

1913

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Recommended Citation

Paul Menzerath, Association Method in Criminal Procedure, 4 J. Am. Inst. Crim. L. & Criminology 58 (May 1913 to March 1914)

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THE ASSOCIATION METHOD IN CRIMINAL PROCEDURE.¹

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In the following article, which is intended as a statement of principles rather than as a review of literature, two closely connected problems are to be discussed. So intimate, indeed, is their relationship that the second stands or falls as a deduction from the first. On the one hand we are concerned with the association method as a scientific question; on the other, with its practical application in criminal procedure. We shall attempt, therefore, to set forth the true psychological import of the association method in general. Then to this exposition we shall join the question of the extent to which the method may serve as an heuristic means of analysing cases or diagnosing fact in present day criminal procedure, aside from any consideration of its general admissibility under existing laws. Our exposition has value chiefly on the side of theory. It is, moreover, predominantly critical.

The time when belief was accorded to unsupported testimony is past. Testimony is regarded critically. The jurist now seeks to restore circumstantial evidence to the place it has lost. Subjective report is preferably replaced by objective datum, an enormous significance thereby accruing to the metric methods of anthropology; to dactyloscopy, for instance. And this tendency is justifiable.

Psychology has to face a problem which in principle is comparable to that of dactyloscopy. In the latter, however, provided the culprit has been captured, the print gives him to the criminologist infallibly—or nearly so—while to the psychologist come the questions—How far is it possible to convict a person of guilt or of the knowledge of guilt without his help, or even against his will? Can convictions be made on a basis of infallible, unequivocal and relatively simple data? Is it possible at least to demonstrate the innocence of an accused?

Psychologically the matter appears relatively simple. Here is a

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crime. There is a subject, or perhaps a number of subjects, from whom the guilty person is to be sought out.

It is a mere matter of finding the method. In these cases we have always to do with a state of affairs which has a strong affective tone—this at least is the opinion expressed by most authors. Our question, therefore, is a part of the more general psychological inquiry: to what extent do affective processes make themselves externally distinguishable?

Methods are very numerous. Some are of a psycho-physiological, some of a psychometrical nature.² Pulse, respiration, heart-beat, all in fact exhibit different sorts of pattern according with mood and changing with alterations of it. Graphic methods of registration (sphygmographic, pneumographic, cardiographic) are of especial importance here. Blushing, paling, quickened respiration, and catches of the breath are well-known affective symptoms. Their registration is relatively easy, even though it requires a considerable amount of apparatus. The small and delicate unconscious movements of the finger also admit of graphic registration. Finally—and here it seems to me personally that the most promising field for future work lies before us—we may think of recording the phenomena technically known as the psychogalvanic reflex (Veraguth). The procedure consists in recording the variable movements of a millimeter which registers changes in the electrical conductivity or resistance of the skin. Numerous other methods have been proposed, but insufficiently tested. I pass them over and come now to the method which is to be dealt with in somewhat greater detail—the association method.

The train of reasoning is comparatively simple. It is this. Let us grant the absolute determination of every psychological reaction. Then from words which refer to a given state, noteworthy because affectively toned, there must follow a reaction (qualitatively and quantitatively) different from that which follows from words not thus connected. We call the whole affective state a "complex." Words which have reference to this state we shall designate in what follows as "complex-words," as opposed to non-complex or "indifferent" words. The method has proceeded essentially from an incidental observation of associations; the observation, namely, that reactions follow certain stimulus-words very slowly or perhaps not at all, while in the majority of cases they run off smoothly in the ordinary way [*nennungslos*, (Jung)]. The method in itself is exceedingly simple.

²I shall not here consider those methods which we call reintegration methods, such as the completion of drawings (Verworn), or of incomplete texts (Stern).

The experimenter calls out a word to the subject and requires that the subject respond to it by giving another word as quickly as possible. This is the method as generally employed. It involves a number of errors, and especially the error of auto-suggestion on the side of the experimenter. That is to say, if the experimenter has come to the opinion that the guilty person is before him, he will make his time-registration different from what he would in the opposite case. The objectivity of the method—a fundamental requirement—is not guaranteed by this (auditory) method. It is, however, the only method which can, before as after, be used with the illiterate or feeble-minded. The visual presentation of stimuli, wherein printed words are exposed in a special apparatus (the card-changer), is preferable, since it is more objective and hence more trustworthy.

Different reactions, we have said, will appear in the case of a guilty person and an innocent one. The diacritical marks which make a differentiation possible are called complex-symptoms. The principal complex-symptoms that have been established are:

1. Of a *material* nature (that is, reactions which point directly and immediately to the supposed situation).

2. Of a *qualitative* nature: such things as assonances, mutilated reactions, failures to react, translations into foreign speech, phrase-reactions, provided that otherwise the reactions have been single words, "sets" in a single direction, perseveration, repetition of the stimulus-word, auxiliary words, misreading or mishearing.

3. Of a *quantitative* nature: the reaction-time for "critical" words, that is, those indicative of a complex, is often lengthened. The delay appears also with the "post-critical" words. Finally what we know as "disturbances of reproduction" (Jung) should be mentioned. They are determined as follows: After obtaining a series of a hundred associations, the experimenter gives the same series of stimulus-words a second time. It will now be found that the answers given to certain words the first time are repeated easily and with certainty. Other answers, however, undergo change or indicate a long effort of memory. There appear finally the phenomena which I may designate analogously as "disturbances of repetition." Kraepelin, as early as 1888, found that repetition of the same series of stimulus-words after stated intervals of time brought about reactions which in part were identical with, but in part also differed from the answers resulting from the first presentation. Jung found that the "disturbed reproductions" belong chiefly to the complexes. Pfenniger recently proved a similar fact about the "disturbed repetitions." "Changes occur

least often with those stimulus-words which in the first series gave reactions without marks of the complex.”

I myself have refined the two last mentioned methods. In one respect I oppose the method in Jung's work. (I have never taken more than twenty reactions on the same day, in order to exclude the uncontrollable influence of fatigue, especially with sick persons.) On the contrary, I maintain the principle of distributing the series of stimulus-words over as large a number of experimental days as possible.* On the last day I required a repetition of the whole series. Only those stimulus-words presented on the last day, therefore, were “reproduced” (in Jung's sense). In this way, also, the connection of the individual stimulus-words with the complex may be demonstrated in an unobjectionable manner. Indeed, the differences sometimes become conscious to the subject himself, especially if the commendable artifice of requiring a report on subjective certainty is employed; that is, if the person is asked each time whether or not he is sure of his answer. With this procedure we may observe the four following cases: 1. The subject gives the same reaction-word and is quite convinced of the correctness of his report. 2. The subject is doubtful about the correctness of his report. 3. The subject gives an answer, but is convinced that it does not coincide with his first answer. 4. The old reaction has completely vanished from memory. Cases 2, 3 and 4 rank as “disturbances,” as opposed to the right answers of case 1, and serve with those mentioned above as complex-symptoms. It may well happen, as we have just indicated, that these differences are observed by the subject concerned. One of them actually said to me spontaneously: “It is remarkable; I am entirely certain of some answers, but not of others.” It should also be noted here that the subjective certainty has nothing to do with the differences in time (somewhat over two months), which I imposed. As regards the report of certainty, that is to say, the answers first given do not suffer more than those given last. Otherwise expressed confidence in the report is not directly dependent upon time elapsed.

There is no doubt, then, as to the fact of complex-symptoms. Still a word, however, as to technique. We gave the visual method of presentation preference over the auditory in what we said above. We must concern ourselves yet further with the matter of registering the time. It is certain that the primitive method of measuring time with the stop-watch measuring fifths of a second can no longer be

* The initiated will recognize at once that this criticism is not relevant to case-analysis (*Thatbestandsdiagnostik*), where the cumulation of stimulus-words is practically inevitable, but that it applies rather to psychoanalysis.

adhered to as it has been formerly, for, as already shown, it is not free from objection. The principle of mechanical time-registration must be unconditionally maintained. The chronoscope (d'Arsonval or Hipp) should be used. Under certain circumstances, however, in connection with the card-changer, the use of the stop-watch may still prove permissible for practical purposes if the following arrangement is made: The card-changer is placed before the experimenter and opposite to the subject, and the watch is started simultaneously with the pressure on the lever. Since then the sequence of stimulus-words is not visible to the experimenter, auto-suggestion drops practically out of account. In this way also the introduction of a third (neutral) person to read times is rendered superfluous. In my own experience this arrangement has given excellent service, especially with an additional lever-system which makes the release of the lever on the card-changer possible by means of the same pressure which starts the watch.

I have purposely given so much space to the technical side of the method, for only with a technique which is free from objection can objectivity of results be guaranteed.

Suppose now this sort of technique has been actually attained. We may now ask whether or not we are today in a position to settle the question of guilt or innocence from the unequivocal, unconditionally reliable data.

I know perfectly well that other authors have answered this question directly and very boldly in the affirmative. In my opinion their answer is incorrect, for as a matter of fact the association method does not meet the requirement.

Still another problem must be considered along with the methodological and technical aspects of the experiment. A problem, moreover, which although it has received no attention hitherto, is necessarily the cardinal point of the whole matter. I mean the question of the sensitivity of various subjects to a complex (*Komplexempfindlichkeit*).

The following instance will show briefly how we came to raise the general question of complex-sensitivity: One of our cases was a highly intelligent alcoholic patient about fifty years of age. He had ruined his family by his way of living and had even gone so far at times as to lay hands on the property of his fellowmen. Here we had a case such as might actually be given in practice. But what resulted from the use of the association experiment? Briefly expressed, nothing; for not a single reaction indicated the existence of the two complexes (alcoholism, theft). If we had not had other

knowledge of these complexes, we should have been obliged to pronounce him innocent unconditionally. We escape the dilemma, however, by this explanation: in cases of chronic alcoholism complex-sensitivity is often so reduced that it cannot be determined by the use of the association method.

Under such conditions, then, our artifice is of no avail. The further question, whether an intelligent subject with great presence of mind may altogether succeed in concealing his complexes, still remains. The answer has turned out variously. It is indeed true that such a result does not appear generally with the normal subject; at least I have met with no single instance in a large number of experiments. I intentionally express myself with caution, however, for the opposite possibility is not to be rejected *a priori*, especially if the given subject understands the scope of the experiment and has a knowledge of complex-symptoms. And special notice should be taken of this fact, for, as is well known, the clever criminal quickly appropriates scientific results and prevents their effectiveness.

Let us admit that insensitivity to a complex is an exception, then. The existence of "exceptions" must straightway be made our special concern, for only in such a way can we give our results that measure of certainty which a diagnostic psychology of the criminal must unconditionally offer.

We mentioned alcoholism as one of these exceptions. It does not stand alone, as psychopathological investigations have sufficiently shown. We must give such cases a somewhat more careful scrutiny. Wladtschko was led to believe from his experiments on paranoiacs that they, in so far as no dementia is present, are able to conceal their complexes extremely well. We were not able to verify this conclusion. On the contrary, we investigated paranoiacs who very certainly were not demented, and who nevertheless showed the recognized complex-symptoms actually in singular purity. Further investigations which we have already undertaken are to settle this debated point conclusively.

On the other hand, we were able partly to verify, partly to rectify Pototsky's results with the traumatic neuroses and the neuroses after skull-injuries (with and without loss of brain substance). We were concerned especially with two groups of these neurotic cases. The members of the one group did not react to complex-words—which in this case had reference to the accident and the payment of indemnity—by giving any signs of the complex whatsoever. Complex-words have the same value for them, therefore, as do indifferent words. On

the other hand, with others, just as with many hystericals, the reaction is exaggerated; they weep, curse, gesticulate, and so on.

If this result is of special prognostic value—such that those who are insensitive to the complex are threatened with dementia post-traumatica (Koppen)—then the fact in itself is of eminent psychological significance, for it is well suited to bring complete clearness into an acknowledged matter of dispute. Lipmann, for example, has attacked the old view that complex-symptoms are chiefly signs of affective sensitivity. He has held, as opposed to this, that a process experienced with interest, and not the affectivity of the experience is the thing of consequence. As we have pointed out, the pathological cases settle the dispute neatly, and of course, adversely to Lipmann. Call to mind again in this connection the case of chronic alcoholism mentioned above. There the complex (theft and alcoholism) was demonstrably and admittedly touched.⁴ In spite of this fact every objective symptom of it was absent. On the other hand the traumatic neurotics, especially those cases in which imbecility had already appeared, furnished examples of paradoxical complex-symptoms. (We have observed the same thing repeatedly with hystericals.) That is, those very reactions which are connected with the complex are exceptionally short—sometimes so much shorter that they attain to only half the reaction-time of indifferent stimuli. Just here, however, the affective moment can no longer be considered; good-natured imbecility or emotional bluntness is very pronounced and the complex is of a purely menemic nature.

A result which grows out of other experiments is also in place here. I refer to the question of the emotional moment in the morally weak-minded [moral insanity (Prichard)], which is also a well-known subject of debate. Hermann has recently asserted that his attempts to use the association method with the morally weak-minded met with no success. This is an inexplicable assertion; our own experiments in all cases brought proof of even a rather low "complex-limen," that is, a high complex-sensitivity.⁵ These persons, then, are amenable to the association method, although the alcoholics with their blunted feelings are not.

And so we see that we cannot apply the method to a very large group of criminals. We may say in fact that it is just the most cunning rogues, the most polished and hardened criminals who elude the association method; in these cases, according to the unanimous state-

⁴This was established by subsequent questions.

⁵Thus we take a stand on the question whether lack of moral feeling is a general, or so to say, a local sort of thing.

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ment of Weggandt, Jung, Patrizii and of Umberto Fiore especially, a bluntness of feeling is characteristic.

The association method, then, does not fulfill the requirements. It is not possible in an absolute sense to convict the guilty person, since actual guilt does not necessarily express itself in complex-symptoms. It is not possible either to prove the innocence of a subject—as Alf. Groos especially has maintained—for diagnostic signs do not have the certainty which permits a conclusion of this sort to be drawn.

And so the second fundamental question may be answered without further delay. The association method does not possess practical value as a means for case-analysis. We decidedly have not disposed of the possibilities, and even less of the exceptions. Only when we can control all cases without exception may we think of use and practical value.

But in principle the answer to our question still remains in doubt. The general failure of this sort of investigation does not at all follow from the fact that the association method does not fulfill the requirement. Methods which will furnish more certainty may eventually be discovered. It seems to me, for example, that an improvement in the matter of the psychogalvanic reflex may do so.

Above all I do not wish it to be inferred from the foregoing statements that I regard the whole series of investigations as an unpromising failure. A skepticism of that sort would not be justified merely by our momentary impotence. The chief purpose of our discussion has been rather to set the countless difficulties of experimental investigation in their true light and to warn against precipitancy. From our discussion we may settle a question which really seems superfluous in the asking—but when psychology is spoken of every one thinks of himself as an expert—the question, namely: to whom should the prosecution of investigations be left? It is quite clear that it can only be to the psychologist, and more especially to the psychiatrically trained psychologist. There is reasonable certainty that if this delegation be made, the uncritical works of certain jurists will soon be superseded.

For the moment, however, as we have said, there cannot in the least be talk of practical application. It would prove an exceedingly bad service to the psychologist himself, for a fiasco would soon occur, and therewith psychological case-analysis would be banished from juristic procedure for years.

In the first place, the theoretical questions should be cleared up; so far as possible, moreover, by experiments on criminals themselves. Were every large prison provided with a psychological laboratory, or

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even were psychologists permitted to make an investigation on prisoners in jail (after their sentence), much would already be gained. A definite and adequate method would thereafter take shape, for there is no lack of capable and enthusiastic investigators for the field.

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