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Presentation of
Madeleine Jeanneau
Word Pattern and Psychological Structure
Empirical Studies of Words and Expressions related to Personality Organization
Doctoral Dissertation

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This is a great honour to be invited to this doctoral examination. The task imposed on me is to summarize the thesis. Before I ll do this let me remind you that Freud famous dictum from his introductory lectures that nothing but words are exchanged in the analytic session could have led to an early start of investigating language as the major medium of the psychotherapy. However it took quite awhile before this avenue for research was opened. Decades after Anna O´s. naively labeling of her treatment as talking cure (Breuer 1893) and quite in tune with the naive consumer´s view of today´s many psychotherapies "popular opinion is not alone in attributing a pivotal position to the talk in psychotherapy; in comparison to the behavioral (i.e. proxemic or kinesic) or physiological constituents of psychotherapeutic interaction, the talk which transpires between therapist and client has consistently been in the critical limelight-in psychotherapy research, theory and practice" (Russell 1987, p.1). For many years, however, the only way of knowing more about the therapeutic dialogue in the therapeutic situation was by listening or by reading

reading the case studies which constituted the major research instrument of the newly developing field (Kächele 1981). Dominated by Freud´s legacy psychoanalysis was a narrative science focusing on narration aspiring to narrative truth (Forrester 1980; Spence 1982).

The advent of tape recording in the forties provided for the first time "a sound basis for the investigation of therapeutic processes, and the teaching and improvement of psychotherapeutic techniques" (Rogers 1942, p 434). Today tape-recording of psychotherapy sessions is a prerequisite for studying discourse (Luborsky &. Spence 1971) and should by now be standard procedure for those who are prepared to undertake serious empirical research on the psychotherapeutic process. However, the number of those who expose themselves to this procedure is still small, nearly as small as the number of those willing to engage in the careful scrutinizing of what they do when practicing psychotherapy.

There are many reasons why many psychotherapists especially psychoanalysts are so reluctant to use tape-recording in their practice. First of all, since Freud's (Freud 1916/17) warnings against the presence of a third person are often extended to the presence of a recorder or of microphones in the consulting room, it is instructive to look at Freud's exact wording. He stated that the patient "would become silent as soon as he observed a single witness to whom he felt indifferent" (p. 18). This statement was used as an argument against the tape-recording of sessions.

Freud could not have known in 1916 that human inventiveness would one day produce a very unobtrusive instrument – the tape recorder. As we know, it gives an unmistakably correct account of the verbal exchange that is superior to any recollection by the therapist, including detailed notes written after the session or – as Freud preferred – late in the evening. Since Freud was trained to follow the rules of the natural sciences, we assume that he would

have welcomed new ways to assure accurate observation and data collection in the psychotherapeutic situation. When he pleaded for training analyses it was in part meant to reduce analysts' distortions in their understanding of their patients' free associations. Although this was not only an utopian but a misleading ideal for psychoanalysis we believe that it was offered in the same spirit in which we suggest that tape-recording and transcribing the dialogue offers a powerful tool for investigating the exchange between patient and therapist – to the extent that this is expressed in their language. Although much more does indeed happen on an unconscious and emotional level, it is the final aim of the psychotherapeutic process to translate or to interpret, that is, to put into words the patient's unconscious wishes and defenses. And these words are the starting point for further investigations.

Madeleine Jeanneau´s dissertation follows this lead. The title "Word pattern and psychological structure - empirical studies of words and expressions related to personality organization" points directly to the focus of the work. There are many ways to approach the study of language; she has chosen to study the potential relevance of the building stones of language - words and expression - for identifying clinically diagnosed personality structures. Other ways could have been focusing on the conversational strategies or identifying latent structures in the Chomskian sense. By selecting the hardware of language contrasted to the software consisting of rules she has taken a easy to quantify domain which is well suited to master a large corpus of text. Rule oriented language analysis is not yet feasible for a substantial amount of text as the history of the field of conversational analysis in psychotherapy research has shown up to now. For this we have yet to wait for computer based approaches that are able to master the intricateness of spoken language. By referring to the computer as a tool in this kind of research I can

connect Jeanneau´s work to an important development in the field since the early sixties.

Computer-aided analysis of psychotherapeutic discourse is as not as new as one might think of; its beginning go back to first approaches within social science propaganda research (Lasswell et al.1952). The first text-processing study using computing devices for content analysis was a study by Sebeok & Zeps (1958); for an analysis of 400 fairy tales of the Cheremis Indians they wrote a programme for the computation of word-contingencies. Somewhat later, but obviously without being well informed about that first effort Stones and Bales from Harvard University developed a first version of the General Inquirer System to study thematic changes in small discussion group protocols (Stone et al 1962). Starkweather & Decker (1964) reported on a programme for counting word-frequencies and type.token indices. In the same year Harway & Iker (1964) published their first paper on the WORDS-system. Working on a recorded psychoanalytic case they developed their philosophy of data reduction procedures "untouched by human hands"(Harway & Iker 1969; Iker & Klein 1974). At the same time Laffal worked on a comparative analysis of excerpts from the famous Schreber case performing various content analyses which later were run by computerized procedures (Laffal 1976). The monograph on the General Inquirer (Stone et al 1966) contained various examples of how computerized content analysis could be useful in psychotherapy research.

In her dissertation Jeanneau points out that this kind of computer based text analysis was introduced in Germany; the circumstance that this happened in Ulm in the beginnings of the seventies may well have contributed to my honorable job of today.

The first part of the dissertation consists in a careful summarizing of the theoretical and empirical body of knowledge we have up to now about the relationship of mental states to linguistic expressions. A charming aspect of the thesis consists in its effort to bridge between very fanciful notions derived from the French

Lacanian school of thought and the empirical minded anglo-saxon world. This in itself is highly appreciative; how far the bridge was built and which load it might carry has been perceived by the author herself:

"It is not easy to use these concepts emanating from French psychoanalysis in an empirical research situation and document their systematic presence in the speech of a person. The gap is too wide between theory and the empirical level (I,p.4). As bridging concept she uses the symbol and arrives thus at the assumption "that all the words being investigated form an order of symbols mirroring a psychic structure, a personality or a psychopathology "(I,p.10). The review of the existing empirical work on patient´s language is excellent and is a real updating of Vetter´s monograph on "Language behavior and psychopathology" (Chicago 1969) - which she must have missed somehow. The presentation of the research results takes up various facets of the field like "verbal patterns in various diagnostic groups", "linguistic signs of conflict and defense", linguistic aspects of the process of changes in psychotherapy. These investigations have precured a fair amounts of results on the immense variability of words or parts of speech. The results are not always pointing in the same direction which becomes especially clear in her discussion on the use of personal pronouns. The state of the art prepares the reader that the process of hypothesis formation was not likely to be an easy one; however it unmistakably points to the direction that the use of words, especially the little words, the so called unimportant not consciously controlled words, the minor encoding habits, is a promissing world to further explore. A decisive methodological step which may was based on the reading of the literature consist in the idea that not any single variable might be crucial , but combinations of them, which has been called linguistic fingerprint. This idea leads to the use of a multivariate statistics - principal component analysis, familiar to you all, more than to me, which I

understand to be a very useful tool for a discovery oriented strategy.

The objective of the study was to identify linguistic patterns differentiating three personality organisations that have been pronounced by Otto Kernberg in his clinical textbooks for quite awhile. In contrast to the wide acceptance of these concepts careful research on these three organizational levels of personality in a contrasting way has been rare. This is another merit of this study. However this starting point has some drawbacks insofar as **the DSM III diagnoses of the patients investigated - for example as described in the various studies displays quite a variation within the groups.** This is my first question. The framing of the patients within the theoretical system of Kernberg´s ideas about personality organization consequently leads to the use of the Structural Interview as a moderately standardized test situation to generate the speech samples. The study explicitly does not focus on patient-therapist interaction; however we know from the research on the so called alexithymia issue that **some features of spoken language are heavily dependent on the situation used to elicit the sample**. This will be another question of mine.

The first study sets out to test the French concepts LACK and DESIRE. 23 variables were chosen like adaptor words, negations, verbs expressing competence and so on. For each of the variable a hypothetical relation to either lack or desire is specified with DESIRE connected to the neurotic, LACK to the borderline and psychotic personality group. Principal component analysis performed on the data set of 21 patients with 23 linguistic variables achieved a weak separation along a continuum representing one significant principal component only accounting for 18 % of the variation. Increasing the statistical power is always a good way to learn more about one´s data; so by performed a PLS discriminant analysis the separation of the three diagnostic groups was improved. The author herself is very cautious in interpreting the

results; she knows that her statistical method is very powerfull "revealing all systematic variance in a dataset which is one reason why the outcome here may be difficult to reproduce" (II,p.18). However she was able to modify or correct first assumptions of the role of certain linguistic variables in their relevance for the theoretical notions of DESIRE and LACK. The problem then arises if this post hoc reasoning leads to better hypothesis formation or points to **the multifunctionality of language elements changing their functions within different contexts.** This will be my third question. Depending on one´s own preference one could be satisfied by finding that NPO patients are more talkative than PPO patients or if one feels inclined to attach them to higher order concepts like lack and desire. **Are there other alternatives to explain these data ?** - my next question.

The third study takes up the same issues with en enlarged sample (N=30) - I could not find out whether the first sample was enlarged or whether this constituted another, fresh sample. Also the set of variables was increased, containing now forty. This time the method of identifying the linguistic parameters on the screen was replaced by using the TEXTPACK system on offspring of the General Inquirer developed in Mannheim.
A first one-way of analysis of variance a number of significant differences among the three diagnostic groups were found that underscored the frequent use of conjunctions, negative adverbs, the adverbs here and now, verbs, the pronoun I, long utterances and a high proportion of speech in the interview were characteristic of NPO patients. These patients differed from the BPO patients in using positively charged adjectives and verbs and references to she, he more often. Differences also were found between BPO and PPO patients which I will not enumerate as well. (III,p.12).
The PCA approach led to one significant principal component accounting for 21% of the variance. To increase differentiation again a PLS discriminant analysis was performed which led to an

impressive diagnostic space chart of the three groups of patients. To better understand these distributions, to see on which words they were based the loadings of the variables were charted resulting in a word space chart, called nicely a linguistic landscape.

Let me use the author´s own words to summarize the main results:

The NPO was marked by a great linguistic variation, long utterances, comparative and deictic expressions. One particularly important variable was the relative frequency of the pronoun I. These variables and the great symbolization ability were interpreted as manifestation of high level psychological defences and identity integration. The BPO group was dominated by depressively charged words and impersonal expressions - interpreted as signs of vacillating identity and lack of psychological presence. The PPO group showed above all a (relative) lack of words and deictic expressions which was interpreted a a low linguistic intensity and defective symbolization activity.

To select only one detail of the mechanics of the procedure I point as the author does to the role of the TTR in this analysis. She found out that the only variable that pulls the differentiation along the X axis toward the negative side thereby characterizing the PPO group. The initial expectation was that PPO patients should exhibit a low TTR designating the clinically meaningful assumed repetitive behaviors of the PPO patients.. However this measure turned out to be extraordinarily high for the simple reason - as we know from the research literature - that PPO patients spoke significantly less than the patients in the two other groups. The author demonstrates herself that by taking 100 word samples the differences between the groups are very small indeed. So I feel inclined to ask - and this is my next question: **why did she not remove this variable from the long list and repeated the principal component analysis.** The general problem involved here is confronting everyone that works with language, this is the dependency of many variables from length of text. This has to do with the probabilistic nature of speech variables, the more text the

more likelihood a phenomena can occur. So length of text has to be controlled for and this is not always achieved by using percentages of total text.
A nice idea to control for chance effects is described as well by different randomized allocation patients to groups. no systematic linguistic patterns in groups composed by chance was found.

In order to find typical patterns within each of the patient groups a SIMCA analysis (Soft Independent Modelling of Class Analogy - I am sure I will return having better understood this sophisticated modeling worlds) was performed. These classmodels should serve as frames of reference for new patients which was evaluated with in the fourth study. According to the result of the validation study nine out of ten new patients were found in the expected personality area of the diagnostic space. On a more detailed inspection "the PPO class model had a good enough validity in terms of sensitivity and specificity - two patients; the sensitivity of the NPO model was also good - three patients. The BPO model though was not valid according to the criteria for satisfactory sensitivity and specificity - five patients" (IV,p.1).
As far as I can follow the statistical excursions - and I am not a statistician at all - I can follow the author´s discussion that the test set was too small. "The variables which the models were built on were too tied to the first thirty patients" (IV,p.14). For the non-expert I would want to know **how large the samples should be to exhaust the potential variability, do you have a guess or how many would you wish to include** (question). This is connected with a simple mind´s question: **why do we want to have a large sample - five to ten times the numbers of variables - in a conservative factor analysis and principal component analysis is satisfied with equal numbers of patients and variables ? Is PCA a kind of ipsative measurement of any given sample and does it not aim to be representative for the total population**

As discussed by the author the question arises whether the concept of BPO is too broad a concept including patients with a variety of clinical diagnoses. I would like to ask the author: **what are the alternatives to BPO theorizing within the psychoanalytical scientific community ?**

The fifth and final paper of the dissertation takes up the very important issue of temporal stability. Ten patients were investigated after five years performing the same procedures. The verbal changes showed that most of the patients had departed from their original position in the word space. One patient had even changed personality organization in terms of changed word pattern. Set aside the small sample size the clinical discussion provided show that this is an intriguing approach which brings us nearer to the heart of the matter. The author herself brings out the crucial questions and I would like to quote her as she does a good service to the promissing approach to correlate linguistic fingerprints with mental state. She write: we do not really know whether the changes in word patterns are actually due to mental change, what would be the linguistic changes have been like had the time lapse been shorter, how much in the changes in word patterns is dependent on actual mental state and how much is incidental or due to interview situations. However some concurrent validity is provided by correlating the verbal changes to changes in the HSRS and the DMT; interestingly enough no correlation to the SCL 90. **Why should the symptomatic state be less articulated in the verbalization ?** this is one - this is my last question in this presentation of a very interesting piece of work.

Hans Strupp a true nestor of psychotherapy research being confronted with totally surprising negative results in his arduous Vanderbilt II study finds true consolation by saying: further research is needed. It is a compliment to the thesis that it ends with the same note.

Questions

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