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PURPOSE OF THE JOURNAL

1. To present theoretical and research papers in scientific graphology according to traditional academic standards.
2. To create a forum for helping graphology gain a wider academic and professional audience in America.
3. To interface with the international professional graphological community.

GENERAL INFORMATION

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THE STATUS AND FUTURE OF SCIENTIFIC GRAPHOLOGY

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ABSTRACT

Graphology has traditionally depended primarily upon abstract reasoning and individual case studies; thus scientific research has provided few statistically validated results. Continued controlled studies and progress in related fields may provide opportunities for gathering further statistical data. Such factual material is essential for the future development of the science.

In *The Structure of Scientific Revolutions,* Thomas S. Kuhn (1962), argues that the social sciences in general are still in what he would call a pre-paradigmatic stage of development (p. 15). Data are collected and systems are proposed to explain the data, but no over-arching theory or paradigm emerges. For any such theory to predominate, it must be demonstrably successful in solving problems and making predictions. It must encompass a broad range of data, and yet it must be precise enough to be tested by experiment.

By this standard, graphology has not yet developed a successful paradigm. We depend on models which we believe describe how the personality works and is reflected in handwriting, but the models are largely untested. This means the most exciting challenges in our field are still before us; also the greatest demand for scientific rigor. It seems appropriate, therefore, to review in general how science develops and in particular the progress of graphology to date.

Every science, in the beginning, is dominated by logic and empiricism. Theories are based on abstract reasoning and specific observations, but there is no way to judge which theories are good and which are bad except by more reasoning and more observation. According to Richard Feyman (1985), the scientific method evolved as a means for weeding out bad theories (p. 308). A good theory, in other words, has to make predictions that come true.

As a science develops, the good theories are gradually consolidated, and a paradigm, a successful overall model of what's going on in the real world, emerges. That's when progress really begins, because, as Kuhn points out, "One of the things a community acquires with a paradigm is a criterion for choosing problems that... can be assumed to have solutions" (p. 37.)

A successful graphological paradigm will undoubtedly grow out of the concept formulated by Wilhelm Preyer in 1895 that handwriting is brain writing. It will have to take into account the universal neurological wiring patterns of the brain, the

unique, genetically determined variations for a given individual and the myriad influences of environment. And it will have to make predictions that come true.

We have some reason to believe that such a complex paradigm is possible, but our ability to make valid predictions is still a long way from a comprehensive analysis of the structure and functioning of personality. Graphological theories of personality still depend to a great extent on logic and empiricism. It may be useful, therefore, to examine these more closely. Each has its uses, and each has its limitations.

Logic is the Aristotelian mode of reasoning. Aristotle (384-322 BC), was once ranked a great scientist, or "natural philosopher," though in modern times he is judged to have had more success in his study of poetry and ethics than of physics. This is because, as Stephen W. Hawking (1988), puts it, "The Aristotelian tradition... held that one could work out all the laws that govern the universe by pure thought: it was not necessary to check by observation" (p. 15). Aristotle believed that non-empirical argument was intrinsically superior to argument from evidence; if an idea was beautiful, coherent and satisfying, it could be assumed to be true and needed no testing.

Only in mathematics can the Aristotelian ideal of pure reasoning be realized, which is why it has traditionally been regarded as the queen of sciences. But it might be more accurate not to think of mathematics as a science at all, at least in the modern sense. It was Roger Bacon (1214-1294), who first defended a preference for experimentation and empirical observation as superior to reasoned explanations. William of Ockham (circa 1300-1349), added the concept of "Ockham's razor," the assertion that the simplest explanation of a phenomenon, the one with fewest assumptions, is the one to be preferred. Francis Bacon (1561-1626), reiterated that inductive is preferable to deductive reasoning, and Galileo Galilei (1564-1642), followed with what is considered to be the first substantial body of experiment and observation in physics.

While there is still room in every scientific specialty for speculation, and while scientific theories will always be valued for their beauty, any field of study other than math which depends on logic without experiment is hard pressed, nowadays, to call itself a science.*

*Editor's note: Mathematics, it can be argued, is an experimental science in that theories are tested according to precise mathematical laws, but they are mental experiments rather than physical ones. Further, there are a number of instances in mathematics whereby abstract theories have found their place in experimental science, e.g., imaginary numbers, such as the square root of negative one, which were conceived of in the 1840's, were eventually used by Einstein's mathematics teacher, Hermann Minkowsky, as a short-cut method to explain the symmetrical relationship between the three dimensions of space and the unitary dimension of time. This development aided Albert Einstein and also Paul Dirac in formulating their various descriptive theories about the nature of space/time and the structure of sub-atomic particles. See *Consciousness and Reality* by Charles Muses and Arthur Young, Avon Books, New York, NY, 1972, and *Thirty Years That Shook Physics* by George Gamow, Doubleday, Garden City, NY, 1966.

If reasoning is not science, still science cannot exist without reasoning, and graphology must not forget or undervalue the work of its great reasoners. In the history of graphology, there have been many brilliant and intuitive Aristotelians, and we still rely on many of their insights; their perceptions still feel beautiful and right.

Thus we accept the idea put forward in Max Pulver's *Symbolik der Handschrift* (1940), that there can be symbolic significance in the three zones of a writing and in rightward vs leftward movements. Since predictions based on this symbolic reasoning have not been successfully tested, we would be unjustified in claiming a scientific basis for it; nonetheless, in individual case studies such reasoning can be helpful. Concepts like rhythm, fluency, and Ludwig Klages' form level or "formniwo," described in *Der Geist als Widersacher der Seele* (1929), are likewise meaningful for the practicing graphologist, even though it isn't easy to test the predictive value of qualities so undefinable. Of course, from the time of Abbe Jean-Hippolyte Michon, in Paris in the 1830's, graphologists have supplemented logical reasoning with empirical data, and hundreds of hypotheses have been derived from individual case studies. For instance, in early graphology, if a long, strong t-bar stood out vividly in the handwriting of a man known for his ambition, the conclusion was drawn that long, heavy t-crossings probably indicate ambition.

Such hypotheses are, indeed, testable and have occasionally been tested. We are all too familiar with the debunking efforts of Hull and Montgomery in 1919. In America, the scientific reputation of graphology is still, unfortunately, based on studies of the over-simplifications and naive literalism of poorly trained graphologists.

Despite its difficulties, empiricism, like Aristotelian logic, is essential to science. A mathematician may invent an entire system out of nebulous assumptions and never worry that there is no correspondence with any physical reality, but any other scientist is obliged to start with something empirical, — a real event or phenomenon, an anecdote, a fact, — and conceive a theory by generalizing from that particular instance.

For this reason, although we no longer believe in a graphology of fixed signs, we must still collect anecdotal evidence, — that is to say, the evidence of individual case studies, — and use it to help us formulate hypotheses.

The next and necessary step is to put our theories to the test. Though we may not say, as Karl Popper does in *The Logic of Scientific Discovery* (1959), "...there is no such thing as induction" (p. 40), induction from empirical data is not sufficient, any more than is deduction by pure reasoning. After logic and empiricism, come validation and confirmation.

Validation is not the same as proof. A good theory is one that withstands many and various tests, but strictly speaking, no amount of validation will ever absolutely provea theory; one piece of negative evidence may someday arise to cancel the positives out. For example, the theory of inert gases predicted that gases whose electron shells were filled would not form compounds; when, in 1962, Neil Bartlett

combined platinum hexaflouride and xenon and produced xenon flouride, the inert gas theory had to be discarded.

Since we can never conclusively prove any theory, scientific method is founded on the principle that every hypothesis must be susceptible of disproof. That's why, as Joseph Rychlak (1977) says, "The scientist always restates his hypothesis into the null form" (p. 181). The null form specifies which evidence would refute the theory. Popper (1959) explains, "It must be possible for an empirical scientific system to be refuted by experience" (p. 40). If a theory cannot be tested, that is to say if no test can be devised which it could conceivably fail, it is not science.

In the hard sciences, — physics, chemistry, biology — validating a theory involves predicting the outcome of an experiment. I predict that if I do X, Y will happen; I do X; if Y does not happen, my theory is not true. If Y does happen, of course, that's nice; it tends to validate my theory. But it isn't proof.

In the softer sciences, — psychology, sociology, economics....and graphology, — it's very difficult to devise experiments in which one can arbitrarily change variables [do X], and measure the results. These sciences deal with complex and subtle subject matter and with materials not susceptible to manipulation, whether for ethical or for practical reasons.

Often, therefore, the best that's possible is to observe, make predictions, and follow observation and prediction with rigorous statistical evaluation. Thus, in graphology, I predict that if I see X in a handwriting, I will find Y in the writer's case history; I look for X in many writings; I check for Y in the case histories; if there is no statistically significant correlation, my theory is not true.

In looking for validation of a hypothesis, it isn't sufficient to cite multiple examples in which a generalization seems to hold, because even with the best intentions a researcher is likely to take notice of instances that support his beliefs and ignore those that contradict them.

In fact, there are any number of ways in which one may unconsciously fudge the facts in research; that's why every study should also be subjected to independent confirmation. Unless a result has been duplicated, it should probably not be accepted.

Our best evidence to date of the validity of graphological concepts is that individual graphologists who profess to be relying on them do make valid predictions. It has been demonstrated that a skilled graphologist can make assessments which correlate extremely well with the results of other methods of evaluation: IQ tests, sales success rates, etc. For example, Alfred Binet, originator of the IQ test, conducted the first study correlating evaluations of intelligence based on handwriting with IQ scores. In *Les révélations dé critures dápre´s un controle scientifique* (1906), Binet reported graphological assessments to be highly accurate, — in the neighborhood of 80% for some graphologists, such as colleague J. Crepieux-Jamin. In 1962, Dr. Ulrich Sonnemann of the University of Basel working in cooperation with John Kernan, then chief of personnel at IBM, was equally successful in rating marketing talent. More recently, a 1979 study of my own work

demonstrated a similar level of accuracy in job screening as compared with the 40% success rate achieved using a panoply of other screening methods.

But not every such test of skill has given equally good results. For instance, Dr. Gordon Allport, Harvard psychologist in the 1930's and former president of the American Psychological Association, had to concede that matching experiments conducted by Edwin Powers at Dartmouth achieved results that were "not greatly above chance." Though he felt this poor showing was mitigated by "prevailingly `good errors' and occasional brilliant successes," he had to concede that "higher validity [remained] unestablished" (p. 7).

Of course, even if we were uniformly successful, this would not prove particular graphological premises correct. Our predictive assessments might be accurate even if we were wrong in explaining how we arrived at them.

This is not to say that nothing of value has been achieved. Essential preliminary research has established, for instance, that handwriting is unique and consistent for a given individual. As Werner Wolff reports in *Diagrams of the Unconscious* (1948), "...graphic movements... have an individual pattern which allows the layman to recognize the individual basis in different appearances" (pp. 179-180). Allport and Vernon, in *Studies in Expressive Movement* (1967), add that "...research studies show handwriting to be a constant over time and stable to a high degree" (p. 246).

Further, some qualities in handwritings may be consistent with other expressive gestures. Allport and Vernon, again, state that "The agreement of graphic movement with gait, gesture, and speech [has been] studied...with results which tend to support the theory that motor activities within personalities are related" (p. 211).

Progress has been made, too, in defining graphological terms reliably. Oskar Lockowandt (1976), in "Present Status of the Investigation of Handwriting Psychology as a Diagnostic Method," sums it up as follows: "The reliability of handwriting on the characteristics level can be considered as proven for the simple measured and ranked characteristics.... Results vary in the more complex characteristics" (p. 27).

In other words, graphologists often agree on the handwriting characteristics they see, especially those which are quantifiable. We should also be able to improve the reliability of complex handwriting characteristics by breaking them down into simpler factors much the way Robert Saudek analyzed various clues for determining speed in a writing in *Experiments with Handwriting* in 1928.

Despite our various successes, however, Lockowandt's survey of content validity research shows a generally low level of correlation coefficients. This may indicate that the successful predictions achieved by Cre'pieux-Jamin, Sonnemann and others may be based on a general correlation between quality of handwriting and overall achievement potential, rather than on any specific correspondences between handwriting characteristics and unique individual behavior patterns or personality.

On the other hand it may simply reveal, as Lockowandt himself says, "a lack of semantic congruence between the evaluations of the writing and psychological criteria" (p. 25). If this is the case, then one would expect more success in studies involving very clear-cut psychological criteria. It should not be surprising, therefore, that the most promising studies seem to deal with extreme aberrations in behavior or personality. For instance, Anita Muhl, in the early 1920's, was apparently able to isolate factors and syndromes predictive of delinquency in the handwritings of adolescents.

In 1965, Maria Paul-Mengelberg found disturbances, possibly attributable to brain injury as well as emotional stress, in handwritings of people who had undergone the severe trauma of POW camps, labor camps and concentration camps. In 1968, C.J. Frederick showed that the handwriting of suicides is identifiable by skilled graphologists. Also, Patricia Siegel and Marc Seifer (1988), have lately had some success with intercoded reliability on independent matching of psychogram scores with the writings of epileptics who had undergone split-brain surgery. In addition, Thea Stein Lewinson (1987), has recently done interesting work on the writings of alcoholics which is described in her article in this issue.

Major changes in psychiatry will undoubtedly provide new opportunities for basic research in graphology. In the most recent *Diagnostic and Statistical Manual of Mental Disorders,* a reference work of clinical diagnosis put out by the American Psychiatric Association, there is a new approach to the issue of personality assessment. The changed diagnostic schema depends on observable behavioral measures not on intrapsychic definitions too often rooted only in accepted dogma. In other words, a diagnosis must now be based on a person's actual symptoms not on an assumption of unconscious conflicts which cannot be directly observed.

One reason diagnostic classifications have changed is that many psychological illnesses are now curable or controllable by drug or electro-convulsive therapy. This means that, allowing for the possibility of coincidental remission or placebo effect, successful treatment tends to validate the accuracy of a diagnosis.

In addition to improvements in treatment and changes in diagnostic formulation, advances in brain research may shortly make even the current enormous gains seem elementary. From the point of view of graphology, all of this may provide a basis for us, with the cooperation of our psychiatric colleagues, to develop a reliable diagnostic methodology, based on handwriting analysis, which might be of value in the practice of psychiatry.

Graphology is more than ready to take advantage of the revolutionary changes going on in psychiatry and neurology. If anything, it has been, until now, a psychometric method of remarkable potential without a valid psychology to measure.

It is for this reason that we must now pause and take stock, to be certain we are prepared for what lies ahead.

In doing individual analyses, we've become accustomed to exploring uncharted and perhaps unchartable waters. Because the human psyche is so full of

contradictions, we avoid predicting behavior and prefer to describe tendencies and impulses which may be latent or unconscious. Such analyses may be right, but they appear to be untestable.

For research purposes, since there is no way that I know of to find and measure an unconscious impulse, we must look for correlations between handwriting factors and actual behavior. The studies cited above are a beginning. Unfortunately, these studies have not yet been expanded or repeated. Indeed, it is sometimes difficult even to find the original work in publication, and many graphologists are unaware of what has been done in our own field.

Three things are needed to correct this situation. The first is an outlet for publication. Though we must strive for a wide audience by publishing in professional journals not devoted exclusively to graphology, we must also collect our work in one superior publication of our own. This will make our accumulating knowledge readily available, and it will give us the opportunity to set proper standards for recognition. These are the aims of this *Journal of the American Society of Professional Graphologists.* In addition to providing for publication, we must seek funding for research. It's possible we will have to produce a more substantial body of work before we can hope for support from established funding sources. If so, that's a challenge we simply must meet.

Finally, we need to develop a concensus about what sort of research needs to be done. It is my contention that our research goal must be to isolate reliably defined factors in handwriting which, separately or combined in syndromes, are predictive of objectively identifiable behavior patterns.

As we struggle to define an over-arching theory of how the personality works and is expressed in handwriting, it is reassuring to know that such a paradigm need not be perfect. As Thomas Kuhn says, "...no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect. On the contrary, it is just the incompleteness and imperfection of the existing data-theory fit that, at any time, define many of the puzzles that characterize normal science. If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (p. 146).

To create a useful paradigm, we have to build on solid statistical results. We need not abandon intuitive Aristotelian formulations, neither need we give up conjectures evidence derived from individual case studies, but we must formulate hypotheses in a manner susceptible of disproof, and then we must test them and re-test them and when the facts contradict them, we must rethink.

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