within and receive influences from without can be best conceived after the analogy of the reciprocal social influences of conscious selves. If growth be internal to the real universe then the unity of the latter must be constituted by the mutual influences of elements which live at once in transient and immanent relations. The fundamental reality in the relations of things is the reciprocity of influence among living centers in a system. This system by the very living and conscious character of its elements and the mutuality and directness of their influences and development may properly be called *spiritual.*

To develop adequately this conception of the ultimate significance of relations would carry us far afield. We should have to pass the limits of a journal article and embark on the wide sea of metaphysical system. With this suggestion of where the theory of relations leads I must close this necessarily meager article.

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DISCUSSION

UNSCIENTIFIC METHODS IN MUSICAL ESTHETICS

THAT the terms used for a scientific theory must be defined, is self-evident. Scientific terms are words; they must be words, to be written and spoken, since the object of science is the communication of knowledge. Most of the modern sciences have been very fortunate in introducing a terminology made up of words which had practically no meaning whatever before the scientist gave them a meaning by referring to *a definite group of experiences.* Practically all the scientific controversies of earlier centuries concerning terms now well defined, for example in mechanics, arose from the fact that a term employed had a vague meaning before it was used as an arbitrary symbol for a definite group of experiences. Mach's 'Science of Mechanics' gives instances enough of this sort. Instances of a similar obstacle to progress in a distinctly modern science will be found farther below (observe the terms 'rhythm,' 'esthetic').

There is a science of very recent origin which is very unfortunate with respect to terminology—esthetics. We must not permit ourselves to be deceived by the fact that many of the terms used in esthetics are clearly of Greek or Latin derivation. However true this may be, they had long ago become associated with a large number of experiences other than those referred to when we first met them in the beginning of a book on esthetics.

Under these circumstances the esthetician ought to be most care-

ful in his terminology. If he is not careful, the result is metaphysics, *i. e.*, terms introduced as symbols for very definite groups of experiences are forgotten to have been introduced thus, and are used for the construction of premises under their old meaning, with all the wealth of their previous associations. Of what scientific value are the conclusions drawn from such premises?

Of course, every one is liable to frequent reversion to his old views. This is human nature. A distinguished author does not lose his distinction when indefiniteness of his terminology can be demonstrated. But if it can be demonstrated, it ought to be, for the benefit of those readers who otherwise might accept a terminology and a system based thereon without seeing the dangers resulting therefrom. These dangers are much greater in the German language than in the English. The indefiniteness of German psychological terminology (I speak of esthetics as a branch of psychology) is very conspicuous in comparison with the English terminology. This, at least, is the writer's impression. It is possible that he is here influenced by the fact that he was familiar with German long before he learned English, so that his German words possess more early acquired associations; however, this is probably more than an individual impression. How much the German language has been misused by speculative philosophy is well enough known.

My intention now is to criticize the terminology and the methods of procedure of a school of German psychologists who have paid special attention to the theory of melody, a problem in which the writer, too, takes a particular interest.

I have in mind Theodor Lipps and his pupils. Quite recently appeared a paper in the *Zeitschrift für Psychologie* which may be regarded as the most significant contribution of this school to musical theory, Weinmann's article 'Zur Struktur der Melodie.' That its views are distinctly Lippsian, is clear from the fact that Lipps is quoted several times on nearly every page, while only a few scattered and rather unimportant references to Meumann, Wundt, Stumpf and Helmholtz appear. Writers other than German are regarded as non-existent. Weinmann's intention is entirely confined to an application of Lippsian views to some musical facts—I should better say: apparent musical facts—to which these views had not been applied before. Within its scope the article is very valuable indeed. Its chief value consists in describing in *Lippsian terminology* the simplest and most common tone phrases which one can find embodied in practically any familiar tune.

Now let me raise the question: what are the fundamental differences between the scientific methods employed by the Lippsian school and the methods employed by the present writer in the same field of investigation?

In founding a science we must try to establish a set of fundamental laws formulated with absolute definiteness and clearness, and then show by a never-ending experimental application to the facts of experience that these laws are correct. By definiteness and clearness I mean that our terms, the symbols we use, must refer to relatively simple and easily observable facts. Whenever such an application necessitates a correction of a law, or the formulation of an additional law, we must act accordingly. What now are these laws in music?

The Lippsian school proceed thus: They select for investigation, by a very arbitrary method, what seem to them the most common, the simplest musical phrases in their own national music only; and in order to find the elements of melody, they divide these musical phrases into as many musical elements as they happen to find, making, thus, the fundamental laws of this science dependent on the chance ability of the observers to analyze completely experiences of extreme complexity into elements, despising a more perfect method because they hate the laboratory. They formulate the psychological laws of the esthetic effect of these elements as they happen to strike them when heard within larger phrases, despising any and every attempt to separate experimentally the esthetic elements for this purpose, save such superficial experiments as can be made on any piano. They formulate the laws in terms which are derived from the Lippsian system of psychology rather than in terms which are defined to mean the fundamental musical experiences themselves. They explain the esthetic effects of particular musical phrases by formulating the effect of particular combinations of elements in laws which they subject to the test of agreeing with the Lippsian system rather than to the test of experiment. They thus construct a theory which is very satisfactory to every one who confines himself within the scope of music treated by the Lippsian school and within the scope of the Lippsian psychological system, but which must appear rather barren to him who rejects such limits, and who believes in higher ideals of scientific research than medieval dialectics.

I shall illustrate these charges against the scientific methods of the Lippsian school by a critical discussion of the most striking instances in Weinmann's paper. But, first, I ask the reader's permission to characterize briefly my own work along similar lines. Compared with the apparent results of the Lippsian school, my own results look very meager. But this meagerness has its advantages.

I did not start from any circumscribed class of music. I spent —the Lippsian school would probably say 'wasted'—much time in trying to find out *all* those combinations of two tones each, repre-

sented by any possible ratios, which strike us as possessing melodic relationship. After having reached a subjective certainty of the correctness of my observations sufficient for a preliminary formulation of a psychological law, I formulated the result in a fundamental law of melodic relationships. This agrees largely with Lipps's law, but differs in certain very important parts. It differs because it is the expression of my observations, whereas the Lippsian law is the result of a speculative derivation from a pseudo-experience of 'micro-rhythm.' I then constructed an absolutely comprehensive table, showing *all* the possible relationships which can be found in *any music* made up of *any number* of related tones, in order to have a definite basis for experimental research. And then, it is true, I did not develop a system of esthetic effects of a limited number of the most common and practically most important melodic phrases, described in terms of a particular system of psychology, but found myself overwhelmed by innumerable questions of fundamental importance which can only be solved experimentally, and which, without an experimental solution, will never be a scientific theory of music. Only a few, very few, of these questions I have been able, as yet, to investigate experimentally far enough to publish the results. The reader may find them in the *American Journal of Psychology,* Commemorative Number, Vol. XIV., No. 3–4, July–October, 1903. How one can dream of solving all these problems, instead of subjecting them to a rigid experiment, by applying to them flowery, esthetically sounding names of distinctly Lippsian color, is hardly comprehensible to an experimental psychologist.

What I regard as the chief result of my own labors in this field is to have made it easier, in some cases I may say even *possible,* to formulate questions with such an accuracy that their experimental solution can be attempted. If any one can find any stimulation towards an experimental investigation in Weinmann's paper (I take this as a representative of Lippsian type) I wish he would tell me in what line on what page. I have found there only a constant encouragement of dogmatism. What I claim for my work is that it helps to raise questions simple enough for experimental investigation. What I find in the work of the Lippsian school is that it tries to satisfy those who are looking for a finished system, despising any experiment except perhaps what can be performed in an arm chair before the writing desk.

I shall now give illustrations.

Very characteristic is the beginning of Weinmann's paper: 'A melody is a unity, a whole, not a mere succession of tones.' This can not mean anything but the fact that only those successions of tones in which we experience relationships between the tones are

called melodies. By 'unity,' then, must be meant the existence of such relationships. Nothing beyond this can be meant, if we remain within the field of science. Weinmann now adds to this definition of the term unity, although this means obviously *esthetic unity,* the term 'esthetic,' without giving a sufficient reason why the word unity alone should be insufficient as a linguistic symbol for the experience of relationships; with the secret purpose, on the contrary, of enabling himself to deduct dialectically from these two words a speculative system.

"And it is an *esthetic* unity of elements which are comprehended in one element to which the other elements are as subjects to a monarch."

Two points of criticism must be brought out:

1. Is the term 'esthetic unity' generally accepted as meaning *subordination* of all the elements of an artistic structure to *one* of its elements? If this were a definition which had been proved to be of scientific usefulness in *all* the other divisions of esthetics, then it would be justifiable to try it, at least as a preliminary definition of *esthetic unity in music.* I am not aware of sufficient reasons to adopt it in other fields of esthetics. The only justification, then, for its acceptance here would be the actual proof that *in tone combinations* esthetic effects are found exclusively when there is such a subordination to a monarchic element. Weinmann obviously takes either this or the universal acceptance of his definition of the term esthetic for granted, without even saying which. Is this a scientific method?

2. I have always protested against the Lippsian definition of melody by means of 'subordination of all the elements to one.' This definition is absolutely in contradiction to my own introspection. All I observe as necessary to speak of an esthetic effect, of melodiousness, is the existence of relationship, not of subordination. It is obvious that this difference of opinion is of fundamental importance. How can a theory of melody be regarded as of any considerable value before such a discrepancy of opinion is settled by experimental methods of investigation? Weinmann starts from his *narrower* definition of melody as from a dogma, as if this were not a matter of observation at all, without hinting by a single word at the possibility and actual existence of a wider definition. Is this a scientific method? Is it useful?

If we accept the Lippsian definition of esthetic unity in tones, we limit the extension of musical science to the music to which Weinmann's discussion limits it. In the music which we have as the result of historical and sociological factors among the European peoples there may be an infinitely small percentage of actually used music which does not possess such 'subordination.' But how about

other music, *e. g.*, Japanese music, to which the writer has paid some attention? It is justifiable to exclude such music from investigation by the very definition of 'esthetic unity'? To me and to others who have heard them, some such melodies possess 'esthetic unity' without showing the subordination of the tones to a monarch tone. Are such observations of less scientific importance than the demands of a speculative system for completeness, for being able to pretend to be a final truth? Can such an arbitrary limitation of the field of scientific investigation in esthetics be called a scientific method?

I said above that the Lippsian school starts, not from an experimental investigation of the elementary facts, but from complicated musical phrases, relying entirely on the ability of its members to analyse them. It is not wonderful, then, that their laws are always narrower than mine. One of these musical phrases is the diatonic scale, which is introduced by Weinmann on page 345 as if it were a divine revelation. No attention whatever is paid to my endeavor to show why the diatonic scale is of so much practical importance for music without being itself a fundamental fact of esthetic natural law. No attention is paid to my endeavor to show that the diatonic scale of musical practice is an extremely many-sided structure, that it is absurd to speak of *one* diatonic scale in just intonation. What I reject is made the corner stone in the Lippsian system; and this not on the basis of any sound reasons given, but entirely dogmatically. Is this a scientific method?

The result of starting from this dogmatic basis is, for instance (p. 349), that both the relationships 2–9 and 3–5 are treated as unmelodious, called 'dissonances,' and are placed into the same class with 2–45 and 5–27 (no relationships at all!); and that a general psychological law of 'resolution' of dissonances is formulated (p. 350) thus: if two unrelated tones appear, they demand the passing of the melody to a tone closely related to both. I venture to say that there is no such law of resolution as this. The few examples given by Weinmann can be theoretically understood without his law. And what is commonly called 'resolution of dissonances' represents too complicated a problem to be solved by the statement of the above 'law.' With respect to Weinmann's classifying the ratios 2–9 and 3–5 with 2–45 and 5–27, I request him to tell who revealed this to him. It can not possibly be the result of an observation of such tone combinations in isolation. It seems to me that it is merely Lippsian doctrine, derived speculatively from his 'diatonic scale.'

Let us turn to another point. The question at issue is this. If one group of facts of experience, A, is well enough known, so that

we can formulate some of its natural laws, are we permitted to *derive* the natural laws of another, different group, *B*, by mere linguistic operations from the laws of *A*? I deny this. The laws of *B*, which is admitted to consist of experiences different from the experience of *A*, can only be found by experimentally studying *B*. The reader will probably ask, what indeed could induce a sane person to derive the laws of *B* from *A*? What can induce some to do this is the hypnotizing power of a *name*, the same name given to both, to *A* as well as to *B*.

It would be scientifically correct to give the same name to *A* as well as *B*, if, and in so far only as, the most fundamental laws of both groups had been found experimentally to be identical. So, *e. g.*, may certain most fundamental facts of heat and light be referred to as 'ether vibrations,' this being used as their common name.

Suppose, now, the laws of light were known in detail, those of heat unknown except that heat were known to resemble in a superficial way the experiences of light. Suppose further, that some one had happened to refer to this resemblance by means of using the same name, ether vibrations, for both. Would, under such circumstances, any physicist have thought for a moment of deriving the unknown laws of heat from the known laws of light? Little knowledge of physics is necessary to know that the laws of heat, as we have them now, could never have been deductively derived from the laws of light, that on the contrary they were actually found by experimental studies of heat.

The Lippsian school are blind to such obvious facts of scientific methodology. How do they proceed? There is a group of *physical* experiences, *B*, to which the physicist refers by speaking of frequency of vibration rates. There is another group of *psychological* experiences, *A*, generally called rhythm. If any one thinks that *rhythm A* and the *esthetic* effects of the physical group *B* obey identical laws, *he has to prove it* by studying *experimentally A* as well as the esthetic effects of *B*. If he can prove thus, that not a few minor details, of course, but the most general laws in both cases are identical, he then has the right to refer to this (limited) identity by using a common name.

I now request every psychologist who is interested in the advancement of his science to address to the Lippsian school this question: You have called the esthetic effects of group *B* by the name of 'rhythm,' 'microrhythm.' Did you study group *A* (rhythm) as well as the esthetic effects of *B* experimentally with a result which justifies what you did?

Did they study experimentally group *A*, the psychological experience of rhythm? One is astonished to find that the latest mono-

graph on rhythm mentioned by Weinmann is Meumann's article published in 1894. Of the extensive literature on the subject issued during the last ten years and published chiefly in American periodicals he is absolutely ignorant.

Did they study experimentally the esthetic effects of group *B*? There is not the slightest reference in Weinmann's paper to what might be called a scientific experiment. On the other hand, my own experimental results are constantly contradicted by Weinmann's linguistic deductions with a naïveté, which would be impossible if he had ever read one of my papers. He is far above such earthly ways of scientific inquiry. And this wonderful empirical basis of knowledge gave them the right to refer to both groups of experiences by the term 'rhythm,' ' microrhythm'!

However, if they would stop here, little harm would be done beyond confusing careless readers. But they do not stop here. Having arbitrarily called group *A* and the esthetic effects of group *B* by the same name 'rhythm,' 'microrhythm,' they proceed to derive—listen and wonder—by purely linguistic operations (I call attention to the frequency of 'demnach' in Weinmann's paper) *the laws of tone relationship from the laws of rhythm.* Whom does it astonish, then, that their results differ from those of my experiments? Does their linguistic skill invalidate my experiments, or do my experiments invalidate their linguistic results?

Here a few instances. I have experimentally shown, so far as a careful interpretation of experiments can show anything, that the deductive theory of Lipps concerning the esthetic effects of a movement from the lower to the higher octave, or the reverse, is experimentally unfounded, an unnecessary complication of the scientific theory. Weinmann repeats the deduction of his master. No hint at an experiment of mine. No hint, of course, at an experiment of his own. The idea seems to be: only reiterate your statements as frequently as possible; the scientific public will then gradually get accustomed to them and overlook their speculative origin.

Another instance: I have shown, I think conclusively, by experiment, that between our satisfaction with tempered intonation and the tendency towards a characteristic intonation of the different intervals there exists no causal connection. Weinmann repeats the opposite opinion on the mere authority of Lipps. No hint at an experiment of mine or at one of himself.

The authority of the master makes itself noticeable also in Weinmann's theory of the difference between minor and major music, 'Moll' and 'Dur,' as the German terms are. The doctrine of the Lippsian school is that, each having of course only one key note, *i. e.*, chief note, 'Moll' has 'four despotic notes' in addition, 'Dur'

only 'two despotic notes.' No attempt is made to derive this theory of 'Moll und Dur' in a mathematically correct manner from the fundamental laws of tone relationships as found combined in what we call minor music. It is derived by linguistic skill from a minor scale, the intervals of which (in ratios) must have been revealed to the master and given by him to the school as a dogma. So I suppose, since nothing is said about the theoretical origin of this scale of intervals save the statement that this scale 'als die eigentlich massgebende Form für die Verhältnisse in Moll gilt' (gilt = is accepted!).

Strange to say, Weinmann adds here the statement that the esthetic character of minor music has given to it the name of 'Moll' as opposed to 'Dur.' The present writer did not expect to find this in a serious article. It is well known that the names 'Dur' and 'Moll' do not refer at all to the esthetic character of any music, but to the manner in which medieval musicians used to write a symbol of musical notation, square or round.

It seems to me that, before one attempts to enter as deep into the details of melodic construction as Weinmann tries to, the foundations of the science ought to be placed on a secure ground by experimental investigation. Otherwise, one's theoretical interpretations of a melody may be as beautifully sounding as this of Weinmann's (p. 374): 'Demnach macht eine in den Tönen des übermässigen Dreiklangs sich bewegende Melodie den Eindruck des Unbegrenzten, Offenen, des sich Ausweitenden und Verlierenden, der starrenden, öden Leere, wie des plötzlich Entfesselten, des schrankenlosen Ausbruches, sei es der Freude, der Lustigkeit oder des Zorns, des Entsetzens.' But such interpretations are not scientific. They do not convey any definite knowledge. They appeal to our familiarity with unanalyzed emotional complexes in order to hide the vagueness of the terms of which they are made up.

My criticism may be superfluous. May be the Lippsian school does not intend to further science. Weinmann says himself what his intention was: not to contribute to a theory of music based on experimental investigation. No such expression as this is to be found in his paper. His intention was: 'die Weiterführung der Ansichten von Lipps,' *deduction from the opinions of Lipps.*

I should regret if my criticism should seem to be personal. There is hardly a psychologist from whose publications in general I have learned more than from those of Lipps. And I have learned this and that from Weinmann's article too. I wish to criticize the methods of a school, not the personality of its members. I wish to protest against their despisal of the experiment, against their unscientific methods, for Science's sake.

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