, remember me to our friends. Yours, RDT]

Thurs 15 June 39, Reich to DuTeil (*Beyond* p. 222-23, box 10, DuTeil flds) “You can hardly imagine what pleasure your letter gave me. I am ashamed to have to confess that your failure to communicate for several months made me believe that you, like many others in the past, had abandoned my sinking ship. I had written two or three letters to you in Nice without receiving a reply. [Therein was the report that since January a new type of bion culture from a heavy inorganic material was produced, which emits a highly interesting, objectively demonstrable radiation. The phenomenon is currently being investigated at a foreign physical radiation institute....You see, I was not idle.] I am happy to read that you have managed to regain your old situation. **There is very little reason to doubt that ultimately we will be victorious. The only question is whether I will live to see that day.** The campaigns of harassment directed against me are still going on, most recently with the psychiatric diagnosis of ‘insanity.’ This does not matter. Every time I look into the microscope, the pain caused by such [Jeder Blick ins Mikroskop befreit mich vom Druck, den diese Gemeinheit] is lifted from me [ausübt].

I am moving to New York. Some of my students there have set everything in motion and are struggling to overcome enormous difficulties. My laboratory and one assistant have already arrived on the other side. **Ich räume meine Osloer Wohnung im Drammensveien am 30.Juni. Meine Adresse wird bis auf weiteres Oslo Hauptpostlagernd (posterestante) sein.**

[The 1000 Norwegian Kroner I lent you, I ask you to repay c/o Dr. Raknes, Oslo. It will help repay a debt from me to the Institute. Explain this accounting to Dr. Raknes, please.]

For the time being, my immigration to America means nothing less than my complete financial ruin. Despite all the countless major and minor new observations I have made, I have not published anything, because I must first compose myself before starting the process of rebuilding. I am wondering whether it wouldn't perhaps be better not to publish anything anymore and just send my material to selected colleagues. This is the only way to escape the appalling smear campaigns waged by so-called scientists. The French Academy did not publish a report on *Bion Experiments on the Cancer Problem* but instead merely consigned it to their archives. I received a letter from Bonnet in which he confirms that he did not verify your experiments when they were en route from here to Paris and that he ‘does not understand anything about it.’ Our Dr. Kreyberg here in Oslo is therefore nothing more or less than a common liar and scoundrel [Lügner und Schuft]. I do not envy him the position in which he will find himself a few years from now. We must stay calm. I, too, have heard from many quarters that the bion theory is taking hold and has found a great deal of support.”

Mon. 19 June, DuTeil to Reich (responded to on 6/22) Original, corresp box 10, DuTeil flds) : I was also very happy to have your news, even though it was sad. We both suffer for the same cause, and we need few words to understand one another. My heart has never stopped beating with yours, and it never will.

I never received the letters you alluded to. But it is not astonishing, for I live right near my old office; they often leave my mail there still. And there [at the Univ.] when they seem interesting, they make them disappear. I have had much lost mail.

The news of your departure for so far away saddens me, for it removes you from me and renders the possibility of seeing you again, which I had not ruled out, very problematic. Who knows when? On the other hand, I'm relieved to know that you are far away from your enemies and in a country where people are really more broad-minded. Remember, this is where [Alexis] Carrel had to flee when he was misunderstood in France; and he has since found his way there.]

Ce que je vous écrivais l'autre jour confirme... (correspondence box 9, fld “English translations”) “What I wrote you the other day confirms what you say yourself, namely, that the idea gradually makes headway. The other day, in Pontigny, our results were cited by one of the scientists as acknowledged facts. I expect much from the work which is going to be done in North Africa (Morocco). I shall keep you posted on the results as officially acknowledged, in order to avoid any such controversy as that which occurred last year. Herrera is very well known and his work on the ‘protobies’ is a classic. This is an important corroboration.

As I told you, I undertook a series of tests, especially on Preparation 12, in connection with the observations on atmospheric ionization. There is a definite relationship between its variations and the periods of success or failure. It will be a

very long process, for it is necessary to find the thresholds of the action, and it will necessitate hundreds of experiments lasting over the four seasons. There are accidental variations as well, and also seasonal variations. I had to learn meteorology! And I think there lies the key to the apparent irregularity of the results.

This said, you think that you have to let the imbeciles and the bad fellows speak. As you phrase it, the microscope is a consolation for many things.

Concerning the [large Reichert Z-] microscope, would you be kind enough before leaving to write me a short letter confirming that this is Reich's microscope, belonging to you, and that I am accountable to nobody but you for it. This is in case, after your departure, they might try from Vienna to really turn on the pressure.

Agreed on what I owe you, I'm going to write very soon to Oslo to confirm that it is to Mr. Raknes that I'll pay it.

As soon as you arrive in New York, give me your address and tell me how everything is over there. Keep me informed about the work. I shall do likewise. What a shame that I am too old; I would have willingly joined you over there to teach philosophy there!

My best wishes to you for your trip... RDT]

Thurs. 22 June, Reich to DuTeil (*Beyond* p. 223, box 10, DuTeil flds) "Your letter of 19 June arrived today. You will be interested to know that your line of experimental inquiry, which led up to the question of solar radiation, is not a fluke. I am sending you a report I distributed some time ago to various institutes. It does not yet contain the experiments I have performed here in the meantime on the basis of the SAPA bions. These experiments also led directly to the question of solar radiation. I have obtained some reliable results, which I will summarize in the near future and pass on to you.

In New York, a seminar of psychiatrists and analysts has been formed and I am to be its educational director.

My address in New York for the time being will be c/o Dr. Wolfe, 15 East 86th St., New York City. If my appointment is recognized by the officials here in Oslo, then I will definitely be in New York in mid-July.

I would like to bid you a warm farewell. But New York is not on another planet, and we will without doubt see each other again soon."

[Higgins note: There is no indication in Reich's archives that DuTeil responded to this letter. Reich's subsequent efforts to locate DuTeil during the war were unsuccessful and, in 1945, he was told that DuTeil had been imprisoned in Marseilles in 1940. According to a biographer of Reich, DuTeil survived the war, but apparently he did not contact Reich.]