A REPLY TO CRITICISMS OF MY PAPER ON ANXIETY NEUROSIS - (1895)

In the second number of Mendel's Neurologisches Zentralblatt for 1895, I published a short paper in which I ventured an attempt to detach a number of nervous states from neurasthenia and to establish them as an independent entity under the name of 'anxiety neurosis'.[1] I was led to do so by the presence of a constant conjunction of certain clinical features with certain aetiological ones - a thing which, in general, should permit us to make a separation of this kind. I found - and in this Hecker (1893) had anticipated me - that the neurotic symptoms in question could all be classed together as constituting expressions of anxiety; and, from my study of the aetiology of the neuroses, I was able to add that these portions of the complex of the 'anxiety neurosis' exhibit special aetiological preconditions which are almost the opposite of the aetiology of neurasthenia. My observations had shown me that in the aetiology of the neuroses (at all events of acquire–d cases and acquirable forms) sexual factors play a predominant part and one which has been given far too little weight; so that a statement such as that 'the aetiology of the neuroses lies in sexuality', with all its unavoidable incorrectness per excessum et defectum, nevertheless comes nearer to the truth than do the other doctrines, which hold the field at the present time. A further assertion which my observations forced me to make was to the effect that the various sexual noxae are not to be found in the aetiology of every neurosis indifferently, but that unmistakable special relationships hold between particular noxae and particular neuroses. Thus I could assume that I had discovered the specific causes of the various neuroses. I then sought to formulate shortly the special character of the sexual noxae which constitute the aetiology of anxiety neurosis, and, on the basis of my view of the sexual process (p. 108), I arrived at the proposition: anxiety neurosis is created by everything which keeps somatic sexual tension away from the psychical sphere, which interferes with its being worked over psychically. If we go back to the concrete circumstances in which this factor becomes operative, we are led to assert that abstinence, whether voluntary or involuntary, sexual intercourse with incomplete satisfaction, coitus interruptus, deflection of psychical interest from sexuality, and similar things, are the specific aetiological factors of the states to which I have given the name of anxiety neurosis.

When I published the paper I have mentioned, I was under no illusion as to its power to carry conviction. In the first place, I was aware that the account I had given was only a brief and incomplete one and even in places hard to understand - just enough, perhaps, to arouse the reader's expectations. Then, too, I had scarcely brought forward any examples and given no figures. Nor had I touched on the technique of collecting anamneses or done anything to prevent misunderstandings. I had not given consideration to any but the most obvious objections; and, as regards the theory itself, I had laid stress only on its main proposition and not on its qualifications. Accordingly, each reader was in fact at

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[1] 'On the Grounds for Detaching a Particular Syndrome from Neurasthenia under the Description "Anxiety Neurosis"' (1895b).

liberty to form his own opinion as to the binding force of the whole hypothesis. I could, moreover, reckon upon another difficulty in the way of its acceptance. I know very well that in putting forward my ‘sexual aetiology’ of the neuroses, I have brought up nothing new, and that undercurrents in medical literature taking these facts into account have never been absent. I know, too, that official academic medicine has in fact also been aware of them. But it has acted as if it knew nothing about the matter. It has made no use of its knowledge and has drawn no inferences from it. Such behaviour must have a deep-seated cause, originating perhaps in a kind of reluctance to look squarely at sexual matters r in a reaction against older attempts at an explanation, which are regarded as obsolete. At all events, one had to be prepared to meet with resistance in venturing upon an attempt to make something credible to other people which they could without any trouble have discovered for themselves.

In such circumstances it would perhaps be more expedient not to answer critical objections until I had myself expressed my views on this complicated subject in greater detail and had made them more intelligible. Nevertheless, I cannot resist the motives which prompt me to make an immediate answer to a criticism of my theory of anxiety neurosis which has appeared in recent days. I do so because its author, L. Löwenfeld of Munich, the author of Pathologie und Therapie der Neurasthenia, is a man whose judgement undoubtedly carries great weight with the medical public; because of a mistaken view which Löwenfeld’s account imputes to me; and finally because I wish to combat at the very start the impression that my theory can be refuted quite so easily by the first objections that come to hand.

With an unerring eye Löwenfeld (1895) detects the essential feature of my paper - namely, my assertion that anxiety-symptoms have a specific and uniform aetiology of a sexual nature. If this cannot be established as a fact, then the main reason for detaching an independent anxiety neurosis from neurasthenia disappears as well. There remains, it is true, one difficulty to which I called attention - the fact that anxiety-symptoms also have such very unmistakable connections with hysteria, so that a decision on Löwenfeld’s lines would prejudice the separation between hysteria and neurasthenia. This difficulty, however, is met by a recourse to heredity as the common cause of all these neuroses (a view which I will go into later).

What arguments, then, does Löwenfeld use to support his objection to my theory ?

(1) I emphasized as a point essential to an understanding of anxiety neurosis that the anxiety appearing in it does not admit of a psychical derivation - that is to say that the preparedness for anxiety, which constitutes the nucleus of the neurosis, cannot be acquired by a single or repeated affect of psychically justified fright. Fright, I maintained, might result in hysteria or a traumatic neurosis, but not in an anxiety neurosis. This denial, it is easy to see, is nothing else than the counterpart to my contention, on the positive side, that the anxiety appearing in my neurosis corresponds to a somatic sexual tension which has been deflected from the psychical field - a tension which would otherwise have made itself felt as libido.

Against this, Löwenfeld insists on the fact that in a number of cases ‘states of anxiety appear immediately or shortly after a psychical shock (fright alone, or accidents which were accompanied by fright), and in such situations there are sometimes circumstances which make the simultaneous operation of sexual noxae of the kind mentioned extremely improbable.’ He gives, shortly, as a particularly pregnant example, one clinical observation (to serve instead of many). This example concerns a woman of thirty, with a hereditary taint, who had been married for four years and who had had a first, difficult, confinement a year before. A few weeks after this event her husband had an attack of illness which frightened her, and in her agitation she ran about the cold room in her chemise. From that time on she was ill. First she had states of anxiety and palpitations in the evening, then came attacks of convulsive trembling, and after that phobias, and so on. It was the picture of a fully-developed anxiety neurosis. ‘Here,’ concludes Löwenfeld, ‘the anxiety states are obviously of psychical origin, brought about by the single fright.’

I do not doubt that my respected critic can produce many similar cases. I myself can supply a long list of analogous examples. Anyone who has not seen such cases - and they are extremely common - of an outbreak of anxiety neurosis after a psychical shock, ought not to regard himself as qualified to take part in discussions about anxiety neurosis. I will only remark in this connection that neither fright nor anxious expectation need always be found in the aetiology of such cases; any other emotion will do as well. If I hastily recall a few cases from my memory, I think of a man of forty-five who had his first attack of anxiety (with cardiac collapse) at the news of the death of his father, who was an old man; from that time on he developed a complete and typical anxiety neurosis with agoraphobia. Again, I think of a young man who was overtaken by the same neurosis on account of his agitation about the disagreements between his young wife and his mother and who had a fresh onset of agoraphobia after every domestic quarrel. Then, there was a student, something of an idler, who produced his first anxiety attacks during a period in which, under the spur of his father’s displeasure, he was working hard for an examination. I recall, too, a woman, herself childless, who fell ill as a result of anxiety about the health of a small niece. And other similar instances. About the facts themselves, which Löwenfeld uses against me, there is not the slightest doubt.

But there is doubt about their interpretation. Are we to accept the post hoc ergo propter hoc conclusion straight away and spare ourselves any critical consideration of the raw material? There are examples enough in which the final, releasing cause has not, in the face of critical analysis, maintained its position as the causa efficiens. One has only to think, for instance, of the relationship between trauma and gout. The role of a trauma in provoking an attack of gout in the injured limb is probably no different from the role it plays in the aetiology of tabes and general paralysis of the insane; only in the case of gout it is clear to the meanest capacity that it is absurd to suppose that the trauma has ‘caused’ the gout instead of having merely provoked it. It is bound to make us thoughtful when we come across aetiological factors of this sort - ‘stock’ factors, as I should like to call them - in the aetiology of the most varied forms of illness. Emotion, fright, is also a stock factor

of this kind. Fright can provoke chorea, apoplexy, paralysis agitans and many other things just as well as it can provoke anxiety neurosis. I must not go on to argue, of course, that, because of their ubiquity, the stock causes do not satisfy our requirements and that there must be specific causes as well; to do so would be to beg the question in favour of the proposition I want to prove. But I am justified in drawing the following conclusion: if the same specific cause can be shown to exist in the aetiology of all, or the great majority, of cases of anxiety neurosis, our view of the matter need not be shaken by the fact that the illness does not break out until one or other stock factor, such as emotion, has come into operation.

So it was with my cases of anxiety neurosis. Let us take the man who, after receiving the news of his father's death, fell ill so inexplicably.(I add 'inexplicably' because the death was not unexpected and did not occur in unusual or shattering circumstances.) This man had carried out coitus interruptus for eleven years with his wife, whom he tried for the most part to satisfy. Again, the young man who was not equal to the quarrels between his wife and his mother, had practised withdrawal with his young wife from the first, in order to spare himself the burden of children. Then we have the student who acquired an anxiety neurosis from overwork, instead of the cerebral neurasthenia that was to be expected: he had maintained a relationship for three years with a girl whom it was not permissible for him to make pregnant. Again, there was the woman who, childless herself, was overtaken by an anxiety neurosis about a niece's illness: she was married to an impotent man and had never been sexually satisfied. And so on. Not all these cases are equally clear or equally good evidence for my thesis; but when I add them to the very considerable number of cases in which the aetiology shows nothing but the specific factor, they fit without contradiction into the theory I have put forward and they allow of an extension of our aetiological understanding beyond the boundaries hitherto in force.

If anyone wants to prove to me that in these remarks I have unduly neglected the significance of the stock aetiological factors, he must confront me with observations in which my specific factor is missing - that is, with cases in which anxiety neurosis has arisen after a psychical shock although the subject has (on the whole) led a normal vita sexualis. Let us see now whether Löwenfeld's case fulfils this condition. My respected opponent has evidently not been clear about this necessity in his own mind, otherwise he would not have left us so completely in the dark about his patient's vita sexualis. I will leave on one side the fact that this case of a lady of thirty is obviously complicated by a hysteria as to the psychical origin of which I have not the least doubt; and I naturally admit without raising any objection the presence of an anxiety neurosis alongside of this hysteria. But before I turn a case to account for or against the theory of the sexual aetiology of the neuroses, I must first have studied the patient's sexual behaviour more closely than Löwenfeld has done here. I should not be content to conclude that, because the time at which the lady received her psychical shock was shortly after a confinement, coitus interruptus could not have played a part during the previous year, and that therefore sexual noxae are ruled out. I know cases of women who were made pregnant every year, and who yet had anxiety neurosis, because - incredible as it

may seem - all sexual relations were stopped after the first fertilizing coition, so that in spite of having many children they suffered from sexual privation through all these years. No doctor is ignorant of the fact that women conceive from men whose potency is very slight and who are not able to give them satisfaction. Finally (and this is a consideration which should be taken into account precisely by the upholders of a hereditary aetiology), there are plenty of women who are afflicted with congenital anxiety neurosis - that is to say, who inherit, or who develop without any demonstrable disturbance from outside, a vita sexualis which is the same as the one usually acquired through coitus interruptus and similar noxae. In a number of these women we are able to discover a hysterical illness in their youth, since which their vita sexualis has been disturbed and a deflection of sexual tension from the psychical sphere has been established. Women with this kind of sexuality are incapable of obtaining real satisfaction even from normal coitus, and they develop anxiety neurosis either spontaneously or after further operative factors have supervened. Which of all these elements were present in Löwenfeld's case? I do not know. But I repeat: this case is evidence against me only if the lady who responded to a single fright with an anxiety neurosis had before then enjoyed a normal vita sexualis.

It is impossible to pursue an aetiological investigation based on anamneses if we accept those anamneses as the patients present them, or are content with what they are willing to volunteer. If syphilidologists still depended on the statements of their patients for tracing back an initial infection of the genitals to sexual intercourse, they would be able to attribute an imposing number of chancres in allegedly virginal persons to catching a chill; and gynaecologists would have little difficulty in confirming the miracle of parthenogenesis among their unmarried lady clients. I hope that one day the idea will prevail that neuropathologists, too, in collecting the anamneses of major neuroses, may proceed upon aetiological prejudices of a similar kind.

(2) Löwenfeld says further that he has repeatedly seen anxiety states appear and disappear where a change in the subject's sexual life had certainly not taken place but where other factors were in play.

I, too, have made exactly the same observation, without, however, being misled by it. I myself have caused anxiety attacks to disappear by means of psychical treatment, improvement of the patient's general health, and so on; but I have naturally not concluded from this that what had caused the anxiety attack was a lack of treatment. Not that I should like to foist a conclusion of this sort upon Löwenfeld. My joking remark is only intended to show that the state of affairs may easily be complicated enough to render Löwenfeld's objection quite invalid. I have not found it difficult to reconcile the fact brought forward here with my assertion that anxiety neurosis has a specific aetiology. It will readily be granted that there are aetiological factors which, in order to exercise their effect, must operate with a certain intensity (or quantity) and over a certain period of time - which, that is to say, become summated. The effects of alcohol are a standard example of causation like this through summation. It follows that there must be a period of time in which the specific aetiology is at work but in which its effect is not yet manifest.

During this time the subject is not ill as yet, but he is predisposed to a particular illness - in our case, to anxiety neurosis - and now the addition of a stock noxa will be able to set the neurosis off, just as would a further intensification of the operation of the specific noxa. The situation may also be expressed as follows: it is not enough for the specific noxa to be present; it must also reach a definite amount; and, in the process of reaching that limit, a quantity of specific noxa can be replaced by a quota of stock noxa. If the latter is removed once more, we find ourselves below a certain threshold and the clinical symptoms depart once more. The whole therapy of the neuroses rests upon the fact that the total load upon the nervous system, to which it has succumbed, can be brought below this threshold by influencing the aetiological mixture in a great variety of ways. From these circumstances we can draw no conclusion as to the existence or non-existence of a specific aetiology. These considerations are surely indisputable and assured. But anyone who does not think them sufficient may be influenced by the following argument. According to the views of Löwenfeld and very many others, the aetiology of anxiety states is to be found in heredity. Now heredity is certainly immune to alteration; thus if anxiety neurosis is curable by treatment, we should have to conclude according to Löwenfeld's argument that its aetiology cannot reside in heredity.

For the rest, I might have been spared having to defend myself against these two objections of Löwenfeld's, if my respected opponent had paid greater attention to my paper itself. In it, both these objections are anticipated and answered. I have only been able to repeat here what I said there; and I have even purposely analysed the same cases over again. Moreover the aetiological formulas on which I have just laid weight are contained in the text of my paper. I will repeat them once more. I maintain that there exists a specific aetiological factor for anxiety neurosis which can be replaced in its operation by stock noxae in a QUANTITATIVE sense, but not in a QUALITATIVE one; I furthermore maintain that this specific factor determines above all the FORM of the neurosis; whether a neurotic illness occurs at all depends on the total load upon the nervous system (in proportion to its capacity to carry the load). As a rule the neuroses are overdetermined; that is to say, several factors operate together in their aetiology.

(3) I need not concern myself so much about refuting Löwenfeld's next comments, since on the one hand they damage my theory very little and on the other they raise difficulties whose existence I acknowledge. Löwenfeld writes: 'The Freudian theory is totally insufficient to explain the appearance or non-appearance of anxiety attacks in individual instances. If anxiety-states - i. e. the clinical symptoms of anxiety neurosis - occurred solely through a subcortical storing-up of somatic sexual excitation and an abnormal employment of it, then every person who is afflicted with anxiety-states ought, so long as no changes take place in his sexual life, to have an anxiety attack from time to time,just as an epileptic has his attack of grand and petit mal. But this, as everyday experience shows, is by no means so. The anxiety attacks happen in the great majority of instances only on definite occasions; if the patient avoids these occasions or is able to paralyse their influence by taking some precaution, he remains exempt from anxiety attacks,

whether he is consistently given over to coitus interruptus or to abstinence, or whether he enjoys a normal sexual life.'

There is a great deal to be said about this. In the first place, Löwenfeld forces upon my theory an inference which it is not bound to accept. To suppose that in the storing-up of somatic sexual excitation the same thing must be happening as in the accumulation of the stimulus which leads to an epileptic convulsion, is to make a far too detailed hypothesis, and I have given no occasion for it; nor is it the only one that presents itself. I need only assume that the nervous system has the power to master a certain amount of somatic sexual excitation even where the latter is deflected from its aim, and that disturbances only occur when that quantum of excitation receives a sudden increment, and Löwenfeld's claim would be disposed of. I have not ventured to extend my theory in that direction, chiefly because I did not expect to find any solid points of support along that path. I should merely like to indicate that we ought not to think of the production of sexual tension independently of its distribution; that in normal sexual life this production, when it is stimulated by a sexual object, takes on a substantially different form from what it does in a state of psychical quiescence; and so on.

It must be admitted that the condition of affairs here is in all probability different from what prevails in the tendency to epileptic convulsions, and that it cannot yet be consistently derived from the theory of the accumulation of somatic sexual excitation.

Against Löwenfeld's further assertion - that anxiety-states only appear under certain conditions and fail to appear when those conditions are avoided, regardless of what the subject's vita sexualis may be - it must be pointed out that he clearly has in mind here only the anxiety of phobias, as, indeed, is shown by the examples attached to the passage I have quoted. He says nothing at all about the spontaneous anxiety attacks which take the form of vertigo, palpitation, dyspnoea, trembling, sweating, and so on. My theory, on the contrary, seems by no means unequal to explaining the emergence or non-emergence of these attacks of anxiety. For in a whole number of such cases of anxiety neurosis there does in fact appear to be a periodicity in the emergence of the states of anxiety, similar to what has been observed in epilepsy, except that in the latter the mechanism of the periodicity is more transparent. On closer examination we discover the presence, with great regularity, of an excitatory sexual process (that is, a process which is able to generate somatic sexual tension), and which, after the lapse of a definite and often constant interval of time, is followed by the anxiety attack. This role is played, in abstinent women, by menstrual excitation; it is played, too, by nocturnal pollutions, which also recur periodically. Above all, it is played by sexual intercourse itself (harmful from its being incomplete), which carries over its own periodicity to the effects it brings about, viz. to the anxiety attacks. If anxiety attacks occur which break through the usual periodicity, it is generally possible to trace them back to an incidental cause of rare and irregular occurrence - to a single sexual experience, something read or seen, and the like. The interval I have mentioned varies from a few hours to two days; it is the same as that which elapses in other people between the occurrence of the same causes and the onset

of the well-known sexual migraine, which has well-established connections with the syndrome of anxiety neurosis.

Besides this, there are plenty of cases in which a single anxiety-state is provoked by the extra addition of a stock factor, by an excitement of some kind or other. The same holds good, therefore, for the aetiology of the individual anxiety attack as for the causation of the whole neurosis. It is not very strange that the anxiety of the phobias should obey different conditions; they have a more complicated structure than purely somatic anxiety attacks. In phobias the anxiety is linked to a definite ideational or perceptual content, and the arousal of this psychical content is the chief condition for the emergence of the anxiety. When this happens, anxiety is ‘generated’, just as for instance sexual tension is generated by the arousal of libidinal ideas. The connection of this process, however, with the theory of anxiety neurosis has not yet been elucidated.

I see no reason why I should try to hide the gaps and weaknesses in my theory. The main thing about the problem of the phobias seems to me to be that when the vita sexualis is normal - then the specific condition, a disturbance of sexual life in the sense of a deflection of the somatic from the psychical, is not fulfilled - phobias do not appear at all. However much else may be obscure about the mechanism of phobias, my theory can only be refuted when I have been shown phobias where sexual life is normal or even where there is a disturbance of it of a non-specific sort.

(4) I now pass on to a remark by my esteemed critic which I cannot leave uncontradicted. In my paper on anxiety neurosis I had written:

‘In some cases of anxiety neurosis no aetiology at all is to be discovered. It is worth noting that in such cases there is seldom any difficulty in establishing evidence of a grave hereditary taint.

‘But where there are grounds for regarding the neurosis as an acquired one, careful enquiry directed to that end reveals that a set of noxae and influences from sexual life . . .’ Löwenfeld quotes this passage and adds the following gloss: ‘From this it appears that Freud always regards a neurosis as "acquired" whenever incidental causes are to be found for it.’

If this meaning follows naturally from my text, then the latter gives a very distorted expression to my thoughts. Let me point out that in the preceding pages I have shown myself far stricter than Löwenfeld in my evaluation of incidental causes. If I were myself to elucidate the meaning of the passage I wrote I should add, after the subordinate clause ‘But where there are grounds for regarding the neurosis as an acquired one . . .’, the words ‘because evidence (referred to in the previous sentence) of a hereditary taint is not forthcoming . . .’ What this means is that I hold the case to be an acquired one, since no heredity is to be discovered in it. In doing so I am behaving like everyone else, perhaps with the slight difference that others may declare the case to be determined by heredity even when there is no heredity, so that they overlook the whole category of acquired neuroses. But this difference runs in my favour. I admit, however, that I am myself to blame for this

misunderstanding, on account of the way in which I expressed myself in the first sentence: ‘no aetiology at all is to be discovered’. I shall certainly be taken to task from other directions as well and be told that I have created useless trouble for myself by searching for the specific causes of neuroses. Some will say that the true aetiology of anxiety neurosis, as of neuroses in general, is known: it is heredity. And two real causes cannot exist side by side. I have not, they will say, denied the aetiological role of heredity; but if so, all other aetiologies are merely incidental causes and equal to one an other in value or want of value.

I do not share this view of the role of heredity; and since in my short paper on anxiety neurosis it is precisely to this theme that I have paid least attention, I will now try to make good some of what I have omitted in it and to remove the impression that in writing my paper I had not attended to all the relevant problems. I think we can arrive at a picture of the probably very complicated aetiological situation which prevails in the pathology of the neuroses if we postulate the following concepts :

(a) Precondition, (b) Specific Cause, (c) Concurrent Causes, and, as a term which is not equivalent to the foregoing ones, (d) Precipitating or Releasing Cause.

In order to meet every possibility, let us assume that the aetiological factors we are concerned with are capable of a quantitative change - that is of increase or decrease.

If we accept the idea of an aetiological equation of several terms which must be satisfied if the effect is to take place, then we may characterize as the precipitating or releasing cause the one which makes its appearance last in the equation, so that it immediately precedes the emergence of the effect. It is this chronological factor alone which constitutes the essential nature of a precipitating cause. Any of the other causes, too, can in a particular case play the role of precipitating cause; and this role can change within the same aetiological combination.

The factors which may be described as preconditions are those in whose absence the effect would never come about, but which are incapable of producing the effect by themselves alone, no matter in what amount they may be present. For the specific cause is still lacking.

The specific cause is the one which is never missing in any case in which the effect takes place, and which moreover suffices, if present in the required quantity or intensity, to achieve the effect, provided only that the preconditions are also fulfilled.

As concurrent causes we may regard such factors as are not necessarily present every time, nor able, whatever their amount, to produce the effect by themselves alone, but which operate alongside of the preconditions and the specific cause in satisfying the aetiological equation.

The distinctive character of the concurrent, or auxiliary, causes seems clear; but how do we distinguish between a precondition and a specific cause, since both are indispensable and yet neither suffices alone to act as a cause?

The following considerations seem to allow us to arrive at a decision. Among the ‘necessary causes ' we find several which reappear in the aetiological equations concerned in many other effects and thus exhibit no special relationship to any one particular effect. One of these causes, however, stands out in contrast to the rest from the fact that it is found in no other aetiological equation, or in very few; and this one has a right to be called the specific cause of the effect concerned. Furthermore, preconditions and specific causes are especially distinct from each other in those cases in which the preconditions have the characteristic of being long-standing states that are little susceptible to alteration, while the specific cause is a factor which has recently come into play.

I will try to give an example of this complete aetiological schematic picture:

Effect: Phthisis pulmonum.

Precondition: Disposition, for the most part laid down through heredity, by the organic constitution.

Specific Cause: Bacillus Kochii.

Auxilliary Causes: Anything that diminishes the powers - emotions as well as suppurations or colds.

The schematic picture for the aetiology of anxiety neurosis seems to me to be on the same lines:

Precondition: Heredity.

Specific Cause: A sexual factor, in the sense of a deflection of sexual tension away from the psychical field.

Auxiliary Causes: Any stock noxae - emotion, fright, and also physical exhaustion through illness or over-exertion.

If I consider this aetiological formula for anxiety neurosis in detail, I am able to add the following remarks. Whether a special personal constitution (which need not be produced by heredity) is absolutely necessary for the production of an anxiety neurosis, or whether any normal person can be made to have an anxiety neurosis by some given quantitative increase of the specific factor - this I am not able to decide with certainty; but I incline strongly to the latter view. -Hereditary disposition is the most important precondition for anxiety neurosis; but it is not an indispensable one, since it is absent in a class of borderline cases. -The presence of the specific sexual factor can, in the majority of cases, be demonstrated with certainty. In one series of cases (congenital ones) this factor is not separated from the precondition of heredity, but is fulfilled with the help of it. That is to say, in some patients this peculiarity of the vita sexualis - psychical inadequacy in mastering

somatic sexual tension - is innate in the form of a stigma whereas ordinarily it is via that peculiarity that they acquire the neurosis. In another class of borderline cases the specific cause is contained in a contributory one. This is when the psychical inadequacy which I have just mentioned is brought about by exhaustion and such causes. All these cases fall into classes which melt into one another and do not form separate categories. In all of them, moreover, we find that the sexual tension undergoes the same vicissitudes; and for most of them the distinction between precondition, specific and auxiliary cause holds good, in conformity with the solution of the aetiological equation which I have given above.

When I consult my experience on this point, I cannot find that there is any antithetic relation as regards anxiety neurosis between hereditary disposition and the specific sexual factor. On the contrary, the two aetiological factors support and supplement each other. The sexual factor is usually only operative in those who have an innate hereditary taint as well; heredity alone is usually not able to produce an anxiety neurosis, but waits for the occurrence of a sufficient amount of the specific sexual noxa. The discovery of the hereditary element does not, therefore, exempt us from searching for a specific factor. On its discovery, incidentally, all our therapeutic interest as well depends. For what can we do therapeutically about heredity as an aetiological element? It has always been there in the patient and will continue to be there until the end of his life. Taken by itself, it cannot help us to understand the episodic onset of a neurosis or the cessation of a neurosis as a result of treatment. It is nothing but a precondition of the neurosis - an inexpressibly important precondition, it is true, but nevertheless one which has been over-estimated, to the detriment of therapy and theoretical comprehension. To be convinced by the contrasting state of affairs, one has only to think of the cases of nervous diseases that run in families (such as chorea chronica, Thomsen's disease, and so on), in which heredity unites in itself all the aetiological preconditions.

In conclusion, I should like to repeat the few statements in which I am accustomed, as a first approximation to the truth, to express the mutual relationships between the various aetiological factors:

(1) Whether a neurotic illness occurs at all depends upon a quantitative factor - upon the total load on the nervous system as compared with the latter's capacity for resistance. Everything which can keep this quantitative factor below a certain threshold-value, or can bring it back to that level, has a therapeutic effect, since by so doing it keeps the aetiological equation unsatisfied.

What is to be understood by the 'total load' and by the 'capacity for resistance' of the nervous system, could no doubt be more clearly explained on the basis of certain hypotheses regarding the function of the nerves.

(2) What dimensions the neurosis attains depends in the first instance on the amount of the hereditary taint. Heredity acts like a multiplier introduced into an electric circuit, which increases the deviation of the needle many times over.

(3) But what form the neurosis assumes - what direction the deviation takes - is solely determined by the specific aetiological factor arising from sexual life.

Although I am aware of the many still unsolved difficulties of the subject, I hope that, on the whole, my hypothesis of an anxiety neurosis will prove more fruitful for an understanding of the neuroses than Löwenfeld's attempt to account for the same facts by postulating 'a combination of neurasthenic and hysterical symptoms in the form of an attack.'

VIENNA, beginning of May 1895.